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\*Indicates articles accepted under Editors Margo Mastropieri and Thomas Scruggs.

## Meta-Analytic Validation of the Dunn and Dunn Model of Learning-Style Preferences: A Critique of What Was Dunn

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A critique of a quantitative research synthesis (meta-analysis) investigating the Dunn and Dunn Model of Learning-Style Preferences is offered. Dunn, Griggs, Olson, Beasley, and Gorman (1995) synthesized the findings from 36 studies and reported an effect size of 0.755. They concluded that the Dunn and Dunn model was validated. We, however, disagree and raise questions about the following areas: the validity of the model, the nature of the literature base, the interpretation of the meta-analytic findings, and the conclusions drawn. Based on our critique, we conclude that the Dunn and Dunn model has not been validated, and further research is necessary before the learning-style approach can be accepted.

Theoretically, the concept makes perfect sense. Because individuals differ with respect to how they learn, benefits might accrue if these unique ways of processing information were identified and then matched to a particular teaching method. The difficulty, however, is that the concept has not received unequivocal empirical support (e.g., Berliner & Cahen, 1973; Cronbach & Snow, 1977; Miller, 1981). The reasons are found in the complexities surrounding the aptitude-treatment interaction (ATI) concept that results from problems in identifying and classifying different aptitudes and in differentiating treatments to match aptitudes (e.g., Bracht, 1970; Salomon, 1972; Tobias, 1976).

The ATI concept is even more irresistible in special education because special students, by definition, have special needs, and assessing those differences and planning appropriately matched instruction becomes critical for successful programming (e.g., Barbe & Milone, 1981). The appeal of ATI in special education was demonstrated by Arter and Jenkins (1977), who found that 99% of teachers believed that modality strengths and weaknesses should be major factors in designing programs and that 93% believed that students learned more when instruction was modified to match modality strengths. Similar to general education, however, ATI in special education also has not received empirical support for some time (see Kampwirth & Bates, 1980; Tarver & Dawson, 1978; Ysseldyke, 1973) and suffers from the same difficulties in crystallizing the concept (e.g., Larivee, 1981; Lloyd, 1984; Ysseldyke & Salvia, 1980).

In an effort to clarify and to combine research findings, Kavale and Forness (1987), using the methods of meta-analysis,

found limited empirical support for modality-based instruction. The common metric, effect size (ES), revealed modest outcomes across 205 individual ES measurements. The average ES of 0.144 translated into a 6 percentile rank gain on an outcome measure; this means that only 56% of students receiving modality-matched instruction demonstrated any gain, a level only slightly above chance (50%). Conversely, 44% of students receiving modality instruction did not reveal any gain. By conventional standards (see Cohen, 1988), the obtained ES (0.144) would be termed "low"; it is in accord with the findings for perceptual-motor training (ES = 0.08), a similar process-oriented intervention (see Kavale & Mattson, 1983). It was concluded that the modality model should not be endorsed because learning is best viewed as a matter of substance over style.

In a critical response, Dunn (1990) objected to the way modality preferences were described in the Kavale and Forness (1987) meta-analysis. Objections were also raised about the failure to (a) recognize demographic and achievement-level differences among groups, (b) acknowledge the multiplicity of preferences, (c) define terms precisely, and (d) identify the influence of poor instrumentation. In addition, Dunn challenged the way ES measurements were interpreted and decried the failure to recognize achievement gains in the special education groups. Kavale and Forness (1990) responded to Dunn's animadversions and reaffirmed the conclusion that modality-matched instruction is not effective in enhancing academic performance.

### A NEW META-ANALYSIS

In Dunn's (1990) critique of the Kavale and Forness (1987) meta-analysis was the suggestion that "Kavale and Forness'

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selection criteria for inclusion of studies in the meta-analysis seem capricious at best" (p. 352). Why was this the case? Basically, the literature selected was viewed as poorly done with respect to design and measurement and, more important, was not viewed as a comprehensive selection because of a failure to include more than a few of Dunn's research efforts. Although this criticism in no way adversely affected the negative evaluation of the modality concept, Dunn seems so committed to her own model that she conducted her own research synthesis (Dunn et al., 1995).

