Practice Guidelines for Direct Attention Training

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This article is part of a series of reports from a committee charged with developing evidence-based practice (EBP) guidelines for rehabilitation of cognitive-communication deficits following traumatic brain injury (TBI). We examine the literature for evidence of the effectiveness of direct attention training to treat attention impairments following TBI. Evidence is gleaned from the outcomes of nine Class I and Class II studies that span intervention from acute to outpatient rehabilitation. Results and discussion are organized using five key questions as a mechanism to review the research to determine if the approach, outcomes, and associated recommendations warrant a change in clinical practice. The key questions are: Who are the participants who received the intervention? What comprises the attention training? What are the outcomes of the intervention? Are there methodological concerns? Are there clinically applicable trends across different attention remediation studies? The complexities and difficulties inherent in implementing clinical trials with the heterogeneous TBI population are discussed. The article concludes with treatment guidelines and options supported by the research review. Future research needs are highlighted.

In recent years, the leadership of the Academy of Neurologic Communication Disorders and Sciences (ANCDS), the American Speech-Language-Hearing Association (ASHA), the Special Interest Division 2 (SID 2—Neurophysiology and Neurogenic Speech and Language Disorders) of ASHA, and the Veterans Administration recognized the trend toward referencing research evidence to support clinical decision making in the management of medical conditions. In 1997, ANCDS embarked on the task of establishing committees of experts to develop evidence-based practice (EBP) guidelines for the following areas: dysarthria, aphasia, dementia, apraxia, and cognitive rehabilitation for traumatic brain injury. This article is part of a series of reports from the committee charged with developing EBP guidelines for rehabilitation of cognitive-communicative deficits following traumatic brain injury. For an overview of the committee process, the reader is referred to the project introduction and initial committee report (Golper et al., 2001; Kennedy et al., 2002).

The initial task of our subcommittee was to outline our philosophy and assumptions relevant to EBP and clinical decision making. This required us to define "evidence." Evidence is the *reason* a clinician would pursue a specific treatment decision. Central to our position is the proposition that the science of clinical decision making for complex and diverse populations includes, but is not restricted to, information generated by randomized controlled trials and other clinical experiments. We described our basic premises in an initial report and position paper (Ylvisaker et al., 2002). An early task was to define the project scope by identifying the major areas within the field of cognitive-communicative rehabilitation (CCR) for which we would develop guidelines. These areas include cognitive-communication assessment, attention training, management of memory impairments, social skills/behavior regulation management, and intervention for metacognitive and executive function deficits. This current article addresses attention training. Specifically, we discuss the evidence relevant to treatment outcomes associated with direct or structured attention training for attention impairments following TBI.

THE EVIDENCE-BASED PRACTICE MOVEMENT

In an attempt to establish defensible, validated intervention practices, the field of cognitive rehabilitation has joined the healthcare movement's vigorous commitment to developing EBP. The primary goal of this burgeoning reform is to ensure that clinical decisions are guided by empirical evidence and, ideally, evidence from well-controlled studies that systematically evaluate outcome, efficacy, and effectiveness of specific interventions (Golper et al., 2001). More specifically, the charge is to develop evidence-based guidelines that causally link treatment protocols to expected clinical outcomes (Robey, 2001). Increasingly, however, it is recognized that strict adherence to a preordained experimental model as a filter to accept or reject particular clinical practices in a heterogeneous population (e.g., the TBI population) may result in unsuccessful generalizations from research to treatment outcomes. Scientifically sound clinical decision making includes analyzing existing experimental evidence *in addition to* a diverse set of individual patient considerations and possibly the result of client-specific experimental intervention trials (Montgomery & Turkstra, 2003; Ylvisaker et al., 2002).

The development of EBP guidelines for CCR in general, and specifically for direct attention training, required us to become conscious of our assumptions and biases relevant to analyzing rehabilitation research. This process made us mindful of three fundamental beliefs. First, when reviewing the literature, we realized the narrow question "Does it work?" oversimplified the complexities behind direct attention training delivered to TBI survivors. Second, we found it critical to remember the bidirectional nature of the relationship between practice and research. Third, we maintain that intervention outcomes are relative to personal goals and values.

Researchers are charged with the task of conducting clinical research that effectively evaluates specific aspects of particular interventions in light of previous work. To make well-founded clinical judgments, practitioners are challenged with the responsibility of integrating existing research findings with their own clinical experience, while respecting the unique challenges presented by their clients. We boldly suggest that it may not be possible to answer the question "Does attention remediation work?" using an unequivocal yes/no format. Such a question assumes that attention remediation is a uniformly delivered intervention that can be compared across studies and that the myriad of relevant contextual variables present in a clinical practice can be dismissed.

Our concern is that the unequivocal "Does it work?" question is divisive and encourages a battle that is neither helpful nor illuminating. For decades, the field of CCR seems to have been trapped in an internal debate over whether it is better to focus on training processes, skills, or complex functional abilities, and in what ways and in what contexts that training might best be accomplished. Although the battle is not over, it is no longer disputed that functional changes must be the goal of treatment and that there are many ways to facilitate functional changes, with an equal number of ways to measure them. The field continues to struggle with a quagmire of measurement issues. We have, however, learned that a one-size-fits-all solution does not work. Individuals and families respond to different interventions in different ways and at different times after injury. The response difference is in part demonstrated by the widely divergent treatment results reported in the literature, which range from no change in performance after treatment to significant improvements (Sohlberg & Mateer, 2001). As described in this current article, the wide swings of outcome in the attention remediation literature suggest that the question "Does it work?" may be better replaced by "When does it work best and for whom?" Then we can move on to such questions as "Is it worth the effort?"

No one disputes the value gained when research advances and improves clinical practice. However, the investigation process is not unidirectional. Clinical practice also shapes research questions. The patient-specific hypothesis testing (PSHT) approach discussed by Ylvisaker and Feeney (1998) reinforces the symbiotic relationship between research and practice. Neither service delivery nor applied research exists in isolation. Clinicians rely on well-founded research to generate clinical procedures that can be defended and supported within their delivery contexts. They must also often employ diagnostic therapy to determine treatment effects in specific patients. In turn, researchers' study of current clinical practices generates new research questions and highlights issues relevant to treatment efficacy, particularly when the research replicates the realities of clinical delivery (e.g., patient demographics, dose, and duration limits). This iterative process is critical to the development of EBP.

One important assumption in our view of developing EBP from existing literature is that this process necessarily draws on personal goals and values. Montgomery and Turkstra (2003) stated that a problem with developing evidence-based guidelines is that what constitutes a meaningful clinical outcome is a personal and social judgment. As such, the ability to scrutinize the literature for evidence will never be a wholly scientific process in the traditional sense of science. The ever-evolving definitions describing different types of outcomes promoted by the World Health Organization (WHO) highlight the fact that this construct is a reflection of social value. Consider the scenario of two CCR experts evaluating a research outcome for an article investigating the effectiveness of attention remediation. One analyst may examine the improvements in neuropsychological test scores following attention training as proof of treatment success, while the other interprets it as a dismissable treatment artifact. This is indeed the case within the research (Park, Proulx, & Towers, 1999; Sohlberg, McLaughlin, Pavese, Heidrich, & Posner, 2001). Acknowledgment of the subjective and value-laden nature inherent in analyzing outcomes is critical to the development of useful guidelines that help clinicians navigate through the literature.

