Letters to the Editor

To the Editor:

I have strong reactions to some of the points made in the editorial published in JAAA, Vol. 14, no 1, 2003. I agree with much of what you have to say about the connection between the laboratory and clinical practice. I would make the same case for the value of integrating differential thresholds for sensitivity and frequency, temporal integration, and the use of other psychoacoustic principles in the APD battery. Where we disagree is in the language you use to describe the Wilson et al. article. Specifically, the editorial states that that the Wilson article “reports normative data for a CD version of the MLD.” In fact the article reports central tendency data for the subjects in the study. The subjects are young adults, and only 24 of them. It would have been more accurate for the editorial to state that the Wilson article provides findings on young adults who are considered normal. There are not nearly enough subjects, nor are the subjects from a widely distributed geographic, SES, educational level, etc. to be considered normative data. Further, the data are too limited to provide real clinical insights into the test-retest variability in a clinic population. The data are for young adults, there are no data for subjects older than 30 and none for subjects younger than 20. This is especially important because you link the MLD to children in your additional remarks, but there are no data related to children.

Additionally, the editorial states “MLD is reduced in children with APD, in some children with histories of COM.” The editorial needs to be accurate in stating that the MLD is reduced in some children with APD, and in some children with histories of COM. We do not actually know the prevalence of abnormal MLD data in any of these groups. The reader who takes for granted that what you write is truth might not see these subtle differences in language.

The Wilson article is a fine piece of research and important to the literature. However I think that the editorial is misleading.

Robert W. Keith
University of Cincinnati

A number of authors have been concerned about potential problems in the differential diagnosis of (central) auditory processing disorder (CAPD or APD) and attention-deficit hyperactivity disorder (ADHD) (e.g., Cacace and McFarland, 1998; Gascon et al., 1986; Riccio et al., 1994). Chermak et al (2002) suggest that professionals use different behavioral descriptors to characterize CAPD and ADHD (primarily inattentive type, PI). They present data showing that questionnaire items most consistently endorsed by pediatricians (+2 standard deviations [SDs] above grand mean) differ from those most consistently endorsed by audiologists. They conclude, “Respondents identified a reasonably exclusive set of behaviors that most highly characterize APD and ADHD-PI.” There are two fundamental problems with this study. First, data of this kind does not establish that a differential diagnosis of CAPD vs. ADHD can be made reliably. Second, characterizing the data in terms of overlap between items 2 SD above the grand mean misses the major trend in the data.

Chermak et al (1999) have argued that although ADHD and CAPD have attention deficits in common, they can be differentiated by the nature of the attention deficits. They suggest that multimodal attention deficits may characterize ADHD and that selective and divided auditory attention deficits may characterize CAPD. However, no real data are provided and these distinctions remain at best conceptual in nature. Chermak et al (1999) further suggest that inclusion of the auditory continuous performance test (Keith, 1994) should aid in “substantiating this distinction”. However, using a test of sustained attention that involves only the auditory modality will not establish the modality specificity of the attention deficit.

The problem is that the same child might receive a diagnosis of CAPD if he/she visits an audiologist and ADHD if he/she visits a pediatrician. This could occur despite the fact that these two professional groups conceptualize these disorders differently. This point is illustrated by results obtained by Cook et al (1993), who report that 12 of 15 children previously diagnosed with ADHD,
determined criterion for CAPD. None of the 10 control children met this criterion. Pediatricians made the diagnosis of ADHD and the study also included questionnaires filled out by parents and teachers. Diagnosis of CAPD was based on below age-level performance on at least 3 of 5 tests from a battery that included the staggered spondaic word (SSW) test and tests from the Willeford battery. Thus the two methods of diagnosis identified a common group of children.

The data presented by Chermak et al (2002) do not address the issue of diagnostic overlap. Rather, they only consider the issue of “conceptual” overlap. Nonetheless, we think these data show considerable conceptual overlap between the way pediatricians think about ADHD and the way audiologists think about CAPD. Although Chermak et al (2002) report that items +2 SDs above the grand mean for pediatricians and audiologists do not overlap, this analysis only considers a small part of the actual data. A complete scatter plot of data from the appendix of Chermak et al (2002) is shown in Figure 1. It is apparent that the two sets of rankings are highly correlated. In fact, the value of Pearson’s $r$ is 0.75 ($p<0.0001$). Thus, an analysis involving all items in the appendix shows considerable overlap.

