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# Effectiveness of Attention Rehabilitation After an Acquired Brain Injury: A Meta-Analysis

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The efficacy of attention rehabilitation after an acquired brain injury was examined meta-analytically. Thirty studies with a total of 359 participants met the authors' selection criteria. Studies were categorized according to whether training efficacy was evaluated by comparing pre- and posttraining scores only or included a control condition as well. Performance improved significantly (using the $d_+$ statistic) after training in pre–post only studies but not in pre–post with control studies. Further analyses showed that specific-skills training significantly improved performance of tasks requiring attention but that the cognitive-retraining methods included in the meta-analysis did not significantly affect outcomes. These findings demonstrate that acquired deficits of attention are treatable using specific-skills training. Implications of these results for rehabilitation theory and future research are discussed.

Rehabilitation of cognitive deficits after an acquired brain injury has long preoccupied neuropsychologists and others concerned with the relation between brain and behavior. Prompted by the dramatic increase in the number of brain injuries during World Wars I and II, eminent scientists such as Goldstein, Luria, and Zangwill developed treatment programs for soldiers and others who had sustained brain damage (Goldstein, 1942; Luria, 1963; Luria, Naydin, Tsvetkova, & Vinarskaya, 1969; Zangwill, 1947). Each of these investigators was also concerned with the issue of how to proceed with rehabilitation. As Goldstein (1942) put it, "Should we simply help the patient to regain his lost performance capacity, to use it in the same way as he did before the injury, or should he learn to compensate with other performances?" (p. 147). In other words, should rehabilitation aim to restore a damaged cognitive function or to develop compensatory or alternative ways of performing tasks?

Later, in the 1970s, Ben-Yishay and colleagues proposed that deficits of attention were a common consequence of head injury that greatly impeded the recovery of other cognitive and functional abilities, and they were the first researchers to develop a series of specific exercises to retrain attentional skills (Ben-Yishay, Piasetsky, & Rattock, 1987; Ben-Yishay, Rattock, & Diller, 1979a, 1979b). The high frequency of attention deficits after brain damage (Lezak, 1995), particularly after traumatic brain injury (TBI; Cohadon, Richer, & Castel, 1991; Gronwall, 1987; Van Zomeren, 1981; Van Zomeren, Brouwer, & Deelman, 1984), and the importance of remediating attention are now widely recognized. The degree of impairment on attention tasks appears to predict the likelihood of returning to work and the performance of other functionally significant activities (Brooks, 1987; Van Zomeren & Van Den Burg, 1985).

At present, rehabilitationists generally use one of two approaches to treat attention deficits after brain injury. The treatment approach that has been evaluated most often in the published literature attempts to directly retrain the damaged cognitive function. The underlying rationale for this approach is the notion that practice on carefully selected exercises promotes recovery of damaged neural circuits and restores function in the impaired attentional processes themselves. The tasks mediated by those circuits are then performed in a way that is similar to non-brain-damaged individuals (Mateer & Mapou, 1996; Robertson & Murre, 1999).

Attention retraining typically requires participants to complete a series of repetitive exercises or drills in which they respond to visual or auditory stimuli, often classifying items on the basis of a rule. For example, in Sohlberg and Mateer's (1987) Attention Process Training program, one of the simpler tasks requires participants to press a buzzer whenever they hear the number 3. In another more difficult task, the months of the year are presented one at a time, and participants must press a buzzer whenever the month just presented is the same month that immediately preceded the month presented one back. Each task generally takes a few minutes to complete, feedback is given on completion of the task, and easier exercises are provided before more difficult

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ones. Treatment is usually considered to be successful when improvement is observed on psychometric tests of cognitive function.

Many of the investigators who have developed and evaluated attention retraining programs work in the tradition of cognitive or neuropsychological rehabilitation. This emerging discipline emphasizes the notion that treatment should be informed by the theoretical ideas of cognitive neuroscience. The idea that attention is not unitary but consists of a number of distinct components (Posner & Petersen, 1990) has been incorporated into many of the attention retraining programs (Gauggel & Niemann, 1996; Novack, Caldwell, Duke, Bergquist, & Gage, 1996; Park, Proulx, & Towers, 1999; Sohlberg & Mateer, 1987; Sturm & Willmes, 1991; Sturm, Willmes, Orgass, & Hartje, 1997), in that tasks requiring different types of attention are provided. For example, the Attention Process Training program includes tasks of sustained, selective, alternating, and divided attention (Sohlberg & Mateer, 1987). This approach may be important for treatment because it has been claimed that specific components of attention require specific training in order for improvement to occur (Sturm & Willmes, 1991; Sturm et al., 1997).

An alternative and much less frequently studied approach to rehabilitation attempts to assist people with attention deficits by having them learn or relearn how to perform specific skills of functional significance. The underlying rationale for this approach is the notion that through carefully structured practice of a specific skill that is impaired as a result of brain damage, it is possible for individuals to compensate and develop alternative neuropsychological processes that rely on preserved brain areas to improve performance of the skill (Backman & Dixon, 1992). According to this approach, brain-damaged individuals learn to perform the given skill in a way that is different than non-brain-damaged individuals. In the present meta-analysis, all studies adopting this approach focused on tasks that were hypothesized to require attention, and attention was trained either concurrently with or in the context of the specific skills.