The Dunn et al. (1995) meta-analysis specifically evaluated the effectiveness of the Dunn and Dunn Model of Learning-Style Preferences, particularly the effects of congruent versus dissonant treatments on learning-style preferences. From 36 studies including 3,181 participants, an average ES of 0.755 was obtained. Data were then aggregated by learning-style stimuli (complete model, emotional, sociological, environmental, and physiological) and a number of study variables (e.g., strength of preference, academic level, and length of intervention). All findings were positive and led to the conclusion that "individualizing instruction to match learning-style preferences improved students' academic achievement and attitude toward learning" (p. 359).

### NEW META-ANALYSIS— NEW OBJECTIONS

Although Dunn et al. (1995) suggested that their "seminal" work should put to rest all prior objections questioning the benefits of learning-style models (e.g., Curry, 1990; Kavale & Forness, 1987; Snider, 1990, 1992), we are far less sanguine about the Dunn et al. findings and do not believe that they provided the support supposed. The purpose of this article is to outline the reasons we believe caution in accepting the optimistic picture about learning-style preferences offered by Dunn et al. is in order.

### THE MODEL

It is necessary to first place the Dunn and Dunn model in perspective. Throughout the Dunn et al. (1995) report, there is an assumption that if the Dunn and Dunn model finds empirical support, then the case is closed with respect to the efficacy of the learning-style model. Kavale and Forness (1987) apparently did not study the correct learning-style model because there is really only one—Dunn and Dunn.

The ATI concept possesses a long history, and even a cursory review of the literature reveals that it is enormously complex (e.g., Cronbach & Snow, 1977). Lloyd (1984), for example, described a number of unresolved issues surrounding ATI and how they relate to different interpretive models for implementing the concept. The ATI concept reveals a Byzantine complexity that is not captured in any single model (e.g., Dunn and Dunn).

Before any empirical evaluation can be deemed valid, there must be a conceptual validation of the model being investigated. The theoretical foundations of the model must be described and a rationale provided as to how empirical findings will relate conceptually. In addition, the development of

the model should be described and justified in terms of what is known and not known about ATI. This conceptual validation is lacking in the Dunn et al. (1995) report, and it is not clear whether the model is based on a sound scientific rationale or on creative musing. For example, the Dunn and Dunn model included such variables as "temperature" and "chronobiological energy patterns." Are these appropriate concepts, and can they be justified on the basis of what is known about learning styles?

### STUDIES AND QUALITY

Meta-analysis, as a method of research review, attempts to provide a comprehensive evaluation of the domain in question. The goal is to capture all the available research literature in a domain; traditionally, this means both published and unpublished literature. The Dunn et al. (1995) meta-analysis seems to have a dearth of published literature because 35 of the 36 studies included were dissertations. When 97% of included studies are dissertations, can we assume that a comprehensive literature search was achieved? Dunn et al., in their description of study selection procedures, seem to have actively sought only dissertations. Why else would the literature search be focused on sources (e.g., *Dissertation Abstracts International*) in which dissertations are sure to dominate? Fundamental to the validity of a meta-analysis is the completion of a comprehensive literature search that accesses all sources for identifying published and unpublished literature. The failure to access such sources as *Educational Resources Information Center*, *Psychological Abstracts*, *Current Index to Journals in Education*, or *Resources in Education* indicates the lack of a comprehensive search on the part of Dunn et al. and the possibility of a limited and potentially biased sample of studies. Additional evidence of a restricted search is the fact that half of the archival sources accessed were *Research on the Dunn and Dunn Model* and *Annotated Bibliography of Research*, both developed at the St. John's University Center for the Study of Learning and Teaching Styles, the home base of the Dunn and Dunn model.

