LSHSS

Letter to the Editor

Auditory Training for Children With Auditory Processing Disorder and Language Impairment: A Response to Bellis, Chermak, Weihing, and Musiek

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Purpose: We respond to Bellis, Chermak, Weihing, and Musiek's (2012) criticisms of the evidence-based systematic review of Fey et al. (2011) on the effects of auditory training on auditory, spoken, and written language performance of children with auditory processing disorder or language impairment. In general, we argue that the conceptualizations and methods on which our review was based were well motivated, and that our original conclusions are valid given the limited evidence that is currently available from clinical studies of auditory training with school-age children with auditory processing disorder or language impairment.

Key Words: central auditory processing, children, efficacy, intervention, language disorders, systematic review

We welcome the critical commentary of Bellis, Chermak, Weihing, and Musiek (2012) directed toward the systematic review we recently reported in *Language, Speech, and Hearing Services in Schools* (*LSHSS*) dealing with auditory processing disorder (APD; Fey et al., 2011), and we are pleased to have this opportunity to respond. The primary purpose of our review was to evaluate the state of the external evidence available in the published literature on the effects and efficacy of auditory interventions for school-age children with APD. Because some of the interventions that we regarded as examples of auditory training are often recommended for children with spoken language impairment, we also reviewed studies involving this population. In their response, Bellis et al. assert that methodological and conceptual limitations of the systematic review led to inappropriate and misleading conclusions regarding the state of the evidence for the use of auditory interventions. More specifically, they criticized (a) our criteria for including children with APD, which they believe led us to include studies of children who did not have APD and to exclude some important studies that document the outcomes of auditory interventions; (b) our definition of auditory interventions, which they claim led us to consider studies involving treatments that are not "truly" auditory and to ask irrelevant questions about children with primary diagnoses of spoken language impairment; and (c) our restriction to studies of children involved in interventions rather than considering studies of adults and animals and basic neuroscience. We will address each of these criticisms after a brief section explaining the factors that led to our review.

Factors Leading to Our Evidence-Based Systematic Review

On page 382, Bellis et al. (2012) mistakenly claim that the document that was produced by the 2005 American Speech-Language-Hearing Association (ASHA) Working Group on Auditory Processing Disorders (henceforth, the 2005 Working Group) addressed the role of the speech-language pathologist (SLP) in central auditory processing disorder (CAPD) diagnosis and intervention. What the report actually said was that "an in-depth discussion of the role of the SLP and other professionals was beyond the scope of this report (ASHA, 2005, p. 1)." In the prologue to our clinical forum, Richard (2011) provided the historical background that led to the formation of our Committee on the

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Role of the Speech-Language Pathologist in Identifying and Treating Children with Auditory Processing Disorders; discussed the 2005 document extensively; and devoted an entire section to controversies in APD that addressed terminologic, definitional, and diagnostic issues.

The first step in meeting the charge of our committee was to develop a position statement on the roles and responsibilities of speech-language pathologists (SLPs) in the assessment and treatment of children with APD. According to an ASHA survey (2004), 67% of school-based SLPs deal with children who have diagnoses of APD. Many SLPs are confused and frustrated about the myriad definitions of the disorder; varying criteria used to describe the clinical profile of children who are diagnosed with it; and significant overlap between children with APD and those with attention, language, and reading disorders. Practicing clinicians are looking for evidence-based guidance in serving children with APD. Our review was designed to evaluate the available treatment evidence in a manner that could provide clinicians with the guidance they need to treat children with APD. The decisions we made concerning criteria for the systematic review must be considered from this perspective.

Definition of APD for Participant Selection

We were surprised to read the Bellis et al. (2012) concern with our use of the term auditory processing disorder (APD) rather than *central* auditory processing disorder (CAPD) on the grounds that CAPD is "the most recent accepted nomenclature for the disorder" (p. 381). In the first place, the 2005 Working Group used and recommended the term, (C)APD, not CAPD. The 2005 Working Group further suggested that (C)APD and APD are synonymous. Accordingly, we adopted the name, APD. Second, one does not have to look far, even at very recent articles, to see that, despite what may have appeared to Bellis et al. as agreement in the field, there is no consensus on what to call this clinical entity. For example, in his independent response to our clinical forum, David Moore (2011) used "APD" throughout his article. Alonso and Schochat (2009), authors of an article cited positively by Bellis et al., used "(central) auditory processing disorders" in the title of their paper, but "APD" throughout the actual article. McArthur (2009), who reviewed six auditory training studies from 2007 and 2008, used the term CAPD to refer to a diagnostic category and the term APD to refer to a type of processing problem. The only consistency in nomenclature is that it continues to be inconsistent.

