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Commentaries upon this paper follow below and the response to these commentaries appears on pp. 65–83.

PACT: some comments and considerations

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Introduction

In their article, Bowen and Cupples (this issue) introduce 'an empirically tested phonological therapy model' that is 'broad-based, flexible, and adaptable, comprising five dynamic and interacting components…'. The model is said to differ from previous models in (a) the extent to which it involves the children's families, (b) its scheduling protocol, with children being seen once weekly in 10 week blocks followed by 10 week periods of no treatment, and (c) the unique mix of five intervention components (none of which are original to the authors) into an innovative composite intervention plan. The approach utilizes less clinician time than most other approaches and, thus, is argued to be highly efficient. This position is supported by the results of an experiment (Bowen 1996), which demonstrated that children who received the intervention were rated less severe following intervention than a group of children withheld from the treatment over the same period.

For the past decade or so, I have held the position that the effectiveness of speech and language intervention depends on far more than the adequacy of the specific techniques or procedures designed to evoke target responses from our clients (see Fey 1986, 1992a,b). To appreciate fully the complexity of intervention and to maximize its effectiveness, we must consider a broad range of variables and evaluate the ways in which they contribute to intervention efficacy. This includes things such as

(1) what goals are selected and how they are determined,
(2) who does the intervention,
(3) where the intervention is performed,
(4) under what kind of conditions it is carried out (e.g. play or drill), and
(5) how specific intervention targets are attacked (e.g. one at a time to criterion, all in the same session, one at a time with no criterion in a cyclical fashion) (Fey 1992a,b; see Bowen and Cupples, figure 1).

Bowen and Cupples embrace this notion and amplify it in their interesting and important intervention model. Their model deserves careful consideration by all clinicians who are looking for more efficient methods of delivering speech services to young children with phonological disorders.

In general, then, I believe that Bowen and Cupples have constructed a useful approach that warrants the attention of clinicians and researchers alike. Some of the intervention components they use have been validated experimentally or quasi-experimentally. For example, there is considerable evidence that direct intervention, using minimal pairs, can facilitate development of trained and untrained sounds (Blache *et al.* 1981, Weiner 1981, Gierut *et al.* 1987, Tyler *et al.* 1987). Other studies have shown that parents can be trained to carry out speech intervention activities outside the clinic as an adjunct to clinician-administered treatment and that such participation contributes to intervention effectiveness (Sommers 1962, Costello and Bosler 1976). Some evidence has emerged, however, suggesting that efforts to train phonological skills to preschool and kindergarten children with language disorders (Warrick *et al.* 1993, van Kleeck *et al.* 1998) can facilitate phonological awareness skills and, perhaps, better early reading, as well. The evidence supporting speech intervention in blocks rather than on a continuous basis is mixed (see Weston and Harber 1975 for a review), but gains in phonology following planned breaks from phonological intervention have been reported in the literature (see case studies in Hodson and Paden 1983, 1990). Furthermore, the basic assumptions of Bowen and Cupples' approach are well supported from a theoretical perspective. In sum, the approach is rational and well motivated, and as implemented by Bowen (1996), it is highly efficient, with children receiving an average of only two sessions with the clinician per month. Based on these factors, I can see no reason why clinicians should feel uncomfortable in attempting the approach with preschool-aged children. Despite this generally positive view, I have a number of questions about the approach, whether or not it works, who it works for and who it doesn't work for, and in what ways it can be modified with confidence by clinicians.

Does the approach work?

There are many reasons to conclude that a clinical approach works and not all of them are based on experimental evidence. As I noted above, there is some support of some kind for each of the components found in Bowen and Cupples' model. This alone gives face validity to the approach, and this is better than what clinicians sometimes have when they develop intervention programmes for individuals with speech and language disorders.

Still, one must question whether the particular combination of features, in the particular 'doses' provided by Bowen and Cupples, had the treatment effect they claim it had. In fact, there are several characteristics of the study's design that make its results difficult to interpret. The most significant of these factors involves the constituency of the groups. Only 7·4% of the total pool were willing to serve as controls. Others selected as controls were unwilling to wait and sought treatment elsewhere. This is an understandable problem, but it is a very serious one, nevertheless. For example, it seems likely that the control group of children and families differed on clinically relevant variables from the children and the families in the treatment group. The parents of children in the control group may have been less nurturing, less generally supportive, less well educated, and/or less likely to do the things at home typically that might have a facilitative effect on their children's development (e.g. shared book reading, child-oriented conversation, play). These differences might have led to faster development among the treatment group children due to factors completely unrelated to intervention. Of course, this assumption could be all wrong, and the groups could have been exactly the same on all variables related to learning. Still, failure to assign the children to groups at random seriously weakens the conclusions one may draw from the results. In my view, it precludes the conclusion that this approach has been experimentally validated.