The specific-skill approach to rehabilitation often applies behavioral principles and is sometimes influenced by a neuropsychologically based understanding of the deficit being treated. For example, Kewman et al. (1985) attempted to improve the driving skills of a group of people with brain injuries. Driving was conceptualized as a complex skill that critically requires attention to keep track of many things at once and to rapidly and flexibly shift focus from one activity to another. The experimental group completed a series of carefully designed driving-related exercises, some requiring divided attention (i.e., auditory or visual monitoring tasks were completed while driving), using a small electric-powered vehicle. The training was accomplished by a shaping procedure. Brain-injured control participants drove the vehicle for the same amount of time as the experimental participants but were not trained in the specific exercises. Effectiveness of rehabilitation was evaluated by having both groups take tests of on-the-road automobile driving before and after training.

The present article is the first to use meta-analytic procedures to review the attention rehabilitation literature. One objective of the analysis was to evaluate the efficacy of the existing programs that have attempted to directly retrain attention in individuals with acquired brain injuries. The reported effects have varied greatly across studies, and conclusions about the efficacy of these programs have ranged from fairly positive (Mateer & Mapou, 1996), to cautiously positive (Robertson, 1990), to somewhat negative (Ponsford, Sloan, & Snow, 1995).

A second objective was to identify methodological factors that may contribute to this variability in improvement. A substantial number of studies did not include appropriate controls, and despite discussion of the need for methodological rigor in rehabilitation research (e.g., Robertson, 1994; Willmes & Deloche, 1997), such studies continue to be published and cited in reputable journals, as well as applied in clinical practice. We were particularly concerned that in studies without control groups, practice on the outcome measures themselves, not training, might contribute to the improvement in posttraining performance. We investigated the importance of this factor by comparing studies that measured improvement on the basis of the performance difference between pretraining and posttraining with no control for practice effects (pre–post only) with studies that controlled for practice (pre–post with control). We also compared the pre–post only and pre–post with control measures of improvement in those studies in which it was possible to determine both types of effect size estimates. We reasoned that if improvement in performance after training was entirely a consequence of practice on the psychological test, performance should not improve after training when assessed by a pre–post with control measure, and the improvement in performance after training, assessed by a pre–post only measure, would provide an estimate of the magnitude of the practice effect.

Comparison of pre–post only and pre–post with control estimates of improvement also provided an opportunity to compare the differing predictions of the specific-skill and direct-retraining approaches. According to the specific-skill hypothesis, practice on the outcome measures themselves should improve performance; hence, the pre–post only estimates of improvement should be positive. However, because this hypothesis postulates no improvement in cognitive functioning, performance should not improve after training when assessed by pre–post with control measures, provided the exercises performed during training do not resemble the outcome measures. Performance should improve though if the training and outcome measures are similar. In contrast, the direct-retraining hypothesis predicts that pre–post with control measures of improvement should be positive, provided the training is effective and the cognitive function targeted during training is required when the outcome task is performed.

The final objective of the meta-analysis was to evaluate whether the direct-retraining and specific-skill programs differed in their effectiveness. This question has not been systematically addressed in any review of the attention rehabilitation literature, although it is clearly an important clinical issue, and its consideration may contribute to efforts to develop a better conceptual understanding of rehabilitation (e.g., Baddeley, 1993a; Caramazza & Hillis, 1993; Plaut, 1996).

## Method

### *Sample of Studies*

Studies were identified through computerized searches of MEDLINE (1966–June 1997) and PsycLIT (1974–June 1997) using combinations of the following descriptors: *stroke, cerebrovascular disorders, head trauma, head injury, brain trauma, brain injury, brain damage, rehabilitation, retraining, remediation, attention, concentration, perception, cognition,* and *neuropsychology.* We also searched Science Citation Index for articles that referred to several well-known rehabilitation studies (Ben-Yishay et al., 1979a; Gray & Robertson, 1989; Gray, Robertson, Pentland, & Anderson, 1992; Sohlberg & Mateer, 1987). The reference section of each retrieved study was also inspected to identify additional articles.

To be included in this meta-analysis, articles had to evaluate the effectiveness of interventions for attentional disorders following brain damage, and the treatment had to involve practice performing either cognitive exercises or specific skills that critically require attention. We relied on the claims of the individual studies themselves that they were attempting to remediate attention because a number of the relevant studies did not provide sufficient detail to allow us to determine the specific nature of the attentional disorders and treatment methods. All but two of the studies explicitly stated that this was a primary goal. These additional two articles (Hajek, Kates, Donnelly, & McGree, 1993; Sivak, Hill, & Olson, 1984) were included because both used a training program that was commonly used in the other reviewed studies (Bracy, 1983). Other studies (including the most well-known articles cited above) discussed the nature of the attentional deficits and treatments in more detail. On the basis of the descriptions in these other studies, and building on the definition of Posner and Petersen (1990), we derived the following definition of *attention* that is generally consistent with its usage in these articles. Attention refers to the voluntary control over more automatic brain systems so as to be able to select and manipulate sensory and stored information briefly or for sustained periods of time. Consistent with previous qualitative reviews (Mateer & Mapou, 1996; Robertson, 1990), we excluded studies that focused specifically on the treatment of hemi-inattention, because the symptoms, underlying processes, and treatment approaches differ from this more general type of attentional disorder.