A more serious but related criticism is that we did not use "current diagnostic criteria for CAPD" (Bellis et al., 2012, p. 382) in selecting studies for inclusion in the systematic review. As was the case for terminology, Bellis et al. (2012) imply that we were unfamiliar with the 2005 Working Group's recommended criteria for diagnosing the disorder. To the contrary, as Richard (2011, p. 243) pointed out in the prologue to the clinical forum, our committee was in general agreement with the 2005 Working Group's single paragraph definition of auditory processing (ASHA, 2005, p. 2). In contrast, the 2005 Working Group's criteria for assessing and diagnosing APD is seven pages long, with subsections addressing test principles, the assessment battery, and criteria for diagnosing APD. The section on test principles (ASHA, 2005, p. 8) lists 13 principles that should be applied to determine the composition of a central auditory test battery.

The section on (C)APD tests lists seven different types as acceptable, if not important: auditory discrimination, auditory temporal processing, dichotic speech, monaural low redundancy, binaural interaction, electroacoustic measures, and electrophysiologic measures. The most specific guideline for diagnosis appears in the next section on test interpretation: "Diagnosis of (C)APD generally requires performance deficits on the order of at least two standard deviations (*SD*) below the mean on two or more tests in the battery" (ASHA, 2005, p. 12). The next sentence (p. 13) advises that audiologists should be alert to inconsistencies across tests that would indicate a non-auditory confound rather than (C)APD. Additional guidelines are provided in this section.

So, the criteria used to diagnose APD are hardly straightforward or universally accepted and applied. Even if we had adopted the recommendations of the 2005 Working Group on what constitutes APD, we had little reason to expect that researchers would have consistently applied it in their inclusionary criteria, especially before 2005. Rather than miss studies that used other definitions, our solution to this problem was to include all intervention studies that included participants who were diagnosed as having APD, regardless of the diagnostic criteria used by the authors. It is true that such a broad criterion led to the inclusion in our review of some studies with participants who did not meet the 2005 Working Group's criteria for APD. Our rating system for evaluating studies in the systematic review did, however, penalize studies for not having well-defined participant criteria.

Bellis et al. (2012) argue further, however, that our criterion of diagnosed APD in the systematic review caused us to "miss much of the accumulated peer-reviewed literature demonstrating the efficacy of auditory training… in individuals diagnosed with CAPD and auditory-based language-learning disorders" (p. 383). The definition of "auditory-based learning disorders" is even less established than that for APD, especially because it implies that the language-learning disabilities of individuals with APD are caused by auditory deficits, which has not been established (Miller, 2011). Consequently, we have nothing more to say about this clinical category.

Given the Bellis et al. (2012) criticism of our broad-based criterion for APD, however, one would expect that they would have used the 2005 Working Group's criterion in citing studies to support their claim that auditory training

is effective in children who have been diagnosed with APD. On page 383, they cite three studies not included in the systematic review that "confirm[ing] the efficacy of auditory training in individuals diagnosed with CAPD and auditory-based language-learning disorders." For Alonso and Schochat (2009), the diagnostic criteria for the 29 children (8 to 16 years) with APD included normal pure-tone, impedance, and brain stem results and "altered results in at least two central auditory processing behavior assessment tests" (p. 727). Individual participant data were not provided, so it is not possible to determine whether "altered results" consistently meant 2 *SD*s below the mean. Thus, the criterion for participant inclusion used by Alonso and Schochat is consistent with our requirement that reviewable studies include only those children with diagnoses of APD; it does not, however, meet the minimal standard set forth by the 2005 Working Group and endorsed by Bellis et al.

The two other examples cited by Bellis et al. (2012) are the seminal studies of Hayes, Warrier, Nicol, Zecker, and Kraus (2003) and Warrier, Johnson, Hayes, Nicol, and Kraus (2004). We cited these studies in our review as examples of the types of neurophysiologic testing that is needed in future research on the diagnosis and treatment of APD. We did not include these studies in our review, however, in part because their participants did not have diagnoses of APD. To be included in these studies, students with "learning problems" had to exhibit a discrepancy of at least 1 *SD* between an IQ measure and reading, spelling, phonological awareness, or auditory processing. The tests administered were the Wide Range Achievement Test (Wilkinson, 1993) and the Woodcock-Johnson Psycho-Educational Battery—Revised (WJ-R; Woodcock & Johnson, 1989). An auditory processing composite score on the WJ-R was derived from the Incomplete Words and Sound Blending subtests, but performance on these subtests was not a participant selection criterion in either study. In Experiment 2 of the Warrier et al. study (2004), children were further categorized as being inside or outside normal limits on a comparison measure of responding in quiet versus in noise. The criterion for being outside normal limits was 1 *SD* below the mean for the normal learner group. Thus, neither study would have been included in a systematic review that included only children with diagnosed APD, as in our review, or only children who meet the operational diagnostic criteria of the 2005 Working Group.