One might counter this viewpoint by citing the very impressive dismissal rates reported by Bowen and Cupples. Isn't this rate much better than we could expect for preschoolers with phonological disorders who were not receiving intervention? Perhaps, but to evaluate this possibility, we need to have a good idea of what the children were like at the outset of the study and what the criteria were for dismissal. When we have so little understanding of the children's problems at the onset of treatment (see below), and no operational definition of 'normal' is provided, it is difficult to interpret the significance of the dismissal rates reported.

For whom does the approach work?

Even if we conclude that the approach, as delivered by Bowen (1996), is effective, we must still question for whom it is likely to work. Clinicians can be confident of similar results only if their clients are from the same population as those who participated in Bowen's investigation. As noted above, however, Bowen and Cupples provide very little about the subjects in this study. Bowen and Cupples tell us that one child fell in the mild category, one fell in the severe category, and all others fit into the mild–moderate category in each group. We are given no basis for these categorizations, however. It is not clear then how many errors the children produced, what types of errors they made (e.g. error patterns, omissions versus distortions versus substitutions), how consistent these errors were in conversation and isolated words, and how much the errors affected intelligibility. In short, it simply is impossible to tell how impaired these children were, especially when some were younger than 3 years of age at the time treatment began.

In what ways can the PACT methods be modified?

Even if we conclude, as Bowen and Cupples do, that the intervention has been shown to be highly effective, it is impossible to tell which components of the approach are critical and which are unnecessary. This is not an inherent weakness of Bowen's research design, and it does not in itself weaken the conclusion that the treatment was effective. No single intervention study can hope to answer all of the questions that might be asked about the treatment tested. This feature of the design does, however, make it impossible to determine which parts of the approach are needed and which are superfluous. This poses a significant challenge to clinicians who can't or don't want to deliver the model as tested by Bowen (1996). For example, what would happen if treatment blocks are 5 or 15 weeks in duration instead of 10, or what if there are no breaks from treatment at all? Similarly, what if a parent is unwilling or unable to participate in the types of homework typically assigned to parents? Are the effects of the programme diminished or eliminated altogether? In future investigations of the treatment, it will be crucial to compare the approach as described with other versions in which certain aspects of the original have been systematically modified or omitted. Just as importantly, clinicians who make modifications to the intensity or duration of treatment or who alter the procedures and activities described by Bowen and Cupples need to report their positive and negative results for the benefit of other clinicians and researchers (see Fey and Johnson 1998).

Conclusion

In sum, I believe that Bowen and Cupples have done an admirable job describing a complex, multifaceted approach. There is much that I find intriguing about the model. For example, I am especially pleased by Bowen and Cupples' attention to the family as an important member of the management team. Such attention should lead to the identification of more appropriate goals and more consistent and frequent administration of intervention procedures at home. These factors, as well as the overarching concern with the family and child's role as a family member, should enhance the efficiency and effectiveness of the intervention. I also believe that Bowen and Cupples' attention to metalanguage at such an early point is well motivated and is likely to reduce the likelihood that these children will be deficient in phonological awareness and early reading skills (see Warrick *et al.* 1993, Fey *et al.* 1995). I find the alternating 10 week intervention and no treatment blocks to be one of the most interesting and time-saving components of the programme. Unfortunately, I also find it to be the component most seriously in need of experimental evaluation.

My critical comments in this article have focused largely on what the research cited by Bowen and Cupples can do to enhance our confidence in the effectiveness of the approach. Due to several methodological flaws that compromise the controls found in the design, I do not believe that it is appropriate to say that the model has been empirically validated; it would be clearly misleading to state that it has been *experimentally* validated. More and more tightly controlled studies are needed to make such a claim.

There is a great deal that remains to be learned about this approach. For example, what types of children and speech sound problems respond best? Which components may be modified or left out altogether? What if the intensity of clinician consults is increased or decreased? Questions such as these remain after *any* experimental evaluation of *any* speech or language intervention approach. They can be answered only with additional research and the reports of clinicians who attempt modified versions of the treatment and systematically monitor their methods and outcomes (Fey and Johnson 1998). Surely, this complex intervention model and its potential cost-effectiveness warrant the efforts required to produce these answers.

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