In addition, articles had to satisfy the following criteria: (a) Participants were adults with an acquired brain injury following a stroke, TBI, or surgical lesion; (b) the specific effects of the attentional intervention could be determined when the treatment was part of a more comprehensive rehabilitation program; (c) at least one quantitative outcome measure was used, and results had sufficient detail for effect size estimates to be computed; and (d) the outcome measures had to differ from the training measures. Six studies were excluded on the basis of this latter criterion (Carter, Howard, & O'Neil, 1983; Ethier, Baribeau, & Braun, 1989; Ethier, Braun, & Baribeau, 1989; Grinspun, 1987; Klavora et al., 1995; Miller, 1980). This review process left us with a sample of 30 studies (denoted by asterisks in the reference list) that involved a total of 359 participants and yielded 481 effect size estimates.

### *Variables Coded From Studies*

Each study was coded according to whether the intention of the study was to directly retrain attention or to improve a specific skill. To qualify as a direct-retraining study, practice performing a series of repetitive, attention-demanding exercises had to be provided. Several studies used exercises from commercially available cognitive-retraining programs, such as Attention Process Training (Sohlberg & Mateer, 1986), THINKable (Ruff et al., 1994), or a series of programs developed by Bracy (1983; Chen, Thomas, Glueckauf, & Bracy, 1997). To qualify as a specific-skill study, practice performing a functional skill, or a closely related skill, that critically required attention (e.g., driving) had to be provided. Attention was then trained either within the context of that skill (e.g., Kewman et al., 1985) or concurrently with skill training (e.g., Carter, Oliveira, Duponte, & Lynch, 1988). Twenty-six studies were coded as direct-retraining studies, and 4 studies were coded as specific-skill studies (Carter et al., 1988; Kewman et al., 1985; Sivak, Hill, Henson, et al., 1984; C. Wilson & Robertson, 1992).

The direct-retraining studies used psychometric tests of cognitive ability as the primary outcome measures. We grouped these tests into three broad categories: attention, learning and memory, and other cognitive tests.[1] Because attention is often thought to consist of a number of distinct components, with treatment improving some components but not others (Sturm et al., 1997), we further categorized tests of attention on the basis of factor analytic research (Mirsky, 1989; Mirsky, Anthony, Duncan, Ahearn, & Kellam, 1991). The factors of attention and the associated tests (shown in parentheses) were (a) focus/execute, which included tests of perceptual or motor speed (Trail Making Test [Reitan & Tarshes, 1959], Letter Cancellation Test [Talland, 1965], Digit Symbol Substitution [Wechsler, 1955], and Stroop Color and Word Test [Golden, 1978]); (b) sustain, which included tests of vigilance or sustained concentration (continuous performance tests); and (c) encode, which included tests of short-term storage or numerical manipulation of information (Digit Span forwards and Arithmetic from the Wechsler Adult Intelligent Scale—Revised [WAIS–R; Wechsler, 1981]).[2] Variants of these tests were also classified using the three factors.

The following additional categories were formed to accommodate the other tests of attention: (a) working memory, which shares important similarities with attention (Baddeley, 1993b; Digit Span backwards from the WAIS–R and Paced Auditory Serial Addition Test [PASAT; Gronwall, 1977]); (b) Picture Completion (WAIS–R subtest), which may assess attention to visual detail (Gray et al., 1992); (c) Mental Control (Wechsler Memory Scale—Revised subtest; Wechsler, 1987); (d) simple reaction time (RT); (e) choice RT, which may measure aspects of attention (e.g., Stuss et al., 1989; Van Zomeren & Brouwer, 1994; but also see Ponsford & Kinsella, 1992); and (f) other, which included infrequently used measures (e.g., divided attention and time estimation). A few direct-retraining studies also measured participant-reported mood with depression or anxiety inventories, as well as subjective ratings of attention on a particular task by participants, other observers, or both.

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[1] The categorization of all outcome measures can be obtained from Norman W. Park.

[2] The final factor in Mirsky's (1989; Mirsky et al., 1991) analysis, flexibility, measured by the Wisconsin Card Sorting Test (Berg, 1948), was not included as a measure of attention because this task is commonly considered to measure other abilities as well, including concept formation and reasoning (Lezak, 1995).

The specific-skill studies evaluated whether performance of functional skills improved after training. The outcome measures assessed activities of daily living (ADL; i.e., functional status in different self-care categories), automobile driving (e.g., ratings by a driver educator), and attention behavior on a particular task (e.g., frequency of attention slips in reading). The type of effect size (pre–post only or pre–post with control), the characteristics of participants (age, time postevent, etiology, and severity of brain damage), and the characteristics of training (number of hours, setting, modality, content, speed, difficulty, administration mode, and feedback) were also coded for each study.

### *Quantifying Study Outcomes*

The effect size index calculated was Hedges's (1982) $g$, the difference between two means divided by the appropriate standard deviation. DSTAT, a software package developed to assist in meta-analytic calculations (Johnson, 1989), was used when appropriate. Positive effect sizes indicated improved performance following training; negative effect sizes indicated worse performance. Calculations based on the pre- and posttreatment means required the standard deviation of the difference scores. When this value could not be calculated directly from the available data, the following standard formula was used: $s_g = (s_x^2 + s_y^2 - 2r_{xy}s_xs_y)^{1/2}$, where $s_g$ is the standard deviation of the difference score, $s_x^2$ is the variance of pretraining scores, $s_y^2$ is the variance of posttraining scores, and $r_{xy}$ is the correlation between pre- and posttraining scores. Because this correlation was often not reported, it was estimated either from treatment studies that had used the same measure and reported sufficient data for its derivation or from an estimate of test–retest reliability for that measure from the literature (for a similar sample and time interval when available). For studies that included both treatment and control groups, the pretraining effect size was subtracted from the posttraining effect size (Wortman, 1994).

