Commentaries

Following are two commentaries on "Development of a Quality-of-Movement Measure for Children with Cerebral Palsy."

Measuring and describing movement quality is an elusive concept. For many years, dancers, choreographers, physical educators, and physical therapists have attempted to capture the essential nature of movement. What is it that separates the pirouette of a prima ballerina from that of a devoted student or a Chopin sonata played by Vladimir Horowitz from that played by the local piano teacher? In either case, the position of the limbs may be the same, the functional result is attained, but one performer is considered a great artist and the other only a dedicated amateur.

Boyce et al have made a courageous effort in taking the first steps toward developing a taxonomy of movement quality. As their article will attest, they have put many hours into planning and constructing the Gross Motor Performance Measure. Inclusion of an interdisciplinary group of experts in the formulation of the instrument is commendable. I do not question the validity of the test instrument for measuring the criteria that they have identified. I question, rather, how they selected these criteria and whether the criteria are accurate indicators of movement quality.

Perhaps the most difficult part of measuring movement quality is in defining the attributes that comprise this movement component. These authors have selected alignment, coordination, dissociated movement, stability, and weight shift. No explanation is offered to justify the selection of these particular movement attributes as criteria of movement quality. The authors introduce some of the important theoretical questions about movement-quality attributes. Is quality a single feature of a movement, or is movement quality multidimensional? Will movement quality be measured by a motor task analysis that reduces movement to its temporal and spatial characteristics? Does movement quality contain the aesthetics and fluidity of movement, and can they be measured? Because the authors have chosen to avoid a theoretical framework, however, these questions are never answered in their article. Instead of operationally defining quality of movement and then selecting criteria on the basis of this definition, the authors adopted only those attributes that the clinicians could easily recognize.

The absence of a guiding theory weakens the usefulness of the instrument. Historically, motor behavior researchers have approached task analyses by studying a common skill, hoping to uncover laws and theory of movement. This approach, however, leads to "disconnected pockets of data that make it impossible to integrate in order to produce more generalizable principles." Perhaps Boyce and colleagues do not realize that, in avoiding the structure of a theoretical framework, they have actually limited the test instrument to examine only those components of movement that were familiar to the participants, without determining the validity of the components as measures of quality.

The authors provide no evidence to suggest that experimental controls were imposed that would protect against the biases of the participants. In making group consensus a primary goal for the assessment of the test instrument, the authors have increased the probability that only those criteria that clinicians were comfortable with or were experienced in observing would be included.
Although the authors acknowledge that movement quality includes effort, force, timing, and velocity, they choose to leave the realm of scientific measurement for the more amorphous dimension of gestalt perceptions rather than address the real problem of quantifying quality. It could be argued that any of the attributes selected, particularly postural alignment and weight shifting, are intrinsic components of gross motor function that need to be further reduced to analyze the performance attributes. I am particularly concerned about their decision to separately assess weight shifting, stability, and coordination, which are considered to be centrally programmed characteristics of each movement and therefore interdependent. Another concern is the static nature of the test instrument. Can these criteria be relied on to measure the fleeting and inconsistent motor performance of a child adapting his or her movement to multiple stimuli in the busy environment of a playground or classroom?

The authors' failure to operationally define quality of movement is a major flaw in this article, because it permits subjective decision making in the application of the assessment tool. Physical therapy is suffering from a dearth of quantifiable measures, and I was sorry to see that these authors relied on the vague clinical descriptors of mild, moderate, and severe impairment. How much stronger the assessment would have been had the group process meetings concentrated on setting clear limits for acceptance of the descriptors of each performance attribute.

I agree with the contention of these authors that a valid test instrument for measuring the subtle changes in quality of movement is almost essential if we are to demonstrate efficacy in many of our treatment interventions. The authors have presented many of the issues that need to be addressed when determining the measurable attributes of movement quality and are to be commended on their effort to approach a subject that so many have discarded as impossible. In so doing, they have given us a tool that can serve as the point of initiation for future studies of this important topic.

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References

In few other areas of clinical practice is the need for more quantitative measures of performance as pressing as in the treatment of the child who has brain injury. The authors' systematic effort to develop such a measure through their Gross Motor Performance Measure (GMPM) is commendable. The promise of a carefully developed scale that is sensitive to change in motor performance, objective, and clinically feasible to administer is encouraging. In this commentary, I will confine my discussion primarily to the first of these issues.

As the authors correctly note, efforts to evaluate the efficacy of therapeutic interventions in cerebral palsy have been hampered by a lack of suitable measures. What are needed are measures that are sensitive to subtle, but meaningful, change in performance, not simply the presence or absence of a particular function. The value of this manuscript is that the authors describe many of the essential steps for the development of an observational measurement scale that is potentially sensitive to subtle performance distinctions. Whether this scale actually achieves the stated goal cannot be assessed with the information presented. This article, however, suggests several shortcomings in the scale's development that lead me to believe it will fall somewhat short of its mark.

The authors may have developed the GMPM with a theoretical framework in mind, but this is not obvious from their presentation. Their discussion of conceptual issues gives quite the opposite impression. If the development of the GMPM lacked a theoretical framework, then I believe this to be a glaring shortcoming of this instrument. The authors attempt to draw a distinction between the writings of Michels' and Rose' on this issue, which is false. Michels advocates theory as a necessary preliminary if measurement is to be scientifically useful. The comments of Rose on classification relate to the development of treatment models, not to measurement instruments. The authors, apparently rejecting theory as a basis for the development of the GMPM, argue instead for the development of "generic measures that use scientifically accepted pathokinesiological and motor control terminology." Yet, such terminology can only be meaningful in the context of the theories of which they are constructs.

If the authors based the scale's item definitions on scientifically accepted terminology, then it would have been helpful to provide specific illustrations of how clinicians involved in the measure's development were instructed to utilize such terminology. In this article, only one motor function from the scale, and consequently example levels of only three attributes, are presented. It is difficult, therefore, to assess the extent to which the scale incorporates acceptable definitions of motor performance. Once again, what is considered acceptable depends on the theoretical framework one chooses.