In sum, although they are not perfect, the broad APD criteria we used in the systematic review were justified for our purposes. Our procedures for grading evidence limited the possibility of weighting evidence from studies with poorly described participants too highly. We thus stand by our conclusion that "there is weak evidence to suggest that intensive, short-term auditory interventions…may be associated with improved auditory functioning among school-age children who meet broad criteria for APD, with or without accompanying spoken language disorder" (Fey et al., 2011, p. 252). We were less positive regarding the effects of auditory interventions on spoken and written language, and we found no evidence supporting the use of auditory integration therapy. As we will show, subsequent systematic reviews support this conclusion.

What Counts as Auditory Training?

Bellis et al. (2012) criticize our criteria for "auditory training" on two grounds. The first deals with our inconsistent treatment of Fast ForWord (Scientific Learning Corporation, 1998) as an auditory treatment and Earobics (Cognitive Concepts, 1997) as a language intervention. Bellis et al. correctly point out that we

> designated Fast ForWord as an "auditory" intervention, citing its acoustic manipulations but ignoring its considerable linguistic stimuli, while designating Earobics as a "language" intervention, despite the fact that the Earobics program includes focused activities/games that target fundamental sound—including speech-sound—discrimination through algorithmic acoustic manipulations. (p. 383)

Although we believe that our stated reasons for making this particular distinction are sound, we agree with Bellis et al. that our classifications of Fast ForWord and Earobics were too restrictive and might be described as "splitting hairs." Our review should have included all studies of Earobics used with children with APD or with spoken language impairment (e.g., Pokorni, Worthington, & Jamison, 2004).

It is unlikely, however, that this change in our position would satisfy Bellis et al. (2012) because of their particular definition of auditory training. For them, "true auditory interventions" depend on the goals of the training approach: "The goal of auditory training is not to improve spoken or written language abilities (AAA, 2010; ASHA, 2005a, 2005b).… Instead, auditory interventions are intended to improve auditory deficits that have been identified by valid tests of auditory function in a targeted, deficit-specific manner" (pp. 382). Thus, in the Bellis et al. view, neither Fast ForWord nor Earobics should be included as auditory interventions because the goal of these programs is to improve spoken and written language. At best, this perspective marginalizes our questions about the effects of auditory training on spoken and written language for children with APD or language disorders. At worst, it renders these questions and a large part of our review irrelevant.

We actually welcome the Bellis et al. (2012) definition of auditory training and recognize it as a positive step forward. For example, this definition should be perfectly reasonable to basic scientists who are interested in determining the modality specificity of APD (McFarland & Cacace, 2009). From a theoretical perspective, claiming that auditory training should be expected to have direct effects only on auditory measures removes one of the great barriers between APD enthusiasts and detractors, because this view implicitly rejects a causal relationship between CAPD and language impairment.

Despite our acceptance of the Bellis et al. (2012) definition of auditory training, we are, for two reasons, puzzled by their use of it to criticize our review. First, to build a case for the efficacy of auditory training, Bellis et al. frequently refer to studies that do not apply the new, more restrictive definition of auditory training. This undermines the very argument they are trying to make. Second, if auditory training leads only to auditory improvements, there is little reason for SLPs to be interested in the outcomes of auditory training research.

Consider key citations that Bellis et al. (2012) make in support of their position regarding the effects and efficacy of auditory training. On page 384, they refer readers to Loo, Bamiou, Campbell, and Luxon (2010) as a review of studies that demonstrate "both structural reorganization and functional improvement of auditory skills… *following auditory training* [emphasis added]." The Loo et al. review is titled "Computer-Based Auditory Training (CBAT): Benefits for Children With Language-and Reading Related Learning Difficulties." The title alone indicates that in this study, spoken and written language outcomes were perceived as the goals of auditory training. Furthermore, Loo et al. reviewed 21 studies of computer-based "auditory training." Thirteen of these studies, however, involved Fast ForWord, and three involved Earobics—interventions deemed by Bellis et al. not to be auditory training programs at all. Why do these programs count as auditory training in the review of Loo et al. but not in our review? The answer to this question cannot be because the Loo et al. conclusions were more favorable than our own to the Bellis et al. position. In fact, Loo et al. concluded that "there is some *initial evidence* to indicate that CBAT *may* remediate auditory processing and phonological awareness deficits, *with no clear effects on reading and spoken language* [emphasis added] in populations with language, reading, and reading related difficulties" (p. 715). Despite the differences in articles reviewed, this conclusion is much like our own, stated on page 253 (Fey et al., 2011).