ESTABLISHING GUIDELINES FOR ATTENTION TRAINING

Establishing practice guidelines for attention training is a current focus in the field of CCR (Cicerone et al., 2000). The committee adopted a broad definition of attention. We were interested in interventions that addressed a wide assortment of skills, processes, and cognitive states that relate to the ability to focus and process incoming information. At first blush, it may appear a straightforward process to measure and analyze the attention outcomes in individuals with brain injury who have received attention training. Unfortunately, close inspection reveals this is not the case.

Direct Attention Training as an Intervention

Attention training is based on the premise that attentional abilities can be improved by activating particular aspects of attention through a stimulus drill approach. The repeated stimulation of attentional systems via graded attention exercises is hypothesized to facilitate changes in attentional functioning (Cicerone et al., 2000; Sohlberg & Mateer, 2001). Most attention training programs assume that aspects of cognition can be isolated and discretely targeted with training exercises. The aspects of attention that are addressed vary widely among interventions and frequently depend on a theoretical model of attention. Attention models, regardless of their operational framework, appear to include functions related to sustaining attention over time (vigilance), capacity for information, shifting attention, speed of processing, and screening out distractions. Some attention efficacy studies evaluate attention interventions that focus on particular attention components such as reaction time and sustained attention for visual information (e.g., Ponsford & Kinsella, 1988). Other efficacy studies use attention training programs that include hierarchical tasks to address a continuum of attention components from basic sustained attention to more complex mental control (e.g., Park et al. 1999; Sohlberg et al., 2001).

Traditionally, studies employ a unique battery of measures to assess possible changes related to intervention. Some studies limit their outcome measures to individually selected neuropsychological tests (e.g., Park et al., 1999; Sturm, Willmes, Orgass, & Hartje, 1997). Alternatively, studies may utilize patients' perception of change (e.g., Cicerone, 2002; Sohlberg et al., 2001) or their task performance in an activity assumed to require attentional processing (Kewman et al., 1985) as an indicator of attention functioning. The disparate outcome markers render it difficult to equate successes and therefore compare interventions.

There are several additional complications that must be considered when reviewing the attention literature. One consideration is the rich opportunities provided by cross-population inferences. As discussed by Ylvisaker et al. (2002), there is a wealth of information on populations who are neurologically and/or symptomatically close to individuals with brain injury. Although our review is limited to the attention training literature on individuals with traumatic brain injury, we want to acknowledge extensive information relevant to attention remediation available in other literature domains such as special education. Additionally, from an ecological standpoint, it may be remiss to evaluate one particular approach for addressing attentional impairments when in reality clinicians often combine interventions. For example, direct training is often paired with pharmacological management, training in the use of external aids, and/or metacognitive training.

Scrutinizing the Attention Literature Using Key Questions

As discussed, we submit that answering a binary question querying whether attention training does or does not work is not possible, given the complexity of client, treatment, and research variables coupled with the value-laden nature of outcomes. We offer, as an alternative, a template of five key questions that professionals can employ to evaluate the literature. This approach offers a mechanism to filter the past, current, and future attention intervention literature and help clinicians decide if the approach, outcomes, and associated recommendations are a "good fit" for their practice. The hope is that the template will allow clinicians to scrutinize the research to determine if research results warrant a change in clinical practice and allow them to develop an evolving portfolio of evidence-based treatment practices. We further hope that the template will assist researchers in designing studies that reflect clinical realities. Key guiding questions for examining the attention training literature are:

1. Who are the *participants* who received the intervention?
   - What is the diagnosis or etiology?
   - What is the injury severity?

- What is the participant's age?
- What is the participant's level of education?
- What is the time postonset of injury?
- Are there dual diagnosis or comorbidity factors?
- What is the participant's cognitive profile postinjury?

2. What comprises the *attention training*?
   - What are the focus and rationale?
   - What are the treatment duration and frequency?
   - Where is the treatment setting?
   - Who are the providers?
   - Are training programs personalized to match client skills and/or needs?
   - Are other interventions incorporated into or in addition to the delivery of attention exercises (e.g., reinforcement or strategy training)?
3. What are the *outcomes* of the intervention?
   - Are there measures suggesting changes in attention impairment (e.g., psychometric tests) following treatment?
   - Are there measures suggesting changes in activity/participation (e.g., changes in an attention-demanding skill such as driving, changes in perceptions/rating of ability by client and/or caregiver)?
   - Are reported changes clinically meaningful?
   - Is there maintenance or generalization of any reported changes?
4. Are there methodological concerns? Are there other explanations for given outcomes, and may results be either exaggerated or hidden?
   - What is the study design?
   - Are treatments compared to an alternative or no treatment condition?
   - Are reliability and/or validity issues addressed?
5. Are there clinically applicable trends across different attention remediation studies?
   - Are there robust findings that warrant a change in practice?

A retrospective application of these key questions to a former cognitive rehabilitation intervention outside of attention remediation serves as an illustration. Consider the example of memory drills that were a common memory impairment intervention during the 1980s. Using this approach, clinicians gave clients multiple trials of list learning or paragraph listening tasks as a means to improve recall ability (e.g., Glisky & Schacter, 1986). Eventually, this approach fell out of favor when both clinicians and researchers found no improvement in either clients' or subjects' memory functioning. Had clinicians subjected the literature to the proposed questioning sequence in the template, the field may have been quicker to discard an ineffective treatment. Specifically, if clinicians had focused their reading of the literature on the measurement outcomes (question 3), they would have noted that memory drills occasionally resulted in improved list learning for specific lists, but that generalization was poor and no improvements were seen in other memory measurements (Godfrey & Knight, 1985; Moffat, 1992). Using the WHO outcome nomenclature, there was a specific improvement in a constrained activity, but no changes in the underlying impairment or activities/participatory performance on related activities.

What Do Existing Evidence-Based Reports Reveal About Attention Remediation?

At the time of this article, there are two comprehensive reviews examining the attention remediation literature (Cicerone et al., 2000; Park & Ingles, 2001). Park and Ingles (2001) reported on a meta-analysis of the attention rehabilitation literature. They coded and analyzed 26 studies as direct retraining studies and concluded that subject performance improved significantly on tasks that were trained, but found no evidence of treatment effects on tasks that were different from those trained. For some of the studies reporting a statistically significant improvement in one or more measures of attention (e.g., Gray, Robertson, Pentland, & Anderson 1992; Niemann, Ruff, & Baser, 1990; Park et al., 1999; Sturm et al., 1997), Park and Ingles suggested that the pattern of improvement was attributable to the acquisition of *specific skills* rather than to the training of attention. It is not possible to scrutinize their findings using our five key questions because the research methodology consolidates studies with different subject profiles, treatment approaches, and outcome measures. For example, many of the participants in the reviewed studies had severe brain injuries resulting in impairment to basic attentional processing; but because grouped data are presented, it is difficult to examine individual profiles. Similarly, the grouped data render it difficult to determine potentially important intervention characteristics. For example, the treatments they analyzed were administered an average of 31.2 hr, but with a standard deviation of 32.7 hr. Nonetheless, this study reminds us of the importance of analyzing the potential transfer of any observed training effects to related tasks. It also encourages the field to address definitional ambiguities such as the distinction between attention *skills* and attention *processes*. Further, the meta-analysis highlights the question of effect size—if there are observed changes in either skill or process performance, we must look at the magnitude of change to determine clinical relevance.

Cicerone et al. (2000) reported the findings of a subcommittee of the American Congress of Rehabilitation Medicine (ACRM) that analyzed existing research addressing CCR interventions for persons with TBI and/or stroke. They selected both treatment efficacy studies (highly constrained, time-limited research with mostly homogenous samples) and studies of clinical effectiveness (empirical treatment evaluations within clinical settings, which reflected the actual use of an intervention). They used a screening process resulting in the selection of 171 articles for inclusion in their review. Thirteen of these studies were assigned to the category of remediation of attention deficits. Of these 13 studies, 3 were classified as well-designed, prospective, randomized controlled trials (Class I studies); 4 were classified as using prospective, nonrandomized controls or a clinical series with controls (Class II studies); and 6 employed single-subject design or clinical reports without controls (Class III studies).