The predominance of dissertations in the Dunn et al. database should not be misconstrued as representing a "better" source than that found in the published literature. Research published in professional journals holds the advantage of having gone through a peer review process before it reaches the professional community. Dissertations may not undergo this second level of scrutiny if not published, and, consequently, there may be reason to have less confidence in their findings. Smith (1980) discussed the question of publication bias, but ultimately it becomes an empirical question that cannot be answered conclusively unless there is a comparison of findings from published versus unpublished sources.

There is always the possibility that no published literature was included because there is no published literature, but then a question arises as to why this is the case. One possibility is that there was little desire on the part of authors to craft journal pieces. A second, more distressing, possibility is that the dissertation research could not withstand the scrutiny offered by the peer review process associated with professional journals. It seems incumbent on Dunn et al. (1995) to explain the curious fact that dissertations dominated the literature base of their meta-analysis.

The restricted nature of the literature search may produce unreliable findings. For example, concern has been expressed that individual dissertations might be biased because of being conducted under direction of persons who developed the model (see Curry, 1990). Although Dunn et al. (1995) denied the credibility of this assumption, they offered no proof. This denial rings hollow, however, when it is realized that 21 (58%) of the 36 studies included were completed at St. John's University, where Dunn heads the Center for the Study of Learning and Teaching Styles. Some tangible proof that no bias exists is absolutely necessary under such circumstances.

The need becomes more apparent when the way St. John's University dissertations were distributed among findings is examined. Dunn et al. (1995) aggregated data by learning-style stimuli described in the model and, when the emotional and sociological stimuli aggregations were inspected, it was found that the databases are represented only by St. John's University dissertations (100%). In addition, 50% of the studies included in the environmental and physiological stimuli aggregations were St. John's University dissertations. With such a heavy influence of dissertations from your "home court," the potential for bias is obviously present. Dunn et al. cannot cavalierly dismiss this possibility without evidence that it does not exist.

Dunn et al. (1995) asserted that they gave particular attention to research quality, but, in reality, it was not as comprehensive as believed. From a pool of 42 studies, 6 were eliminated on the basis of Campbell and Stanley (1966) criteria, a common practice in meta-analysis. A final decision about research quality in meta-analysis is ultimately an empirical question and cannot be achieved with only a priori judgments. Most meta-analyses rate studies with respect to research quality (e.g., high, medium, and low) and then examine (a) whether there is any relation between ES and research quality and (b) whether there are any ES differences among high-, medium-, and low-rated aggregations. A procedure of this sort provides the empirical evidence necessary to determine the influence of research quality on outcomes.

An example of how research quality may influence outcomes was provided by Snider (1992) in an analysis of learning styles and learning to read. In demonstrating how difficult it is to evaluate results, Snider posed a number of questions, particularly about the study by LaShell (1986) that was included in the Dunn et al. (1995) meta-analysis. The questions included

- What was the teacher-to-pupil ratio in each of these studies and was it the same for both the experimental and the control conditions? LaShell (1986) reported a teacher-to-pupil ratio of 12:1 for both groups. Given the highly individualized nature of reading styles curricula, might not teacher-to-pupil ratios influence effectiveness?
- Exactly what kind of instruction did the control group receive? LaShell (1986) reported that the control group received a traditional special education curriculum that consisted of a structured phonics approach. This could mean anything from workbooks to direct instruction. Instruction in special education classrooms varies greatly.
- To what extent are subjects' reading problems inextricably linked to the lack of motivation that results from repeated failure? For example, LaShell (1986) reported that half of the

participants in her study had previously flunked a grade. Low motivation certainly compounds reading disabilities. The novelty and individual attention given to the experimental group are likely to have a positive effect on achievement, regardless of method (p. 14).

These are not trivial points; it is incumbent on Dunn et al. (1995) to explain how this study passed muster and was included in the database. A similar scenario about each included study might be possible, and it becomes obvious that research quality is far more than passing a priori minimal standards.

In summary, although Dunn et al. (1995) addressed the issue of research quality, the discussion was far from complete and provided little confidence that the individual findings were of sufficient quality to supply definitive answers about the effectiveness of the learning-style model.