In the four single-case studies (Bracy, 1983; Franzen & Harris, 1993; Podd & Seelig, 1992; C. Wilson & Robertson, 1992), we obtained a proxy estimate of the standard deviation of the population because that estimate could not be obtained from the data.[3] In three single-case studies that used psychometric tests as outcome measures, the standard deviation was derived from published norms. In a fourth study, the standard deviation was estimated from a large number of behavioral observations ($n$ = 48), obtained over several weeks, which were shown to be nonserially dependent (C. Wilson & Robertson, 1992).[4]

### *Analysis of Effect Sizes*

The $g$ statistics were converted into $d$ statistics to avoid bias (Hedges & Olkin, 1985). Aggregate results were reported in terms of weighted mean estimates ($d_+$), calculated by weighting each $d$ by the reciprocal of its variance. Thus, more reliable effect size estimates were weighted more heavily (Hedges & Olkin, 1985). A 95% confidence interval (CI) around the mean was included to provide an estimate of the variability of $d_+$ and to test whether $d_+$ was statistically different from zero.

A homogeneity statistic, $Q$, which has an approximate chi-square distribution with $k - 1$ degrees of freedom, where $k$ is the number of effect sizes, was also calculated to determine whether the values of $d$ used to calculate a mean effect size were consistent within the set. Heterogeneity was indicated when the $Q$ statistic had a large, statistically significant value, suggesting that one or more features that were present in some studies and absent in others were affecting the magnitude of the effect sizes. When $Q$ is statistically significant, investigators typically perform post hoc analyses to try and identify which attribute or attributes are contributing to the heterogeneity. Rosenthal (1991) pointed out that a $Q$ statistic is an omnibus statistical test and recommended additional analyses (particularly to test a priori hypotheses), even when the $Q$ statistic is not statistically significant, to determine whether the magnitude of the effect sizes covaries with some attribute or attributes of the study. The relation between study attributes and effect size estimates was investigated using fixed-effect categorized models (Hedges, 1994; Hedges & Olkin, 1985). These models partition the total variability in the effect size estimates into the variability between classes or attributes ($Q_B$) and the variability within each class ($Q_W$). When $Q_B$ is statistically significant, it indicates that the attribute partitioning the data significantly determines the effect size; $Q_W$ can be used to test whether the effect size estimates within that class are homogeneous.

## Results

### *Publication Characteristics*

The mean publication year was 1990 ($SD$ = 4.3), and half of the studies were published since 1991. The majority of studies (57%) were completed in the United States, and most of the remaining studies were completed in Canada, the United Kingdom, or Germany.

### *Participant Characteristics*

In 57% of the studies, all participants in the treatment group were TBI survivors; in 13% of the studies, all participants were stroke survivors; and in the remaining 30% of the studies, the participants were of mixed etiology (but included TBI survivors in all but one study). In studies with TBI survivors, the mean age was 29.5 years ($SD$ = 4.7 years); in studies with stroke survivors, the mean age was 54.3 years ($SD$ = 17.9 years); and in studies with mixed groups, the mean age was 38.4 years ($SD$ = 15.5 years). From the available information, it appeared that many of the TBI survivors had a severe injury on the basis of the duration of posttraumatic amnesia ($M$ = 44.7 days, $SD$ = 27.9 days, $n$ = 4) and the duration of coma ($M$ = 29.8 days, $SD$ = 22.8 days, $n$ = 10).

### *Training Characteristics*

In 55% of the studies, treatment was provided at least 1 year postinjury ($M$ = 846 days, $SD$ = 1,135 days), and in 64% of the studies, treatment was provided on an outpatient basis. Treatment was administered for a mean of 31.2 hr ($SD$ = 32.7 hr). In 50% of the studies, training was administered, at least in part, by computer. Sixty-seven percent of the studies included both auditory and visual exercises.

[3] A few studies included in the meta-analysis presented results from similar treatments as a series of single-case studies (e.g., Gray & Robertson, 1989; Sohlberg & Mateer, 1987). In these studies, we aggregated results across the single cases so as to be able to directly estimate the effect size from the data in the study.

[4] Analyses showed that effect size estimates for the single-case studies did not differ significantly from those for the group studies.

Table 1
*Performance Improvement After Training in Different Cognitive Functions and Skills Assessed by Pre–Post Only and Pre–Post With Control Measures*

| | Pre–post only | | | Pre–post with control | | |
|---|---|---|---|---|---|---|
| Outcome measure | $d_+$ | 95% CI | Number of cases ($k$) | $d_+$ | 95% CI | Number of cases ($k$) |
| Attention | 0.68 | 0.50–0.86 | 20 | 0.15 | −0.08–0.37 | 12 |
| Learning and memory | 0.35 | 0.06–0.64 | 12 | 0.05 | −0.23–0.33 | 7 |
| Other | 0.43 | 0.15–0.71 | 11 | 0.08 | −0.23–0.40 | 5 |
| ADL | 1.01 | 0.23–1.80 | 2 | 0.49 | −0.71–1.70 | 1 |
| Driving | 1.96 | 1.22–2.70 | 2 | 1.15 | 0.28–2.02 | 1 |
| Attention behavior | 0.62 | −0.46–1.70 | 2 | 1.01 | 0.08–1.94 | 1 |
| Attention rating | — | — | — | 0.45 | −0.23–1.13 | 2 |
| Mood | 0.26 | −0.33–0.84 | 3 | — | — | |

*Note.* Dashes indicate that data are not available. CI = confidence interval; ADL = activities of daily living.