Trends across studies were summarized, describing the clinical implications of their literature review (Cicerone et al., 2000):

> Evidence . . . supports the effectiveness of attention training beyond the effects of nonspecific cognitive stimulation for subjects with TBI or stroke during the postacute phase of recovery and rehabilitation . . . Interventions should include training with different stimulus modalities, levels of complexity and response demands. The intervention should include therapist activities such as monitoring subjects' performance, providing feedback, and teaching strategies. Attention training appears to be more effective when directed at improving the subject's performance on more complex, functional tasks. However, the effects of treatment may be relatively small or task specific, and an additional need exists to examine the impact of attention treatment of ADLs or functional outcomes. (p. 1600)

Summary of Studies Included in Existing Review Papers

In this section, we utilize our five key questions as the organizing matrix to review the findings from those studies reported to have the strongest research methodology in the existing comprehensive reviews of attention remediation (i.e., the Class I and Class II studies discussed in the reviews by Cicerone et al., 2000, and Park & Ingles, 2001). We selected only those studies that specifically evaluated the direct training of attention to subject pools that included survivors of traumatic brain injury. Table 1 presents the Class I and Class II studies for subjects in the acute rehabilitation phase. Table 2 summarizes the Class I and Class II studies for subjects in the postacute rehabilitation phase. We review this older body of literature collectively and then examine the recent literature by discussing the most recent studies individually.

Reliability for coding the six studies using the five key questions was examined. The first two authors independently reviewed four of the six studies (.66) and compared completed tables. There was 100% reliability on key points, with two exceptions. There was not a consistent description of the focus and rationale of treatment (key question 2) between the two raters. Further, for several studies, there was disagreement about whether outcomes (e.g., responses to questionnaires) reflected impairment or activity/participation level changes. With minimal discussion, consensus was reached on both issues.

Who Received the Intervention?

Consistent with our premise that readers must scrutinize research to determine its relevance to their clinical practice, the wide variety of subject characteristics does not merge neatly into a singular pool of shared demographics. Two studies (Novack, Caldwell, Duke, Bergquist, & Gage, 1996; Ponsford & Kinsella, 1988) focused on acute rehabilitation patients, and the remaining four (Gray et al., 1992; Niemann et al., 1990; Sohlberg & Mateer, 1987; Strache, 1987) focused on outpatient clients. Only three of the six articles (Niemann et al., 1990; Novack et al., 1996; Ponsford & Kinsella, 1988) reported on subjects with only TBI or closed head injury (CHI); the other studies' subject pools included unspecified "non-traumatic brain injury" (Gray et al., 1992), CVA (Strache, 1987), and aneurysm and penetrating head injury (Sohlberg & Mateer, 1987). Three of the six studies examined performance of subjects with severe or very severe injuries (Novack et al., 1996; Ponsford & Kinsella, 1988; Sohlberg & Mateer, 1987). One study's subjects (Niemann et al., 1990) spanned moderate to severe injuries, and another study (Gray et al., 1992) described their participants as having mild to moderate difficulties with attention, but did not provide information on the nature or degree of actual brain injury. No injury severity information was provided in the sixth study (Strache, 1987).

TABLE 1. Class I and Class II studies for participants in the acute rehabilitation phase.

Reference	Novack et al., 1996 Class I	Ponsford & Kinsella, 1988 Class II
Clients/Subjects		
Number of subjects	22 pairs	10
Controls	Random assignment to "focused" vs. "unstructured" remediation	16 ortho patients (evaluation not treatment controls)
Diagnosis/etiology	TBI	CHI
Severity	"severe"	"very severe"
Age	m = 27.8 (13.2) yrs vs. 26.4 (10.9) yrs	m = 24.4 (8.7) yrs vs. 25.8 (7.8) yrs.
Education	m = 11.5 (2.4) yrs vs. 11.8 (1.6) yrs	m = 11.0 (1.9) yrs vs. 11.8 (2.0) yrs.
Time postonset	m = 5.9 (3.3) wks vs. 6.4 (4.9) wks	range = 6–34 wks
Dual dx/co-morbidity	Not provided	Not provided
Cognitive profile postinjury	Not provided	Sufficient sensory and motor function for computer tasks
Attention Training		
Focus	Focused vs. unstructured computerized activities. Focused tasks targeted vigilance	Computer tasks: directed stimulation vs. independent work. Tasks emphasized vigilance
Rationale	Focused program will promote more extensive recovery of attentional skills	Cognitive benefit of boosting speed and selectivity of information processing
Duration/frequency	m = 20.5 (8.8) sessions vs. 20.9 (11.7) sessions. 30 min. 5 × wk	30 min for 15 days without feedback; 30 min for 15 days with reinforcement
Treatment setting	Rehab hospital	Rehab hospital
Providers	Masters level educator or psychometrician	Not provided
Program individualization	Advancement for focused group based on level of accuracy	Not provided
Additional or concurrent cognitive intervention	Cuing during focused activities to ensure success	Feedback, reinforcement, and graphing of results for one phase
Outcomes		
Impairment/psychom. change	No significant differences in attentional skills or general cognitive abilities	No significant treatment effects; trends noted on single subject performance level
Participation level changes	No significant difference of ADL or cognitive FIMs	No significant change on Rating scale. Ceiling effect seen on video measurements
Methodology review		
Study design	Pre- to posttreatment comparison	Multiple baseline across subjects
Reliability	Not provided	Not provided
Validity	Not provided	Not provided

A general trend was seen for age, with the majority of subjects falling between the range of 25 to 35 years. Two studies (Niemann et al., 1990; Strache, 1987), however, included subjects above the age of 60. Education trends were also seen. The subjects of five studies had at least 9 years of edu-

TABLE 2. Class I and Class II studies for participants in the postacute rehabilitation phase.

Reference	Gray et al. 1992 Class I	Niemann et al. 1990 Class I	Sohlberg & Mateer, 1987 Class II	Strache, 1987 Class II
Clients/Subjects				
Number of subjects	17	13	4	Two groups of 15
Controls	14	13	None	15
Diagnosis/etiology	TBI or non-TBI	TBI	CHI, PHI, aneurysm	Head trauma, CVA, other etiologies
Severity	"Mild-moderate to severe" attentional dysfunction (not severity of injury)	Moderate to severe	24 hrs to 7 weeks LOC	Not provided
Age	m = 26.18 (7.58) yrs vs. 34.14 (18.44) yrs.	28.9 (8.2) yrs vs. 34.3 (12.0) yrs.	25–30 yrs.	m = 32 yrs (range 20 to 70)
Education	Not provided	13.8 (1.8) yrs vs. 13.7 (2.5) yrs.	11–13 yrs	9 or more yrs
Time postonset	Range:7 weeks to 10 yrs	41.0 (21.5) months vs. 37.1 (20.1) months. Range: 12–72 months	12–72 months	Less than 6 months to more than 3 yrs
Dual diagnosis/ co-morbidity	Not provided	No substance abuse issues. No premorbid psych. admissions	Not provided	Not provided
Cognitive profile postinjury	Subjective reports of poor concentration for real-life situations	DRS score of 100. No severe apahasia	VIQ at 80–87; PIQ at 74–98; FSIQ at 77–85	Not provided
Attention Training				
Focus	Microcomputer delivered tasks: speed of attending and information processing	Computer assisted attention retraining program	APT and other commercial products	Apparatus supported training for deficits of concentration
Rationale	Increased skills will reflect improvement in similar neuropsych tests	Computerized attention training will improve measures of attention but not memory	Repeated stimulation of attention will improve impaired systems	Increased attention skills will improve physical, psychological, social, and vocational process of rehab (extrapolated—not directly stated)