## ANALYSIS AND INTERPRETATION

Although we have some concerns about the actual methods of analysis, they are probably too esoteric to be of value for this discussion. With respect to interpretation, however, we believe that Dunn et al. (1995) provided a narrow focus that did not elucidate the whole picture. An ES of 0.755 was reported, and, using Cohen's (1992) guidelines, it was suggested that the magnitude of such an effect could be considered medium to large. We believe that such an interpretation is limited, and we would like to place the Dunn et al. (1995) findings in a larger context.

## VARIABILITY

One of the first limitations noted by us in the Dunn et al. (1995) findings was the failure to report the variability associated with the respective mean values. The mean as a measure of central tendency tells us about the "center" of a distribution of scores but does not describe the extent to which the individual scores surrounding the average differ. Most meta-analyses report an associated standard deviation, a measure of dispersion around the mean, that provides insight into the range of individual scores contributing to the mean value. For example, distributions may possess equal values for measures of central tendency but possess quite different shapes because of more or less associated variability. When variability is small, scores tend to cluster, and greater confidence is possible about the stability of mean value.

Without an associated measure of variability, a mean value cannot be interpreted fully. For many educational interventions, findings demonstrate that they often reveal more variability than effectiveness; the standard deviation is larger than the ES (see Kavale, 1990). If the two statistics are used to represent a form of expectation ( $ES \pm SD$ ), then theoretically the effectiveness of a particular educational intervention can vary from negative to zero to positive over a wide range. For example, take the case of psycholinguistic training (see Kavale, 1981), at one time a major process approach to intervention. A modest average ES of 0.39 was found, but with an associated standard deviation of 0.54; theoretically,

we can have a very effective intervention (0.93), a very ineffective intervention (-0.15), or something in between. The reader should be afforded the opportunity to place the mean value in a context provided by a measure of variability. Dunn et al. (1995) have not afforded us this opportunity, and hence, it is not at all clear that we can accept their interpretation of the mean value.

Some insight into the nature of the associated variability can be gleaned from the information provided by Dunn et al. (1995). Although influenced by sample size, the range ( $X_{\max} - X_{\min}$ ) is a useful descriptive statistic that can complement other variance estimates. The range and mean values associated with each reported facet of the Dunn and Dunn model are shown in Table 1.

On average, across the five learning-style stimuli aggregations, the range exceeded 5.5 standard deviation units. Although the overall effect was found to be a standard deviation of 0.75, it is evident that the findings are marked by considerable variability. In addition, in each case, the obtained mean value was closer to the minimal value, suggesting a skewed distribution. In summary, the apparent variability and skewness associated with the findings suggests caution in interpreting findings, and until more information is forthcoming, it is premature to interpret the mean ES value as medium to large or anything else.

### OUTLIERS

A difficulty introduced by the large-range statistics is the possibility that the extreme values represent *outliers*, which are potentially nothing more than spurious findings. Glass, McGaw, and Smith (1981) discussed the influence of outliers, methods for detecting them, and means of handling them; in general, one should be suspicious of outliers. They may represent oddities (measurement errors, miscalculations, misprints, and so on) that ought to be eliminated in describing the typical features of the data, especially the mean value. The Dunn et al. (1995) meta-analysis seemed to include some potential outliers. For example, one study in the physiological stimulus aggregation produced an ES of 22.294, and, when usual interpretation of ES is considered, it is difficult to provide a context of meaning for such a value. When interpreted as a standard score and accompanying area of the normal curve, any ES above 2.17 includes the 99th percentile. For example, an ES of 3.70 covers 99.99% of the area in a normal distribution. Within this context, what kind of meaningful interpretation can be provided for an ES six times as

TABLE 1  
Range of Effect Size (ES) for Learning-Style Stimuli  
From the Dunn and Dunn Model

Learning-Style Stimuli	Mean ES	Mean ES		Range
		Smallest	Largest	
Complete model	.458	.122	2.594	2.472
Emotional	.536	.060	1.370	1.310
Sociological	.481	.066	1.186	1.120
Environmental	.492	.074	1.370	1.296
Physiological	1.039	.068	22.294	22.226

large? In many respects, an unusually large ES loses meaning, and it is best to approach it with caution. The Dunn et al. meta-analysis includes seven potential outliers, and it seems necessary to determine if they are artifacts and if they had a negative influence on findings.