Eighty-three percent of the studies specified that the tasks were graduated in difficulty, and 77% of the studies indicated that feedback on training performance was provided. The number of training tasks varied considerably ($M = 7.7$, $SD = 8.7$, $n = 23$), with half of the studies providing five or more tasks. Almost all of the training programs (89%) had a component in which speeded or paced performance was encouraged.

### *Effects of Training on Different Cognitive Functions and Skills*

To determine whether rehabilitation improved performance on the different measures of cognitive function and skills, the *g* scores for all outcome measures of the same category (e.g., psychometric tests of attention) and type of effect size (pre–post only and pre–post with control) were averaged within a given study. These aggregated effect size estimates were then converted into $d_+$ statistics. Table 1 displays the results of this analysis. $Q_W$ was nonsignificant in all conditions. Results showed that all measures of cognitive function (attention, learning and memory, and other tests) improved significantly when assessed by the pre–post only type of estimate because the lower limits of the 95% CIs for $d_+$ were greater than zero (Hedges, 1994; Shadish & Haddock, 1994). However, no measure of cognitive function improved significantly when measured by the pre–post with control estimates (i.e., the lower limits of the 95% CIs for $d_+$ fell below zero). The finding that the pre–post only estimates were substantially larger than the pre–post with control estimates may be attributed to practice effects inflating the pre–post only measures.

The pre–post with control effect size estimates for the different measures of specific skills (i.e., ADL, driving, and attention behavior) were all greater than 0.49, and driving and attention behavior improved significantly after training. In contrast, the pre–post with control estimates for the different measures of cognitive function were all less than 0.15, and in no condition was the improvement significantly different from zero. Attention ratings and measures of mood also did not improve significantly after training. The latter result is of particular interest because it suggests that rehabilitation on its own does not act as a "placebo" to improve emotional functioning and that improvements in other outcome measures are therefore not likely due to underlying changes in mood.

### *Effects of Training on Measures of Attention*

To determine whether rehabilitation improved performance on specific measures of attention, we averaged across all tests of the same measure (e.g., focus/execute) and type of effect size (pre–post only and pre–post with control) within a given study. Table 2 displays the results of this analysis. $Q_W$ was nonsignificant in all conditions. Seven of the nine estimates of improvement in the pre–post only condition were significantly greater than zero, whereas none of the estimates in the pre–post with control condition were significantly greater than zero. Thus, as in the previous analysis, the pre–post only estimates of improvement were substantially larger than the pre–post with control estimates.[5]

### *Analysis of Differences in Pre–Post Only and Pre–Post With Control Measures*

To investigate the reasons for the substantial differences between the pre–post only and pre–post with control estimates of improvement, we examined the eight studies in

[5] In most studies included in the meta-analysis, the posttraining measures were administered shortly after completion of treatment. However, a few studies administered a delayed follow-up assessment to examine the stability of the training effects. To determine whether the pattern of improvement changed after a posttraining delay, we compared the controlled direct-retraining studies that administered outcome measures only immediately after training ($k = 10$) with those that included a delayed follow-up assessment (Gray et al., 1992; Wood & Fussey, 1987). The immediate versus delayed analysis of each cognitive function and specific measure of attention showed that none of the $Q_B$ or $Q_W$ statistics were statistically significant.

Table 2
*Performance Improvement After Training in Specific Measures of Attention Assessed by Pre–Post Only and Pre–Post With Control Measures*

| | Pre–post only | | | Pre–post with control | | |
|---|---|---|---|---|---|---|
| Type of attention | $d_+$ | 95% CI | Number of cases ($k$) | $d_+$ | 95% CI | Number of cases ($k$) |
| Focus/execute | 0.56 | 0.33–0.78 | 12 | 0.22 | −0.03–0.47 | 10 |
| Sustain | 0.36 | −0.01–0.73 | 3 | 0.10 | −0.38–0.59 | 3 |
| Encode | 0.47 | 0.07–0.86 | 6 | 0.32 | −0.06–0.57 | 5 |
| Working memory | 0.78 | 0.43–1.13 | 7 | 0.12 | −0.20–0.43 | 5 |
| Picture Completion | 0.76 | 0.45–1.07 | 5 | 0.12 | −0.35–0.58 | 2 |
| Mental Control | 0.18 | −0.42–0.77 | 2 | −0.02 | −0.48–0.44 | 2 |
| Simple RT | 0.89 | 0.57–1.21 | 3 | 0.17 | −0.21–0.56 | 4 |
| Choice RT | 0.60 | 0.20–1.00 | 4 | 0.13 | −0.37–0.63 | 3 |
| Other | 0.79 | 0.47–1.10 | 6 | 0.19 | −0.18–0.57 | 4 |
| *M* | 0.60 | | | 0.14 | | |

*Note.* CI = confidence interval; RT = reaction time.

which both types of estimate could be obtained. We hypothesized that if the differences between the estimate types were attributable to practice, the results within these selected studies should be similar to the overall pattern reported thus far. If, in contrast, there was some systematic difference between the pre–post only studies and the pre–post with control studies, such as in the nature of training or another factor, the results within this subset of studies should differ from the overall pattern. The effects of training on the different cognitive functions and skills and specific measures of attention were examined within these eight studies, and these results were compared with those previously reported for the entire study sample.