(continues)

TABLE 2. *continued.*

Reference	Gray et al. 1992 Class I	Niemann et al., 1990 Class I	Sohlberg & Mateer, 1987 Class II	Strache, 1987 Class II
Duration/frequency	m = 15.35 (2.06) hr over 3–9 wks vs. 12.7(3.8) hrs recreational computing	Two 2-hour sessions/ wk × 9 weeks	7–9 training sessions/ wk for 4–8 weeks	Twenty 30-min sessions over 4 wks
Treatment setting	Outpatient/postacute clinic	Outpatient program	Post-acute day treatment program	Neurologic rehabilitation center
Providers	Not provided	Not provided	Not provided	Trained assistants under instruction of neuropsychologist
Program individualization	Not provided	Advancement through menu of tasks per predetermined criteria	Treatment tasks and degree of difficulty re: intake testing. Advancement per individual criteria	Assigned to group re: intake testing. One group with standardized training, one group with progress-dependent advancement
Strategy training or feedback	Yes	Yes	Not provided	Not provided
Outcomes				
Impairment/psychometric Change	Improvement on 2 measures of attention and in auditory working memory at 6 months follow-up.	Significant improvement on 4 measures of attention. No treatment effect generalization to dependent variables	Significant gains in attention skills but not in visual processing	Improvement for both groups beyond spontaneous recovery, test repetition, or direct training of tested functions. "Slight but clear advantage" for the baseline and progress-dependent group on mnemomic functions
Participation level changes	Not provided	Not provided	All subjects improved in independent living or return to work status	Not provided
Methodology review				
Study design	Pre- and posttreatment comparison	Pre- and posttreatment comparison	Single subject multiple baseline across behaviors design	Pre- and posttreatment comparison
Reliability	Not provided	Not provided	Not provided	Not provided
Validity	Not provided	Not provided	Not provided	Not provided

cation, and two studies (Novack et al., 1996; Sohlberg & Mateer, 1987) reported subjects with at least 1 year of college. Gray et al. (1992) did not provide premorbid education demographics.

Given the heterogeneous nature of survivors of TBI, it was striking that only one of the six studies provided comorbidity information (Niemann et al., 1990). Descriptions of cognitive profiles at the onset of treatment varied widely. Specific markers such as IQ (Sohlberg & Mateer, 1987) and subjective descriptions of distractibility (Gray et al., 1992) were used. Studies also reported results of intake testing, but specific details were often lacking and interpretation was left to the reader (e.g., Strache, 1987).

What Comprised the Attention Remediation?

Details of treatment specifics varied widely among the six studies and ranged from specific descriptions of tasks (Ponsford & Kinsella, 1988) to general treatment profiles (Strache, 1987). Computerized attention programs were used in all six studies and were either the sole focus of the treatment (Gray et al., 1992; Novack et al., 1996; Ponsford & Kinsella, 1988; Strache, 1987) or were supplemented with other tasks (Niemann et al., 1990; Sohlberg & Mateer, 1987).

Studies also varied in the explicitness of descriptions of the rationale and focus of the treatment. In broad strokes, each of the studies used exercises designed to stimulate discrete types of attention, with the expectation that improved performance would follow repetitive drill. The duration and frequency of interventions, however, varied between the studies. Specific attention tasks in at least one study lasted for 5–10 min (Niemann et al., 1990). Treatment sessions ranged from 30 min (Novack et al., 1996; Ponsford & Kinsella, 1988) to 120 min per session (Niemann et al., 1990) and varied from one to two sessions daily (Sohlberg & Mateer, 1987) to twice per week (Gray et al., 1992; Niemann et al., 1990). Overall length of treatment varied from 4 (Niemann et al., 1990) to 9 (Novack et al., 1996) weeks. As mentioned, treatment occurred in acute rehabilitation settings for two studies (Novack et al., 1996; Ponsford & Kinsella, 1988) and in outpatient clinics or settings for the remaining four. Treatment providers ranged from "trained assistants" (Niemann et al., 1990) to teachers with graduate degrees (Novack et al., 1996). No information was given about the providers in the other four studies.

Individualized treatment plans following screenings or extensive evaluations were created in three studies (Novack et al., 1996; Sohlberg & Mateer, 1987; Strache, 1987). A hierarchy of exercises based on assumed increases in difficulty was used in all six studies, although criteria for advancement to more difficult tasks varied from operationally defined parameters (Niemann et al., 1990; Sohlberg & Mateer, 1987) to individual clinician decision (Novack et al., 1996) to advancement contingent on success at each level of difficulty (Strache, 1987). Speed of processing was a key component in the tasks of at least four of the studies (Gray et al., 1992; Ponsford & Kinsella, 1988; Sohlberg & Mateer, 1987; Strache, 1987). Two studies (Novack et al., 1996; Ponsford & Kinsella, 1988) focused on vigilance and selective attention skills, although a small percentage of Novack et al.'s subjects advanced to alternating attention drill. Subjects from the remaining four studies were exposed to alternating and/or divided attention drill for at least portions of their treatment protocol.

Studies varied on reinforcement, feedback, and strategy training. Novack et al. (1996) strategically added cues to guarantee subject success. Scores reported to subjects included formal reporting (Niemann et al., 1990), discrete computer screen displays of task scores (Ponsford & Kinsella, 1988), comprehensive feedback (Niemann et al., 1990), and graphing of performance changes over time (Ponsford & Kinsella, 1988). At least two studies used tasks as a springboard for strategy training (Gray et al., 1992; Niemann et al., 1990). One task in Gray et al's. study focused on overt "verbal regulation" training (i.e., coaching the client to verbally self-cue as an attention strategy) during an alternating attention task.

What Are the Outcomes of Intervention?

The six articles offer excellent opportunities to examine the complicated issues related to the subjective values underlying the claim of a successful or unsuccessful outcome. In general, the studies tended to employ a battery of measurements to assess changes related to intervention. Before we can make sense of the complexities behind outcome decisions, we must discuss the different tools and approaches used for measurement.

All six studies assessed some aspect of impairment via standardized testing, and this ranged from extensive neuropsychological test batteries (Gray et al., 1992; Sohlberg & Mateer, 1987; Strache, 1987) to a small number of specific tests supplemented by speed and accuracy measurements of nonspecified tasks (Novack et al., 1996). Use of these cognitive measures also varied and ranged from pretreatment/posttreatment comparisons to repeat administration of specific tests over the span of treatment (Ponsford & Kinsella, 1988; Sohlberg & Mateer, 1987). Some studies used tests assumed to predict real world demands (Niemann et al., 1990). Other tests were selected specifically because they were similar to treatment tasks (Gray et al., 1992; Niemann et al., 1990). Four of the six studies used the neuropsychological test PASAT as part of their measurement tools (Gray et al., 1992; Niemann et al., 1990; Novack et al., 1996; Sohlberg & Mateer, 1987), although the manner of test delivery (e.g., taped presentation vs. live voice vs. revised tests) was not specified. Clear comparisons with other outcomes are further tempered by the fact that testing was incomplete (Novack et al., 1996) and that raw data were analyzed differently between studies (e.g., Gray et al., 1992; Sohlberg & Mateer, 1987). Other inconsistent use of impairment level testing was also evident. Studies differed in using parallel versions of repeated tests, including the use of versions supplied by the publisher (Sohlberg & Mateer, 1987) or created by the research administrators (Ponsford & Kinsella, 1988). One study created a unique way to score test data (ratios of accuracy scores to completion time) in an attempt to capture subjects' changes of performance that were not reflected in formal scoring (e.g., Novack et al., 1996).