There are two questions that must be answered by research. To what extent do the populations of children meeting criterion for ADHD and CAPD overlap and to what extent do the instruments used for assessment correlate. These are the basic types of data that are necessary to answer questions concerning the relationship between current CAPD and ADHD diagnostic procedures. There are many conceptual distinctions that can be made but only data concerning diagnostic overlap and intercorrelations of measures provide direct evidence about the distinctiveness of ADHD and CAPD as diagnostic entities. On the other hand, there is considerable room for improvement in the methods used to diagnose both disorders. We contend that the use of multimodal test batteries would prevent inclusion of supramodal processing disorders (like ADHD) from being diagnosed as CAPD (McFarland and Cacace, 1995; Cacace and McFarland, 1998). There are also many problems with the diagnosis of ADHD, not the least of which is the fact that diagnosis is based primarily on parent and teacher report. This is the case with either formal questionnaires or informal report to professionals such as a pediatricians, neurologists, psychiatrists, or psychologists. Furthermore, concepts such as impulse control, attention, and executive functioning are difficult to define and measure. In contrast, auditory processing disorders can be defined simply as modality specific effects as measured by psychophysical procedures (McFarland and Cacace, 1995).

REFERENCES


Dennis J. McFarland  
Anthony T. Cacace  
Wadsworth Center  
New York State Health Department  
Albany, New York  
Department of Surgery  
Albany Medical College

Reply to McFarland & Cacace

We appreciate McFarland's and Cacace’s interest in our work and the opportunity to comment on their remarks regarding our 2002 study, as well as a concern they raised about an earlier paper. We agree with McFarland and Cacace that the type of data we reported in our 2002 study does not establish that a differential diagnosis of APD vs. ADHD can be made reliably. However, establishing a differential diagnosis was not the goal of our 2002 study. As stated, “this research sought to compare the salience of behavioral signs of APD and ADHD-PI between those who conventionally diagnose each disorder” (p. 334). We also agree the Chermak, Hall and Musiek (1999) publication was largely a conceptual paper, examining the differentiation of APD and ADHD from the perspectives of neurobiology, audiology and cognitive psychology.

McFarland and Cacace seem to disagree with the assertion, in our 1999 paper, that inclusion of auditory continuous performance tests “should prove helpful in substantiating” the distinction between APD and ADHD. (p. 292). Apparently, they discount the value of a global measure of auditory vigilance/executive function as a component of an auditory processing evaluation or diagnostic test battery, arguing instead for a multimodality approach. Again, we agree that multimodality testing serves a useful role in differential diagnosis. We would argue, however, that both multimodality testing and global measures of auditory attention should be included in a comprehensive (and multidisciplinary) test battery to differentially diagnose APD from both multimodal and supramodal deficits (Jerger and Musiek, 2000; Tillery, Katz, and Keller, 2000).

Finally, we see no merit in their argument concerning our statistical approach to data analysis reported in the 2002 paper. This study was predicated on the frequency with which clinical symptoms of APD and ADHD overlap. We were interested in the differences between professionals’ ratings of behaviors. It was not our objective to examine the relationship between ratings of the two disorders. Indeed, use of a correlational approach, as suggested by McFarland and Cacace, would not help us answer our question, and in fact, would simply confirm what many others have reported (see Chermak et al. 1999 for extensive references) without providing insights as to the source of this comorbidity or association. Not surprisingly, and as demonstrated by McFarland’s and Cacace’s calculation (r= 0.75), our data are highly associated. We elected to examine differences in professional ratings of behaviors by identifying behaviors that were ranked 1 and 2 SD above the respective grand means by audiologists and pediatricians. This enabled us to identify behaviors that the audiologists and pediatricians we surveyed considered highly characteristic of APD in contrast to ADHD-PI, thereby offering direction for additional research to advance both differential diagnosis and differential intervention for these disorders.

REFERENCES


Gail D. Chermak  
Ellen Tucker  
Tony Seikel  
Western Washington University

280