The effects of training on different cognitive functions and skills for the subset of eight studies were similar to those reported in Table 1 in that the pre–post only weighted effect size estimates (shown first) were greater than the corresponding pre–post with control estimates (shown second) for all available outcome measures ($d_+$ values for measures of attention: 0.61 vs. 0.27, learning and memory: 0.33 vs. −0.02, other tests: 0.56 vs. 0.12, ADL: 1.56 vs. 0.49, and driving: 2.27 vs. 1.15). The pre–post only estimates were significantly greater than zero for all outcome measures, whereas only the pre–post with control estimate of driving was significantly greater than zero (95% CI = 0.28 to 2.02).

The effects of training on measures of attention for the subset of eight studies were similar to those reported in Table 2 in that the pre–post only weighted effect size estimates (shown first) were greater than the corresponding pre–post with control estimates (shown second) for all available outcome measures ($d_+$ values for measures of focus/execute: 0.69 vs. 0.27, sustain: 0.21 vs. 0.06, encode: 0.59 vs. 0.32, working memory: 0.81 vs. 0.10, Picture Completion: 0.66 vs. 0.12, Mental Control: 0.06 vs. 0.04, simple RT: 0.50 vs. 0.38, choice RT: 0.57 vs. 0.46, and other tests: 0.73 vs. 0.33). Seven of the nine estimates of improvement in the pre–post only condition were significantly greater than zero, whereas none of the estimates in the pre–post with control condition were significantly greater than zero. None of the $Q_W$ values in the aforementioned analyses were statistically significant.

In summary, the analyses in this section showed that the pre–post only effect size estimates tended to be large and significantly greater than zero, whereas the pre–post with control estimates tended to be small and not significantly greater than zero. This pattern of results was found in the entire study sample and in the subset of studies in which both types of estimates could be obtained. These findings suggest that the large effect sizes found in pre–post only studies are mainly attributable to the effects of practice on the outcome measures. The notable exceptions to this pattern, however, were the large, statistically significant pre–post with control estimates of improvement in two of the specific-skill measures (i.e., driving and attention behavior).

### *Review of Individual Studies*

The results presented thus far suggest that the type of attention rehabilitation programs that were used in the reviewed studies may improve the performance of specific skills but do not directly restore general cognitive ability or any specific component of attention. It is possible, however, that the failure to find positive effects of direct retraining is the consequence of a strong relation between the nature of the training program and the pattern of cognitive improvement (Sohlberg & Mateer, 1987; Sturm et al., 1997). If so, improvements in specific cognitive functions may vary from study to study and could be masked by the aggregation of results across studies. Alternatively, our aggregation of outcome measures within a study may be inappropriate and could be obscuring strong patterns of improvement.

For these reasons, we examined a number of studies individually. Only those studies that investigated the efficacy of direct retraining with a control condition ($k$ = 12) were considered because of the previously presented evidence that effect size estimates from studies without a control condition largely reflect the effects of practice on the outcome measures. Six of these studies reported no statistically significant improvement after training (Chen et al., 1997; Hajek et al., 1993; Malec, Jones, Rao, & Stubbs, 1984; Middleton, Lambert, & Seggar, 1991; Piskopos, 1991; Ponsford & Kinsella, 1988), and six studies reported a statistically significant improvement in one or more measures (Gray et al., 1992; Niemann, Ruff, & Baser, 1990; Park, Proulx, & Towers, 1999; Ruff et al., 1994; Sturm & Willmes, 1991; Wood & Fussey, 1987). However, the pattern of improvement in this latter set of studies seemed to be more attributable to the acquisition of specific skills rather than to retraining of attention.

In Gray et al.'s (1992) study, the treatment and control groups were tested on 22 different outcome measures shortly before and after training, as well as 6 months after training. At the first follow-up, there were no statistically significant differences in test performance between the two groups; however, at the 6-month follow-up, the treatment group performed better than controls on two working memory tasks (i.e., the PASAT and the Arithmetic subtest of the WAIS–R). The authors pointed out though that participants received training on tasks that involved the storage and manipulation of numbers (e.g., mental arithmetic under time pressure). Thus, improvement in this specific skill, and not in attention, may have aided performance on the PASAT and the Arithmetic subtest.

In Niemann et al.'s (1990) study, performance on the Trails B task improved to a greater extent in the attention training group than in a group that received memory training. However, given that one set of training exercises required participants to alternate attention between different stimulus dimensions, a skill similar to that required in Trails B, the improved performance on Trails B may have been due to specific skills acquired during training.

In Sturm and Willmes's (1991) study, two measures that were special test versions of the training tasks improved substantially after training (mean $d_+ = 1.45$). However, smaller effect sizes (mean $d_+ = 0.14$) were found with the eight other measures of attention.

In Park, Proulx, and Towers's (1999) study, performance on one trial in one condition of the Brown–Peterson task (consonant-trigrams version) improved to a greater extent in the training group than in the control group. However, the authors concluded that the specificity of the observed improvement was more consistent with the acquisition of specific skills rather than retraining of attention.

In Ruff et al.'s (1994) study, performance on the 2 and 7 task improved substantially after training, but performance on other measures of attention improved only marginally or worsened. Unfortunately, this study did not describe the training procedure in enough detail to determine whether this pattern of improvement was attributable to specific exercises.

Finally, in Wood and Fussey's (1987) study, performance on two attention behavior measures and two vigilance tests improved after training, but none of the other attention measures improved significantly. It is notable that the tasks that showed the greatest improvement were measured for a long period of time (20 to 60 min), whereas the tasks that showed the least improvement were measured for a short duration (approximately 5 min). Thus, it is plausible that the 60-min training sessions taught participants to remain focused for long periods of time on a given task and that this specific skill aided performance on the outcome tasks that were measured over a long period of time.