Finding effective methods to measure relevant change poses a significant challenge. As discussed, we need to understand the relevance of neuropsychological (i.e., impairment level) testing for determining success or failure of intervention. The report of positive gains following direct attention training has been criticized and dismissed as treatment artifact by some authors (Park et al., 1999), whereas others (Gray et al., 1992; Sohlberg & Mateer, 1987) specifically chose neuropsychological measurements for their similarity to the training tasks and based their interpretation of findings on test score improvement. Additionally, philosophical differences are seen when results of cognitive tests are used to validate or reject the effectiveness of the training. Two studies (Gray et al., 1992; Niemann et al., 1990) reported or implied poor generalization of training skills when secondary tests did not improve over the pretreatment-posttreatment cycle. Sohlberg and Mateer (1987) viewed the absence of change in an untreated skill area (i.e., the double disassociation observed with unchanged visual-processing scores and improved attention scores) as positive proof of the effectiveness of attention skills training.

In addition to the impairment level testing, three studies incorporated activity/participation level information in their assessments. The Functional Independence Measure was used on a subset of one study (Novack et al., 1996). Ponsford and Kinsella (1988) created a rating scale of distractibility and also scored subject distractibility during an unstructured work task. Sohlberg and Mateer (1987) provided anecdotal information about community reintegration status after treatment was delivered.

Although it may be impossible to combine the outcomes into a binary decision on the efficacy and/or effectiveness of attention training, it is helpful to look at the outcomes of the individual studies. The acute rehabilitation studies (Novack et al., 1996; Ponsford & Kinsella, 1988) reported improvement in their subjects, but based on control group performance or subject changes during baseline phases, attributed it to spontaneous recovery, and not to treatment. Both studies' participation level testing failed to reflect benefit, although ceiling effect problems were reported for Ponsford and Kinsella's use of videotaped measurements.

Successful postintervention improvement was reported in the outpatient studies. Gray et al. (1992) reported their subjects showed improvement in storage and manipulation of numerical material in working memory, but the success emerged at follow-up testing only. They reported improvements in picture completion and speed of processing skills, but admitted that contributions of premorbid IQ and length of time since injury could not be ruled out. They also reported that performance decline in the control group could have artificially skewed the improvements of the experimental group. Strache (1987) reported that subjects demonstrated progress in concentration, psychomotor function, and intellectual and memory function, and reported even stronger memory improvement for a subset of subjects whose progression through their treatment hierarchy was contingent on their accuracy of performance, and not arbitrarily decided. Niemann et al. (1990) reported their subjects performed better on four measures of attention in their evaluation battery, including Trials B and a cancellation test. Sohlberg and Mateer (1987) reported successful gains by all subjects on the PASAT and anecdotal reports of success in returning to work or independent living.

Despite the successes of the four studies, impairment level improvement does not easily translate into clinically meaningful improvement. Even if impairment testing should prove to be ecologically valid in predicting improvements in independence or community reintegration, clinician judgment and client/family input would be required to determine if the cost-benefit ratio was acceptable. This posthoc information is not provided in the studies. Similarly, there was almost no attempt to measure maintenance of effects over time in any of the studies. Decisions about clinical relevance and further confidence in the success of the intervention would be easier if activity/participation-level assessment tools had been used.

Are There Methodological or Clinical Concerns?

We acknowledge that it is easier to "criticize than to do" and that all studies, regardless of flaws, contribute to the development of EBP. Nonetheless, savvy consumers must identify the strengths and weaknesses of the research as they consider incorporating new treatment ideas into their clinical practice.

We classified the six studies into three Class I (Gray et al., 1992; Niemann et al., 1990; Novack et al., 1996) and three Class II (Ponsford & Kinsella, 1988; Sohlberg & Mateer, 1987; Strache, 1987) studies. The Class I studies centered on pretreatment versus posttreatment measurement comparisons; although controls were used, none of the three had a "no-treatment" group. Novack et al. (1996) compared their experimental "focused stimulation group" with an "unstructured intervention program." Niemann et al. (1990) compared their experimental attention training group with a memory training group in a repeated measurement design, whereas Gray et al. (1992) compared a group who received computerized training to a group that used computers for recreation only. The Class II studies included a multiple-baseline, across-subjects design (Ponsford & Kinsella, 1988; Sohlberg & Mateer, 1987). Strache (1987) used two experimental groups and a control group that received "normal clinical intervention." The two experimental groups received additional computerized training but with different advancement criteria (arbitrarily determined vs. contingent on accurate performance). Subjects were assigned to the groups based on performance in "extensive diagnostic pretesting." In all the studies, the absence of a no-treatment group may undermine the rigor of the design, especially when questions of spontaneous recovery or treatment effects are raised.

All six studies referenced their attempts to control for the many variables that complicate clinical research for TBI subjects. Typical controls included gender, age, severity, education, premorbid IQ, and so forth. Time postonset was also routinely addressed, and markers ranged from weeks to months to years. As previously noted, the studies reported no improvements in attention training in the acute rehabilitation phase. Inspection, however, shows that one study (Ponsford & Kinsella, 1988) included acute care subjects who received intervention between 7–8.5 months following their injury. This time frame overlaps with that of subjects of at least two of the four postacute studies (Gray et al., 1992; Strache, 1987).

Further methodological anomalies are noted in the acute rehabilitation studies. Novack et al. (1996) were unable to collect baseline measurements on their subjects with the exception of two tests, for which they devised "new methods of scoring" to capture performance. Also, their participation level measurement, FIM, was collected in the middle of the intervention phase; it was unclear if the data were collected on 12 or 24 of the 44 subjects. Ponsford and Kinsella (1988) created a rating survey of attentional behaviors in daily activities and incorporated video-based analysis of distraction in a work module. Ceiling effects undermined the effectiveness of the video task, and no information is provided on the design and reliability of the rating scale. Measurement issues also arise when the absence of normative data is noted for the newly created parallel versions of one of their key measurements.

Finally, despite both studies' efforts at controlling for differences between treatment and control groups, information is not provided about the behavioral level or compliance of the subjects at the time of the intervention, although Ponsford and Kinsella (1988) imply that their subjects were no longer in posttraumatic amnesia. Even with controls for age, severity, and time postonset, participation in structured tasks could vary widely depending on the level of agitation, confusion, and fatigue. It is easy to imagine very different outcomes for subjects who received the same treatment but differed greatly in their motivation and potential for treatment participation.

Subject recruitment and participation issues must be addressed for the four outpatient studies as well. Although we are interested in attention training delivered to survivors of TBI, we must accept that researchers in only one of the studies used a subject pool comprised solely of persons with that etiology (Niemann et al., 1990). Subjects for the other studies included survivors of aneurysm or other CVAs (Gray et al., 1992; Sohlberg & Mateer; 1987; Strache, 1987), those with penetrating head wounds (Sohlberg & Mateer, 1987), and those who had other "neurosurgical procedures" (Gray et al., 1992). Non-TBI survivors provided 50% or more of the subject pools of the three studies. If we assume differences in presentation of injury, participation in therapy, and courses of recovery between the etiologies, one must acknowledge that evidence outcomes results may be skewed or at best may be difficult to interpret. Controls' age may also need to be considered. In Gray et al. (1992), the control group is slightly older, but more important, the standard deviation spread is significantly wider. Given the authors' interpretation that some positive results of the study may be caused by a decline in the control group's performance, this disparity must be acknowledged.

