Inappropriate Article for AJOT

We feel that the article published in the July 1984 AJOT [pp 462-468] entitled "A Review of Therapeutic Intervention Research on Gross and Fine Motor Progress in Young Children with Cerebral Palsy" and authored by Parette and Hourcade was not appropriate for publication in our major professional journal for the following reasons:

1. The AJOT is subsidized by occupational therapists, and we are unwilling to bear the cost of publishing articles, especially by individuals from other professions, that present unsubstantiated statements about the inefficacy of therapeutic interventions by occupational (and physical) therapists or unsubstantiated statements about the outcome of future research such as that beginning at the bottom of the first column of page 466:

   Thus, one may draw the conclusion that, as research investigating the effectiveness of therapeutic intervention becomes increasingly rigorous in terms of research design and statistical analysis, empirical documentation of program efficacy becomes less likely.

   4. The authors' inclusion of Ayres as a reference is inappropriate and reflects serious deficits in the theoretical framework that render the article unsuitable for publication in our journal. Dr. Ayres' work was not developed as an intervention strategy for young children with cerebral palsy, as the authors imply it was.

5. The system for classifying therapeutic approaches, borrowed from Marks, is inadequate and unacceptable to us. This classification reflects a poor clinical and theoretical background in occupational therapy.

6. A literature review of 18 articles covering the last 32 years might be helpful to therapists if it were annotated and examined for a historical perspective, but an attempt to validate or invalidate treatment approaches over such a long span when research in all fields lacked sophisticated methodology and analysis is of little benefit either to "knowledge" or to our discipline.

7. There is more recent literature (and with more sophisticated methodology) related to treatment of cerebral palsied children which the authors did not review. In conclusion, this article provides no new information and no new approach to examining professional literature, and it draws unwarranted conclusions. We feel that the publication of articles, especially articles by individuals in other disciplines, that inaccurately reflect the efficacy of occupational therapy is a disservice to us as individuals and to the profession.

Mary Paige Rose, LOTR
LuAnn Pearson, LOTR
Timothy C. Jones, LOT
Carla Saulsbery, LOTR
Kim Norton, LOTR
Terri Winans, COTA
Susan Brokaw, LOTR
Suzanne P. Lee, LOTR
Annette W. Simmons, LOTR
Shelia Davidson, LOTR
Louise W. Campbell, LOTR
Pam Carter, OT
Debra DeHart, LOT
Mary Ellen Terrell, LOTR
Mary K. Welter, LOTR
Angelica Lacer, LOTR
Anita B. Gilliam, LOTR
all Shreveport, LA

The Authors' Response

We would like to address the significant points raised by the letter writers:

1. This objection unfortunately appears to be analogous to a youngster proclaiming that all must play baseball by his rules, or he will take his ball and bat home. Such professional wall building and turf protection is especially inappropriate today in light of contemporary service delivery systems. We would go on to suggest that, in science, positions should be evaluated by the nature of their empirical support or lack
thereof (1, p 6), not by professional affiliation, and that open and multidisciplinary access to ideas in scholarly forums is a necessity for this process. The suggestion that program efficacy receives relatively less data-based support in more rigorous research studies is simply a finding based on the studies reviewed in the paper. In fact, we go on at some length to speculate why the effectiveness of intervention efforts (which we “know” exists) may have remained undocumented in many of the reviewed reports.

2. Given the relative lack of literature reviewing intervention effectiveness, we felt it appropriate to include all possible sources of information. In reviewing the article once again we see no source of confusion in the way this information has been integrated.

3. It is certainly true that more detailed information can be generated from analyzing individual merits and demerits of each study. However, space constraints prohibit this sort of analysis, although of course each is referenced and available for further investigation.

Moreover, such a comment misses a generic purpose of reviews of literature, which is to help the reader see the forest without the trees. At no point do we claim that we are doing field research; in fact, we consistently refer to the manuscript as a review, doing so in the title, the abstract, and consistently throughout the paper.

4. The specific statement that referenced Ayres was as follows: “A number of treatment systems that differ in underlying philosophy and specific intervention strategies have been advocated by professionals. . . .” That single Ayres reference was one of five cited to indicate the diversity of intervention approaches. Regardless of one’s acceptance or rejection of sensory integration, a more innocuous statement is difficult to imagine. Contrary to the letter writers’ assertion, one review of sensory integration began as follows: “Sensory integrative therapy as described by Ayres was originally intended as a treatment technique for children with cerebral palsy . . . .” (2).

5. We believe that the Marks classification system possesses utilitarian value and regret that the letter writers dismiss it so lightly that none of its alleged “serious deficits” were noted.

6. Nowhere do we attempt to “validate or invalidate treatment approaches.” Again, our paper simply summarizes reported results available in the literature and comments on the results of that summary.

7. Unfortunately, a review of literature begins to be dated the moment it is written. Our review used all sources that might identify reports relevant to the paper. Any possible omissions (we are aware of none, and none were identified by the letter writers) were unintentional.

As the conclusion of our paper repeatedly suggests, our purpose as professionals delivering intervention services should be to develop the technologies in research methodology, assessment, and intervention so that we may meet increasing demands for program accountability. We feel that professionals from a variety of disciplines may contribute in these efforts, and thus we would question, and are disturbed by, the themes of isolation and turf protection that permeate the letter from the readers. Such attitudes can only prevent all of us from our primary professional purpose: the provision of programs of the highest quality.

Howard P. Parette, Jr., EdD
Jack J. Hourcade, PhD
both Ruston, LA

REFERENCES

Wants Practical Information

I found “Shoulder Pain in Hemiplegia” by Lori Anderson in the January 1985 AJOT [pp 11–19] interesting and useful. As a recent graduate, I appreciate articles that present concepts in easy-to-understand language with information that I can incorporate into my treatment sessions. Anderson’s article is one of these.

I find most AJOT articles are too technical or present nonpractical information. Although research plays a vital role in our profession, clinical therapists are always looking for practical useful ideas/information to incorporate into their treatment.

I hope my colleagues will submit more practice-related articles that either describe unique pieces of adaptive equipment and treatment techniques, or that describe general principles and information, such as in the Anderson article. Those articles that improve treatment should be the number one goal of AJOT.

Max Queen, OTR
Augusta, GA