## Discussion

One objective of this meta-analysis was to evaluate quantitatively, for the first time, the efficacy of rehabilitation programs that attempted to directly retrain attention. Several lines of evidence showed that these methods produced only small, statistically nonsignificant improvements in performance in all general measures of cognitive function and in all specific measures of attention when improvement was determined using pre–post with control effect size estimates. We also examined individual studies that reported an improvement in cognitive functioning. Of the 12 direct-retraining studies with a control condition, 6 reported no statistically significant improvement in performance after training. In the remaining 6 studies, the pattern of improvement was specific in each case and, in most cases, could be attributed to specific skills acquired during training. Thus, support for the hypothesis that direct retraining can restore or strengthen damaged attentional function was not found in the reviewed studies.

It is important to emphasize, however, that the findings of this meta-analysis apply only to the direct-retraining methods that have been reported thus far in the literature. It is very possible that there are circumstances in which direct training can restore cognitive function. Robertson and Murre (1999) recently proposed that in individuals with mild to moderate brain lesions, neural circuits might potentially be reestablished if appropriate training is provided. Recovery in individuals with severe lesions though depends on compensation by other brain areas and behavioral adaptation. This proposal could account for the findings of our meta-analysis in that many of the participants in the reviewed studies had severe brain injuries and therefore would not have been expected to benefit from direct retraining. The reviewed studies may have included individual participants for whom direct retraining of attention was effective, but because grouped data were usually presented, we were unable to examine such cases. Direct tests of these types of theoretical ideas that involve careful selection of participants and training procedures may well yield more convincing evidence in support of the direct-retraining hypothesis.

The second objective of this meta-analysis was to identify methodological factors that may contribute to the variability in training efficacy across studies. Effect sizes derived from studies without a control group were consistently much larger than those from studies with a control group for the different measures of cognitive function and types of attention. This same pattern of results was found in the subset of studies in which both pre–post only and pre–post with control estimates could be obtained. These findings strongly suggest that the larger effect sizes in the pre–post only studies are attributable to the effects of practice on the outcome measures and not to other associated factors. Our findings are consistent with previous studies that have demonstrated practice effects in a broad range of measures (e.g., Lehmann, Ban, & Kral, 1968; Quereshi, 1968; Ryan, Morris, Yaffa, & Peterson, 1981; Stuss, Stethem, & Poirier, 1987).

The presence of substantial practice effects is significant for methodological reasons because it underscores the importance of controlling for these effects when one is designing studies to evaluate training efficacy. It also highlights the difficulties of drawing firm conclusions about treatment effectiveness from studies without adequate controls (but see Prigatano, 1999, for a different perspective). Furthermore, the demonstrated practice effects may have important clinical and medico–legal implications in that individuals with acquired brain injury are often tested two or more times on many of the same measures included in the meta-analysis to determine whether recovery has occurred. Our results suggest that "recovery" may reflect, at least in part, practice on the psychological tests.

Perhaps most important, however, the consistency and magnitude of practice effects, observed even after a single exposure to test material, demonstrate that people with acquired brain injuries can quickly learn a broad range of skills. This finding suggests that treatment aimed at helping people learn or relearn skills after an acquired brain injury will probably be effective, particularly if the skill being learned has a substantial attentional component. The most rewarding programs will likely be those that focus on training skills that also are of great functional importance to the individual participants.

The third objective of this meta-analysis was to evaluate, for the first time, whether the direct-retraining and specific-skill approaches differ in their effectiveness. The direct-retraining methods used in the reviewed studies produced only small, statistically nonsignificant improvements in performance, whereas the few studies that attempted to rehabilitate specific skills requiring attention showed statistically significant improvements after training and had considerably larger effect sizes.

The magnitude of the effect sizes from studies with a control condition can be expressed in terms of the binomial effect size display (Rosenthal & Rubin, 1982). This display expresses the percentage of people whose performance improved after training versus the percentage of people whose performance improved in the control condition. Overall cognitive performance (i.e., attention, learning and memory, and other tests) improved for 52% of the participants that received direct retraining, but 48% of those in the control condition also improved (based on $d_+ = 0.09$). In contrast, overall performance improved for 69% of the participants that received specific-skill training (i.e., driving, ADL), whereas performance improved in only 31% of those not trained (based on $d_+ = 0.82$). This degree of improvement after skill training is similar to that reported in a meta-analysis evaluating the effectiveness of psychotherapy in which 66% of the treated participants improved versus 34% of those untreated (Smith & Glass, 1977).

The magnitude of the performance improvement can also be interpreted in terms of the proposal that a $d$ value of 0.20 is a small effect, 0.50 is a medium effect, and 0.80 is a large effect (Cohen, 1977; but see Mayo, 1978). According to these standards, the improvements in cognitive functions after direct retraining are small, whereas the performance improvements after specific-skill training are medium or large.

Thus, the results of this meta-analysis demonstrate that acquired deficits of attention are treatable. Results from the specific-skill studies clearly show that performance on attention-demanding tasks can be improved. Although there is little evidence in the currently reviewed studies to support the efficacy of direct-retraining programs, we hope future investigations will identify the specific conditions in which direct retraining may be successful.