A disappointing methodological flaw present in all six reports reviewed in this subsection is the lack of inter- and intrarater reliability. This information is lacking in the scoring of objective and subjective assessment tools, the collection of data during treatment sessions, and judgments related to the interpretation of outcomes. Validity issues are broader in scope and more difficult to quantify, yet the absence of information from the authors is once again noted. To be valid clinical interventions, the treatment tasks should be replicable across settings. The six studies here differ in the details provided and range from a sentence-length description of the intervention (Novack et al., 1996; Strache, 1987) to a menu of treatment areas from which a subject's personal treatment plan was created (Sohlberg & Mateer, 1987). Replicability of the specifics of each of the studies would be impossible. Similarly, re-creating the dose (e.g., 120-min sessions in Niemann et al., 1990) or length of treatment (e.g., 9 weeks in Gray et al., 1992) for a client who was dependent on third-party payment of rehabilitation services might be difficult.

Are There Trends Across Studies Published Prior to 1999?

Our review, in addition to Cicerone et al.'s (2000) and Park and Ingles' (2001), suggests there is evidence in the literature of improvement in attention-based skills with direct training; however, the studies that reported improvements are open to interpretation. Exercises may promote the acquisition of specific skills and outcomes may be task-specific (Cicerone et al., 2000; Park & Ingles, 2001). The review of the older attention literature and the equivocal outcomes directed us to scrutinize specific intervention characteristics and outcomes in more recent efficacy articles. For instance, we looked to more contemporary studies to evaluate whether treatment effects were associated with incorporating strategy training in the attention remediation, or whether those studies that matched attention intervention tasks to individual subject profiles had different outcomes than the studies that delivered a standard program to every subject. Similarly, we were interested in whether more recent studies provided outcome reports that shed light on the task-specific nature of attention training.

Recent Attention Efficacy Studies

A literature search was conducted to identify articles published after 1999 (i.e., after the Cicerone et al., 2000 report) that evaluated the efficacy or effectiveness of attention training. Databases that were searched included PSYCH INFO, MEDLINE/PubMed, Eric, and CINAHL, using combinations of these key words: brain injury, closed head injury, attention, remediation, rehabilitation, and training. Twenty-seven articles were identified. These studies were reviewed and selected based on the following criteria: (a) written in English, (b) experimentally evaluated the direct training of attention remediation to adults, (c) excluded studies dealing with left hemispatial inattention, (d) subjects included people with traumatic brain injury, and (e) outcome data were reported in the study. This screening process revealed three studies: Cicerone, 2002; Park et al., 1999; Sohlberg et al., (2001) (see Table 3).

The first two authors independently reviewed the articles and coded them as Class II studies. The articles were reviewed using the five key questions. Again, there was high agreement between the two readers in the coding. There was discussion about how to describe particular features (e.g., whether outcomes were classified as impairment or participation), but the content of classification was consistent, and reliability was deemed acceptable. Tables 1–3 summarize literature according to the first four key questions. The fifth key question (Are there clinically applicable trends across the literature, is addressed in Table 4, with the generation of practice guidelines based on the evidence.)

Park, Proulx, and Towers (1999). These investigators evaluated the effectiveness of the commer-

TABLE 3. Recent studies (post-1999).

Reference	Parks et al., 1999 Class II	Sohlberg et al., 2001 Class II	Cicerone, 2002 Class II
Clients/Subjects			
Number of subjects	16	14	4
Controls	Culled from previously collected data. Selectively matched for age and education	Crossover design	4
Diagnosis/etiology	TBI	TBI, anoxia, tumor	TBI
Severity	Severe	LOC range: Null to 7 months	Mild
Age	m = 37.3 yrs (2.66 SE)	m = 33.1 yrs vs. 38.1 yrs	m = 31 vs. 34.75 yrs
Education	No reported difference between groups	11 yrs vs. 13 yrs	m = 15.25 yrs
Time postonset	Less than 1yr to 4 yrs	1 to 5 yrs	m = 8.25 months
Dual diagnosis/ co-morbidity	Not provided	3 with cd issues; 4 with mood medications	Exclusion for those with significant history
Cognitive profile postinjury	"Slightly above average"	Large range on neuropsychological testing	Impairments on 2/6 admit tests plus subjective complaint
Attention Training			
Focus	APT with hierarchical exercises	APT with hierarchical exercises	Working memory tasks combined with strategy training
Rationale	Repeated stimulation of attention will improve impaired systems	Repeated stimulation of attention will improve impaired systems	Teaching conscious use of strategies to boost allocation of resources and manage speed demands
Duration/frequency	40 hours (median) for 7.2 months (mean) "about 2 hours" per session	APT: 3 hrs/wk × 10 wks; education: 1 hr/ wk × 10 wks	60 mn/wk × 11–27 wks
Treatment setting	Not provided	University clinic	Outpatient clinic
Providers	Psychologist	Certified SLP or supervised graduate student	Not provided
Program individualiztion	Repetition of exercises when 3 or more errors. Adjunct counseling re: salient clinical issues	Tasks chosen to match attention profile	Timing and tasks varied re: client need
Strategy training or feedback	Yes.	No during APT; yes during brain injury education	Yes
Outcomes			
Impairment/psychom. change	No significant difference in attention or working memory improvements between 2 groups No change in depression.	Attention improved after APT (PASAT; Stroop and Trails b for low vigilence subjects). Significant Improvement for aspects of working memory	Three quarters of clients improved on attention tests

(continues)

TABLE 3. *(continued)*

Reference	Parks et al., 1999 Class II	Sohlberg et al., 2001 Class II	Cicerone, 2002 Class II
Participation level changes	Not provided	Memory/attention improvements per surveys and interviews	All of experimental group resumed vocation or social roles
Methodology review			
Study Design	Pre- postcomparison	Crossover within subject experimental design	Pre- postcomparison
Reliability	Not provided	Provided for rating scales and interviews	Not provided
Validity	Not provided	Not provided	Not provided

cially available Attention Training Program (APT) (Sohlberg & Mateer, 1987). Data were presented on 16 individuals with severe TBI who were beyond the period of spontaneous recovery. Subjects were identified by "specialists" as having a profile suggesting they would benefit from treatment. Descriptions of individual subjects were not provided.

Remediation consisted of structured APT exercises that were administered until a subject made no more than two errors. Program administration was standard, and exercises were not selected to match a subject's individual attention profile. Treatment included the provision of feedback about performance and discussion of error patterns. As the program proceeded, participants were also educated about different types of attention, and parallels between difficulties of daily living and problems performing particular APT exercises were highlighted. Subjects typically received twenty 120-min sessions spread over 29 weeks; thus, the training generally occurred less than once a week for more than 7 months.

Outcomes (question 3) were measured by comparing pre- and posttreatment performance on two neuropsychological tests (PASAT and Consonant Trigrams). The PASAT was used because it is assumed to be sensitive to attention impairments; however, the authors hypothesized that the recall measure of the Consonant Trigrams would not be affected by the types of attention addressed in the APT program. The Beck Depression Inventory was used to assess the impact of attention training on mood. Results showed that the TBI group who received attention training improved on both of the neuropsychological tests, but not on the Beck Depression Inventory. Measurement of maintenance of effects was not addressed.