More importantly, Earobics is the program that was used in the treatment studies by Hayes et al. (2003), Warrier et al. (2004), and Russo, Nicol, Zecker, Hayes, and Krause (2005). These studies are consistently cited by Bellis et al. (2012) as evidence of the effects of auditory training. Russo et al., however, correctly indicate that "*Earobics* is a commercial auditory training program that provides training through interactive computer games of *phonological awareness* [emphasis added], auditory processing, and *language processing* [emphasis added] skills" (p. 97). Based on this description, because it targets language outcomes, Earobics does not meet the Bellis et al. definition of true auditory training. So, based on their own standard, Bellis et al. cannot legitimately cite these studies as evidence in support of auditory training.

Our second reason for surprise with the Bellis et al. (2012) definition of auditory training as the basis for criticism of our review is that this comes at significant cost to the professional usefulness of auditory training research. If auditory training is expected to result in changes only in auditory-based behavior and auditory neurophysiologic function, it is not likely to be an acceptable treatment option to SLPs, who seek educationally relevant changes in academic, communication, and social performance and learning ability. Even if there were a strong clinical literature supporting the efficacy and effectiveness of auditory training as defined by Bellis et al., SLPs would likely find it of limited interest or importance. For example, it is unlikely that an individualized education plan goal of changing the latency or amplitude of a brainstem or cortical EEG waveform or of increasing a child's sensitivity to a nonnative sound contrast or contrast boundary would be acceptable to the school, the SLP, or the family.

In sum, we see some significant long-term advantages in the definition of auditory training that is recommended by Bellis et al. (2012), and we endorse it for future studies. There is, however, no evidence that investigators or clinicians implement interventions referred to as auditory with the expectation that beneficial effects will be limited to auditory functioning; in fact, it is clear that some interventions have been designed with spoken and written language objectives in mind. It also appears that Bellis et al. have not rigorously embraced their definition of "auditory interventions" in their own assessment of available research. Consequently, our committee's examination of evidence concerning the effects of broadly defined auditory interventions, such as Fast ForWord, on spoken and written language performance among children with language impairment was both well motivated and justified.

What Counts as Evidence of Intervention Efficacy

Perhaps the most significant general criticism Bellis et al. (2012) cast on our review is that we failed to consider many articles containing evidence that auditory interventions are efficacious. On page 384, Bellis et al. assert,

> but many forms of true auditory interventions and virtually all of the studies, both neurophysiological and behavioral in both animals and humans, that support the efficacy of true auditory training for auditory disorders were excluded. For example, missing from the Fey et al. review was any discussion of the myriad studies providing neurobiological evidence that supports the efficacy of true auditory training approaches for auditory dysfunction, including reports substantiating both structural reorganization and functional improvement of auditory skills, as well as neurophysiological representation of acoustic stimuli following auditory training.

Our primary concern here lies in what Bellis et al. (2012) consider as evidence of clinically relevant efficacy. As narrative reviewers, they are willing to use even basic research, including that involving animals and studies of typical adults or children, as support for the auditory effects

of auditory training. However, such studies are rarely included in evidence-based systematic reviews, and when they are, they are judged to represent the lowest level of evidence of treatment efficacy. Given the specific questions we were trying to address, such studies were not appropriate in our review. As Dollaghan (2007) noted, "This does not mean that basic research findings and theoretical considerations are necessarily irrelevant to applied questions; it just means that their relevance has to be tested rather than assumed" (p. 64). Our focus was squarely on evidence that directly addressed the five questions in the systematic review.

One might legitimately criticize our review for addressing questions that were too focused on children and for being interested in the effects of auditory interventions on children with spoken language impairment but not children with written language disabilities or other disabilities. It is true that broader questions would have resulted in our inclusion of more studies. On the other hand, given our questions and their focus, we believe we reviewed all of the appropriate and relevant literature up to 2008 and were sufficiently specific in reporting our conclusions. Consideration of research on animals or adults or even typical children would not have changed our analysis of the relevant literature for 6- to 12-year-old children with APD, spoken language impairment, or both.

Therefore, our conclusions apply only to children within the targeted age range and with the targeted intellectual and behavioral profiles. Similarly, because of the questions we addressed, we expect that our review is of limited value for individuals who are looking for evidence that an approach like Fast ForWord or Earobics (or some other more clearly disorder-specific intervention) might be a good option for, say, an adolescent client with right brain injury or auditory neuropathy.

Summary

In this response, we have addressed the major criticisms of Bellis et al. (2012). In doing so, we have defended most of our decisions and have shown that they were more reasonable than the alternatives proposed by Bellis et al. In one case, we acknowledged that because of its fundamental dependence on systematic acoustic modifications of game stimuli, Earobics should have been classified as an auditory approach rather than a language approach. With this change, we would accept the limited evidence that Earobics can improve phonemic awareness and slightly modify our general conclusion from the systematic review on page 254 (Fey et al., 2011): We believe that some interventions that are principally auditory *may* provide limited benefit in auditory function and phonemic awareness, but there is no compelling evidence that these interventions improve other language or academic outcomes for children diagnosed with APD or spoken language impairment.

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