### ES INTERPRETATION

Judgments about ES magnitude require a context. The designation of an ES as high, medium, or low represents a simple rule of thumb and does not provide a comprehensive interpretation in which it is compared and contrasted with other findings. Forness and Kavale (1996) offered such a comparison in a mega-analysis of special education meta-analyses; in a rank ordering of 17 meta-analyses, 5 including modality instruction were deemed modest. In the modest category, other interventions included perceptual-motor and social skills training, which thus places learning styles in a context, suggesting difficulties in endorsing a popular intervention.

The Dunn et al. (1995) meta-analysis produced an ES (0.75) that was five times greater than the 0.15 ES found by Kavale and Forness (1987), and this was suggested as evidence supporting the validity of their model and the benefits of matching instruction to individual learning styles. We do not believe this to be the case, particularly when the Dunn et al. findings are placed in the context of instructional effectiveness. Walberg (1984) analyzed 50 factors that contributed to enhanced achievement. Eight factors revealed an ES greater than 0.75, including such factors as reinforcement, cues and feedback, IQ, acceleration, and graded homework; one common element is the straightforward nature of the effects that most enhanced learning. For example, reinforcement (ES = 1.17) is far easier to introduce and implement than the machinations required for assessing and matching instruction to learning styles (ES = 0.75). In addition, simply knowing a student's IQ (ES = 0.71) produces an effect on learning that almost equals those purposefully produced by the Dunn and Dunn model. It is also interesting to note that among the variables analyzed by Walberg, not one involved ATI. Thus, a majority of methods designed to enhance the quality of instruction seem, if not more effective, at least as effective as those claimed for the Dunn and Dunn model and hold the advantage of being far easier to implement in practice.

It is also possible to place learning styles in the context of special methods designed to enhance academic performance. Forness and Kavale (1996), in their mega-analysis, included a number of special interventions that produced a larger ES than the 0.75 found by Dunn et al. for learning styles. For example, Direct Instruction (see White, 1988), involving academically focused, teacher-directed learning with sequenced, structured materials and high levels of student involvement, produced an ES of 0.84. Behavior modification techniques that emphasized reinforcement of effort or accomplishment in the academic domain produced an ES of 1.57 (see Skiba & Casey, 1985). Efforts directed at enhancing reading comprehension (e.g., strategy training, vocabulary, and text enhancement) have shown comparable findings in two meta-analyses with obtained ESs of 1.13 and 0.98 (Mastropieri, Scruggs, Bakken, & Whedon, 1996; Talbott, Lloyd, & Tankersley, 1994). Finally, methods that employed mne-

monic strategies (e.g., keyword and pegword) and also required a form of matching produced an ES of 1.62 (see Mastropieri & Scruggs, 1989). Thus, a number of special methods that might be considered in the same realm as learning-style instruction produced larger effects and enhanced achievement to a far greater extent.

Why might this be the case? The methods described previously possess the common element of dealing with academic content. In each case, emphasis was on what was taught rather than on the more indirect influence resulting from the implementation of the learning-style model. Kavale and Forness (1987) summarized this by concluding that "learning appears to be really a matter of substance over style" (p. 238) and that the learning-style model "will serve only to deflect attention away from the primary requirement for learning—substance" (p. 360). The Dunn et al. (1995) findings with an ES of 0.75 do not seem to contravene this conclusion, especially when placed in the context of meta-analytic findings, demonstrating the effectiveness of procedures that focus on more substantive aspects of learning rather than on more ethereal aspects of learning (i.e., style).