### *Limitations of This Meta-Analysis*

One difficult issue encountered in this study was how to classify and aggregate different outcome measures. As a consequence of this difficulty, it could be argued that the effectiveness of direct-retraining programs was masked either by the aggregation of outcome measures within studies or by the aggregation of results across studies. We dealt with this issue by testing for homogeneity of the effect size distributions using the $Q$ statistic. If there had been particular outcomes from specific studies that were effective, they would have emerged as measures that deviated significantly from the mean effectiveness in that particular condition. In addition, we computed the effectiveness of training at several different levels of measurement so as to increase our chances of detecting specific improvement. Finally, we examined individually all controlled studies that reported significant treatment effects for support of the direct-retraining hypothesis. Taken together, we believe that these steps make it unlikely that there is strong evidence in the reviewed studies for direct retraining of attention.

A second limitation is that the majority of the reviewed studies investigated the efficacy of the direct-retraining approach, and only two group studies with control conditions evaluated the efficacy of specific-skill training (Carter et al., 1988; Kewman et al., 1985). Further empirical research is clearly required to confirm that specific-skill training is effective across a broader range of conditions and to determine the underlying processes responsible for improved performance. However, the large effect sizes currently found in the specific-skill studies, together with the pervasive and large practice effects found in a variety of measures, suggest that future research will establish that specific-skill performance can be improved substantially with training.

A final difficulty that we encountered was the issue of how to define attention. Although we had hoped to categorize more of the specific characteristics of the attention deficits and treatment methods, few studies provided sufficient information. Thus, in identifying studies for the meta-analysis, we relied mostly on the claims of the individual studies themselves that they were attempting to treat deficits in attention. As a result, it is possible, due to the lack of consensus about the meaning of this term, that studies may have had different notions of the type of impairment they were treating, and we may have failed to identify relevant studies because of differences in terminology. However, the consistency of the results across the set of reviewed studies suggests that the primary findings apply to a wide range of treatments for attention deficits and perhaps also to interventions for other cognitive disorders. That the treatment methods reviewed in our study were not able to directly retrain attention is consistent with findings that memory function is not improved by training exercises or drills (Glisky, 1995; Glisky & Schacter, 1989b). In a similar manner, our finding that specific attention-demanding skills can improve with practice is consistent with studies in which specific knowledge and skills have been acquired by amnesic patients with sufficient practice (e.g., Glisky & Schacter, 1987, 1989a; Glisky, Schacter, & Tulving, 1986; B. Wilson, 1992). Our results are also in line with a recent qualitative study that systematically reviewed the effects of cognitive rehabilitation on outcomes for individuals with TBI (Carney et al., 1999); the study concluded that compensatory cognitive strategies should be applied, but that at this point, there was not strong evidence for the effectiveness of direct-retraining approaches to cognitive rehabilitation.

### *Implications for a Theory of Rehabilitation*

Our results suggest that the learning that occurs as a function of training is specific and does not tend to generalize or transfer to tasks that differ considerably from those used in training. This specificity of improvement was shown in the reviewed direct-retraining studies in that there were large effects of practice on the outcome tasks but no effects of training when performance was assessed using pre–post with control measures. Specificity of improvement was also shown in the specific-skill studies and in the individual examination of the direct-retraining studies that had reported an improvement in cognitive functioning.

We believe that the transfer appropriate processing hypothesis provides an overall conceptual framework in which to interpret our results. According to this hypothesis, performance on a particular task after training will improve to the extent that the processing operations required to carry out that task overlap with the processes engaged during training (Kolers & Roediger, 1984; Morris, Bransford, & Franks, 1977). That is, performance will improve after training if the training task is similar to the targeted outcome measure.

This hypothesis, however, gives no indication of how to train a person with a brain injury to perform a complex task. For example, if the treatment goal is to drive a motor vehicle, what is the best way to train this skill? In Kewman et al. (1985), brain-injured individuals were trained on a small electric-powered vehicle. For one group of participants, the complex skill of driving was broken down into simple components, and shaping procedures were then used to help them gain the skill as they practiced. After training, this group had improved substantially more on a variety of driving-related measures relative to a second group that spent the same amount of time driving but was given no specific instruction.

Thus, we believe that rehabilitationists perform a critical role in the success of a training program by structuring it for brain-injured people. We call this activity *neuropsychological scaffolding* because it enables a brain-injured person to perform or learn a skill that could not otherwise be performed as effectively or learned as quickly. Structuring rehabilitation in a particular way may be important because many survivors of TBI (which was the etiology of all but 1 participant in Kewman et al. [1985] and of the majority of participants in this meta-analysis) have a specific impairment performing controlled cognitive processes but are unimpaired when performing automatic processes (Park, Moscovitch, & Robertson, 1999). Because controlled processing is heavily involved in the early stages of learning a skill and is likely less involved as a skill comes to be performed more routinely with practice (Anderson, 1983; Shiffrin & Schneider, 1977), training programs that reduce the requirement for controlled processing during learning may be the most effective. Reduction of controlled processing can be done by breaking down a complex skill into simpler components, by providing practice on these components, and by structuring training in such a way that performance feedback can be more easily interpreted. The technique of "shaping" may be a particularly efficient way to train people with controlled processing deficits because the process of shaping links the difficulty of a task to trainees' performance. As a result, the trainees make fewer errors, and the interpretation of feedback is more straightforward. A central challenge for rehabilitationists will be to develop new, more efficient training procedures. These procedures, based on an emerging set of learning principles, will provide a foundation on which to construct a theory of rehabilitation (Baddeley, 1993a; Plaut, 1996).

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