The study design (question 4) compared pre- and posttraining test scores to test scores of age- and education-matched controls culled from data from an unrelated research project undertaken 11 years prior to the study. These control subjects were administered the two neuropsychological tests on two separate occasions over the course of a week instead of the 7-month interval of the subjects. The control group data revealed improvement on the PASAT but not on the Consonant Trigrams, whereas the subjects with brain injury who had received APT improved on aspects of both tests. Individual test performance data were not provided; thus, it is not possible to analyze performance within subjects. The authors interpret their findings to suggest that APT facilitated learning of specific skills, but not improvement of damaged attention functions. Reliability and validity concerns, and measurement issues, were not discussed.

Sohlberg, McLaughlin, Pavese, Heidrich, and Posner (2001). This group of investigators published an efficacy study of 14 postacute clients with mild-severe brain injuries, who exhibited impaired attention abilities as determined by neuropsychological evaluation. Group heterogeneity was reported, including histories of substance abuse, depression, and/or ongoing litigation.

Intervention consisted of APT attention exercises selected for each subject based on the results of their particular neuropsychological profile. For example, a subject displaying particular difficulty with sustained and selective attention worked on therapy tasks designed to target these areas at an initial level where he achieved 70–80% accuracy. Individuals received 24 hr of attention training administered in three 60-min sessions each week for a total of 10 weeks. Attention drills were grouped by specific attention abilities (e.g., sustained attention). Explicit performance strategies were not provided, and instructions or activities to foster generalization to real world tasks were not offered. Subjects also received a single 60-min session each week devoted to brain injury education for the same number of weeks as the APT attention drill work. Education consisted of a combination of topics selected from a menu of choices in combination with supportive listening (a "check-in" for how the week was going) and relaxation training.

Outcomes for the two intervention programs (APT and Brain Injury Education) were measured and compared using both impairment- and activity/participant-based measures. Impairment-level measures were obtained using a battery of neuropsychological tests selected to assess different attention networks (vigilance, orienting, working memory, and executive functions). Tests were administered before and after each of the two intervention phases. Activity/participation measures were obtained using standardized questionnaires and structured interviews to assess subjects' perceptions of their neuropsychological and psychosocial performance in daily life.

A crossover design was used, in which half of the subjects received the attention training prior to the Brain Injury Education and half of the subjects received the opposite order of treatment. This design allowed subjects to be used as their own controls. Based on responses to structured interview and performance on neuropsychological testing, improvement in complex attention abilities (e.g., working memory, alternating attention) were seen following APT. In contrast, there was little specific improvement in basic attention (e.g., vigilance or orienting abilities) following APT administration. Brain Injury Education was most effective in improving self-reports of psychosocial function. Improved PASAT scores were found to be correlated with self-reports of improved attention on structured interviews. It was noted that vigilance level influenced the improvement resulting from attention training, in that those clients with higher initial vigilance improved more on measures of executive attention. The authors interpreted their findings as suggestive that APT was effective in improving working memory or complex attention in clients with intact vigilance. They do not address other possibilities for improvements, such as subjects learning some type of behavior or skill from performing APT exercises that resulted in increased performance on specific neuropsychological tests.

Reliability and validity issues regarding the standardized questionnaires and structured interviews were indirectly and directly addressed. The three surveys administered to the subjects were from previously published articles and, in two of the three, answers were collected from the subjects and a member of their family. Specific information on the structure of the interviews, the transcription, coding by naïve readers, and data analysis are described.

Cicerone (2002). This investigator recently evaluated the effectiveness of an intervention designed to address attention deficits following mild traumatic brain injury (MTBI). Treatment participants consisted of a convenience sample of patients referred to a postacute brain injury rehabilitation program based on a diagnosis of MTBI. Four subjects received attention remediation and four served as a control group based on their inability to receive treatment. Neither criterion for MTBI nor individual subject data was presented. The subjects were matched closely for age, gender, education, and months postinjury, with all at least 3 months postinjury. All subjects had to meet criteria for significant impairment on two out of six attention measures; however, the treatment group participants were initially less impaired than the control group.

The treatment focus in this study varied from the other two intervention studies reviewed in this section. Similar to the Sohlberg et al. (2001) study, the author employed hierarchically organized attention remediation tasks targeting complex attention skills via working memory tasks that were tailored to match the specific attentional profiles of the individual clients. However, unlike both of the aforementioned studies, the focus was on using the attentional tasks as a method for training in the use of metacognitive strategies such as verbal mediation, rehearsal, anticipation of task demands, self-pacing, and self-monitoring. The intervention emphasized the conscious and deliberate use of such strategies to increase the participants' ability to allocate their attention resources and control the pacing of task performance. Hence, although there was repetitive administration of attention exercises, strategy training was a primary emphasis. The schedule of treatment was 1 hr per week for 11–27 weeks.

Treatment outcomes included neuropsychological measures, self-rating for perception of change, and informal reports of changes in status for vocational and social roles. Impairment-based measures compared pre- and posttreatment scores on a number of attention tests with the performance of the no-treatment control group. Results showed that three of the four participants improved significantly on the attention tests, but none in the control group did so. The treatment group also improved significantly on their self-report of a greater reduction of attention difficulties in comparison to no change in the control group. The author anecdotally reported that all of the treatment group participants returned to previous vocational and social roles, but none of the comparison group participants did so during the same period. Change was attributed to improved strategy use rather than improved underlying attentional processing, although there is no attempt to parse out these factors.

The study used a prospective, case-comparison design with groups of four individuals. Statistical analyses were thus performed on a very small sample. The author reported that the rating scale was developed for the study and that formal psychometric review had not been completed before use. Reliability of measurements was not discussed. Individual subject profiles were not provided; thus, it is difficult to analyze possible threats to internal validity. One threat to internal validity resulted from the fact that the treatment group performed better initially on attention measures, which could indicate they had more cognitive resources and, therefore, just needed the strategies to facilitate improvement.

Summary of Recent Efficacy Studies

Subject variability was quite large across studies. Park et al. (1999) stated that subjects had severe brain injuries, whereas the subjects in Cicerone's study (2002) were all diagnosed with MTBI. The subjects in the Sohlberg et al. (2000) study spanned the range from mildly to severely impaired. All subjects were reportedly beyond the period of spontaneous recovery. All three studies administered hierarchically organized attention drills, with two of them (Cicerone, 2002; Park et al., 1999) adding a strategy feedback component. One of the studies reported a low intensive therapy regime (less than once per week), with a protracted time of service (Park et al., 1999). In the other two studies, subjects were treated at least weekly, with the subjects in the Sohlberg et al. (2001) study receiving therapy 3 hr per week, whereas Cicerone (2002) treated subjects about 1 hr per week. These same two studies individualized the selection of attention tasks.

To measure outcomes, Cicerone (2002) and Sohlberg et al. (2001) used impairment- and activity-based measures, whereas Park et al. (1999) used impairment measures only. All three studies reported changes on attention tests in the group receiving attention training. The interpretation of these findings, however, differs widely between the three studies. Park et al. suggested that the changes demonstrated by their subjects were not significantly different from those in the nonbrain-injured control group and that the changes were most likely due to specific practice on tasks that resemble the outcome measures. Sohlberg et al. suggested that the profile of change on tests in the neuropsychological battery supports improved cognitive functioning, specifically in complex attention/executive functions and working memory processes. Cicerone claimed that the benefits of his treatment were due to participants' improved ability to compensate for residual deficits and adopt strategies for more effective allocation of their remaining attentional resources.

The two studies reporting more robust changes following treatment, including improvement in subjects' daily functioning (Cicerone, 2002; Sohlberg et al., 2001), shared the following features: (a) individualized attention exercises, (b) treatment sessions that were 1 hr (vs. 2 hr) in duration, (c) at least weekly treatment sessions, (d) outcome measures that included a range of different tests sensitive to attention and working memory, and (e) outcome measures that included activity-based measures using client self-report data. Additionally, examination of older literature in conjunction with this current literature suggests that the inclusion of strategy or metacognitive training, as part of direct attention training, increases treatment effectiveness.