The Dunn et al. (1995) report makes reference to an earlier meta-analysis (i.e., Sullivan, 1994) synthesizing studies on the Dunn and Dunn model conducted between 1980 and 1990 with the hope that "it would reveal that accommodating individuals' learning-style preferences results in increased academic achievement and that specific moderators may affect the results" (p. 355). We take this opportunity to inquire whether the Sullivan meta-analysis achieved its desired goal. What were the findings? Do they support the Dunn et al. conclusions? It seems curious that the Kavale and Forness (1987) meta-analytic findings were the only conclusions referenced when, in fact, the Sullivan meta-analysis, with a similar literature base, has the potential to offer the confirming evidence about the efficacy of the learning-style model. The Sullivan meta-analysis was a St. John's University dissertation, suggesting that Dunn et al. had ready access to it and no reason to give it such short shrift, with only a single reference and no discussion of its findings. It would indeed be useful to know the results of the Sullivan meta-analysis so that they may be compared and contrasted with the reports of both Dunn et al. and Kavale and Forness (1987) in an effort to bring greater closure to questions about the effectiveness of the learning-style model.

The Dunn et al. (1995) meta-analysis also included other findings that require context (and explanation) for proper interpretation. For example, no mention was made of the fact that the Dunn and Dunn model seems to be less effective at the secondary level. A modest ES of 0.314 was found at the secondary level, which means that just 62% of students provided learning-style matched instruction would be better off, a level closer to chance (50%) than the 78% found overall. Another dimension that requires explanation surrounds the content areas in which the learning-style model was presumed most effective. The largest effect (ES = 9.799) was reported for the academic category labeled "other," but Dunn et al. nevertheless concluded, "The content area most responsive to learning-style accommodation was mathematics, followed by other subjects and language arts" (p. 38). The mathematics ES was, for example, 1.013; although we may quarrel with some interpretations offered, 9.799 seems to be larger than

1.013. How could Dunn et al. conclude that mathematics was the area most responsive to learning-style accommodation?

Finally, there was joy in the fact that learning-style model interventions that were conducted for more than 1 year produced an ES of 1.345. Dunn et al. need to temper their glee, however, when it is realized that the expected ES for the average student in the average school receiving "average" instruction in an academic content area over 1 year is 1.00. Consequently, the real effect of the learning-style model is only about 0.345. The situation becomes worse with interventions that lasted for an unspecified period of months (ES = 0.561, reported by Dunn et al., 1995). Within the context of regular instruction, 6 months of instruction, for example, would produce an ES of 0.50 in an academic domain; learning-style instruction would thus hold only a 0.06 standard deviation advantage. Given the limited magnitude of the "real" effects, are the machinations required to implement the Dunn and Dunn model justified when plain, old, good instruction will produce essentially the same results?

## CONCLUSION

The Dunn et al. (1995) meta-analysis has all the hallmarks of a desperate attempt to rescue a failed model of learning style. The weak rationale, curious procedures, significant omissions, and circumscribed interpretation should all serve as cautions to the educational community before accepting the findings as truth when, in reality, they remain far removed from the truth.

The intuitive appeal of the ATI concept is almost irresistible, but this appeal must be tempered by the fact that it has not received the necessary empirical support. The number and complexity of interrelations among aptitudes, the problems associated with measuring aptitudes, and the difficulties in matching instructional methods to particular styles have been long-standing problems. From many quarters, these vexing problems have been acknowledged, not the least by demonstrations that the research evidence lacks rigor and independent replication. Yet, none of these concerns seem to have slowed the zealous quest of Dunn et al. (1995) to supply the empirical proof lacking for the learning-style approach.

In rational debate, it must be recognized that passion cannot substitute for the reality offered by empirical evidence. The Dunn et al. (1995) meta-analysis does not offer that reality and should not be regarded as the final word with respect to validation of the Dunn and Dunn model. We raised a number of questions about the Dunn et al. meta-analysis, and answers are necessary before the findings can be in any way accepted.

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