WHAT HAVE WE LEARNED FROM THE LITERATURE?

The answer to this question lies in our interpretation of the evidence. Unfortunately, studies evaluating outcomes following cognitive intervention are not as straightforward as pharmacologic studies. The heterogeneity inherent in the TBI population, coupled with the strengths and limitations unique to each setting and practitioner and the range of opinions regarding what constitutes meaningful change, makes it difficult, perhaps impossible, to design studies with clean, unequivocal outcomes. It is hoped that because this article has been written by a committee of researchers and clinicians incorporating a broad range of perspectives, the bias that occurs when interpreting evidence will be tempered. In general, we feel confident in our assessment that certain aspects of attention training are helpful in improving attention performance in some adults with TBI.

Recognizing the difficulty inherent in interpreting the evidence, our task has been to examine the attention training literature for evidence that responds to the question "When does attention training facilitate the greatest change and for whom?" We conclude with a set of recommendations for implementing direct attention training. Practice guidelines are recommendations for patient management reflecting moderate clinical certainty, usually evidence from Class II experiments or a strong consensus of Class III evidence (Miller, Rosenberg, Gelinas, & the ALS Practice Parameters Task Force, 1999). The suggestions offered in Table 4 were generated from our review of the attention training literature. Given the uneven and incomplete nature of the experimental literature, individual studies addressed different clinical practice questions. We selected aspects of our five key questions that we believe are sufficiently supported by evidence from one or more Class II studies to encourage clinicians to adopt a particular clinical practice. Table 4 should be viewed as our interpretation of the current literature in conjunction with our own collective experience as cognitive rehabilitation practitioners.

Examining the literature for evidence that reveals the source of observed changes in attention performance highlights the interpretation challenge. For example, Park et al. (1999) hypothesized that the improved performance on the Consonant Trigrams test suggested changes in attention behaviors or skills rather than improved cognitive processing. An alternative explanation may be that this recall measure requires working memory, which had improved as a result of attention training. Conversely, Sohlberg et al. (2001) claimed their training resulted in improved processing; however, it is plausible that the training may have in part encouraged adoption of an assertive attitude or a reduction in anxiety while performing the more complex tests and daily living tasks (Fasotti, Kovacs, Eling, & Brouwer, 2000). In short, the question of why some individuals' performance changes with the attention training remains unanswered.

FUTURE RESEARCH DIRECTIONS

Our review process exposed and educated our committee on the challenges to writing EBP as well as illuminated future research directions. One of the difficulties we encountered was how to code the relevant features of the efficacy research. We formulated our committee with the notion of assembling expert reviewers who both provide cognitive rehabilitation services and who conduct research in the field. Another option would have been to use blind reviewers to code the studies in order to achieve more objectivity in the review process. Instead, we elected to use discussion and consensus to identify and code relevant research features. Further, bias was possible by having reviewers who have contributed to the attention research literature and who have intellectual ownership of the intervention under question. However, the committee approach to this project provided checks and balances to guard against this type of bias. There are pros and cons of relying on "front-line" experience versus objective appraisal.

A further limitation of our review comes from summarizing rather than providing the details of some of the important methodological research issues. For example, we chose not to discuss in detail the specific types of reliability and validity that were lacking in studies. This decision was made due to space considerations and a desire to discuss a broad range of research issues.

We look forward to continued research that facilitates efforts to develop evidence-based guidelines for attention training. The most outstanding need revealed by our work is to develop methods that measure the impact of attention impairments on daily life for individuals across the lifespan. The relationship between neuropsychological tests and attention as deployed during real world activities is not clear. The increased availability of functional brain imaging may help identify neuropsychological circuits that are affected by training, but again, we need to understand how these circuits relate to the performance of functional activities. It may be fruitful to study alternative assessment paradigms used in related fields such as the functional assessment model described in special education research and practice (e.g., Lucyshin, Albin, & Nixon, 1997) or interpretive research methods adapted to evaluate the impact of impairments in an individual's daily life (e.g., Simmons-Mackie & Damico, 1996).

Our review process highlights the need for research that better describes the specific elements of attention training that are most effective in particular contexts and the outcomes that result from such training. For example, the knowledge gained from our review encourages future studies that in-

TABLE 4. Clinical recommendations based on the review of literature, organized by key questions 1 through 3.

Key Question	Practice Recommendations	Summary of Evidence	Clinicians Should
1. Who is a good cadidate for direct attention training?	**Guideline** for postacute or mildly injured clients, with intact vigilance	Two Class I and four Class II studies with descriptions of participants	Scrutinize candidacy and monitor responses to training
	Insufficient evidence to make recommendation for clients at acute phase of recovery	One Class I and one Class II studies, but with questionable internal validity (incomplete data, unreported variable controls, etc.)	Scrutinize candidacy. Monitor responses to training and know that observed improvements may in part be a result of spontaneous recovery
	Unknown for use with clients with severely impaired vigilance	Evidence provided by incomplete acute care studies (one Class I and one Class II).	Be cautious and aware of uncertainties of outcome. Proceed on case-by-case basis
2. What are the critical features of direct attention training?	**Guideline** for using direct attention training in conjunction with metacognitive training (feedback, self-monitoring, & strategy training)	Two Class I and one Class II studies	Use attention training in combination with self-reflective logs, anticipation/prediction activities, feedback, & strategy training
	Guideline for program individualization	Three Class II studies	Identify client strengths and needs prior to treament; select exercises to address specific areas of weakness
	Guideline for treatment frequency	Two Class I and four Class II studies	Administer treatment at least once per week
	Guideline for complex attention tasks	Four Class II studies	Use a hierarchy of tasks that emphasize working memory, mental control, and selective, alternating, and/or divided attention
	Unknown for improving vigilance or reaction time	One Class I and one Class II studies with acute participants as described above	Be cautious when using remediation programs that focus on simple vigilance or reaction time
3. What outcomes can you expect from direct attention training?	**Guideline** for obtaining task specific, impairment level outcomes	Two Class I and four Class II studies	Identify desired outcomes. Measure performance
	Unknown for obtaining generalization to untrained, impairment level tasks	One Class I and two Class II studies but striking differences in results interpretation	Identify desired outcomes. Measure performance
	Uncertain for obtaining generalization to participant level tasks	Criterion-referenced outcomes in three Class II studies	Identify desired outcomes. Use methods that can reliably measure clinically meaningful progress

vestigate the effects of combining strategy-based training and direct attention training. More controlled studies with larger numbers of participant groups that are carefully described are also critical. The use of control groups receiving serial measurements without any treatment would strengthen our understanding of intervention effects, as would the use of examiners who are blind to study designs and participant group assignment. Controlled studies would help sort out questions such as the effectiveness of attention training for acute patients.

We recognize the challenge of formulating treatment groups within the brain injury population. Looking to populations that share similar features with the brain injury population may mitigate this research challenge. For example, a recent study evaluated the effects of attention training in a pediatric population with attention deficits as a result of brain radiation for cancer treatment (Butler & Copeland, 2002). Their results were consistent with a number of studies reviewed for this article and showed significant changes on neuropsychological tests measuring attention following attention training, but the changes did not generalize to a functional academic task. Widening our search to include other populations such as individuals with developmental attention deficits may be productive.

It is our hope that summarizing future research using the five key questions presented in this article may provide an organizational framework that will allow clinicians to make decisions that are empirically supported.

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