Letters to the Editor

Studying the Effectiveness of Neurocognitive Remediation in Schizophrenia

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To the Editor:

Neurocognitive remediation is a costly and time-consuming enterprise, and careful study is needed to determine its usefulness in schizophrenia treatment. In the 1980s in head injury treatment settings, a zealous desire to rapidly improve recovery of independent functioning led to the adoption and commercialization of untested cognitive remediation protocols. This resulted in a massive siphoning of resources that might have been put to better use. We welcome the growing interest in the cognitive deficits in schizophrenia, yet caution that rigorous standards for studying their remediability should be met. Within this historical context we consider the recent Schizophrenia Bulletin article by Medalia et al. (1998).

Medalia et al. (1998) studied the effectiveness of a computerized attention remediation protocol in State hospital inpatients with a diagnosis of schizophrenia. Their study and the conclusions that could reasonably be reached have certain limitations. Difficulties stem from several factors: (1) The authors use only one measure (the computerized Continuous Performance Test [CPT; Rosvold et al. 1956]) to assess cognitive functioning before and after treatment. (2) The control condition fails to provide for adequate experimental control. (3) Claims that the treatment effect is generalizable cannot be supported by the reported findings.

The authors' use of only one criterion for therapeutic change in cognitive functioning reduces study costs by decreasing both the required sample size and the duration of each assessment. Optimally these assessments should be proxies for neurocognitive functions mediated by clearly defined neurophysiological systems with well-characterized clinical or functional significance. However, neurocognitive tests actually derive from conventionally accepted hypothetical cognitive constructs whose relationship to functional behavior remains under study. It is well known that neurocognitive dimensions do not exist in isolation from one another. For example, the CPT, the dependent measure in this study, is putatively a measure of sustained attention. However, this test has been shown to correlate highly with motor speed (Walker and Green 1982; Earle-Boyer et al. 1991; van den Bosch et al. 1996).

Neuropsychologists typically employ a battery of tests with several overlapping measures to permit a more accurate estimation of the degree of impairment in the neuropsychological domains involved and to assess whether observed improvements are either stable or domain specific. Medalia et al.'s (1998) use of the CPT eliminated both the difficulty of multiple comparisons and any opportunity to study the mechanism for the observed effect or its specificity to attention.

The nature of the control task (looking at videotapes of National Geographic programs) also raises several difficulties. (1) Several studies have demonstrated that CPT performance correlates highly with simple motor reaction time in schizophrenia. Because the control task involved no motor response while the experimental treatment involved at least two reaction time exercises per session, it would be reasonable to conclude that motor speed and not attention was being remediated. (2) The study provided for one-to-one exposure to a clinician in both experimental and control tasks. In the control condition the clinician did not initiate or reinforce verbal interaction on the subject matter (a potentially awkward situation if spontaneous interaction initiated by the subject is systematically extinguished). In the experimental treatment, however, the clinician initiated and reinforced active participation. This could account for differences in level of
test motivation on the criterion measure, a known influence on neuropsychological tests. (3) The uniqueness of the condition relative to the subject's previous experience might also differentially affect the level of arousal or interest in the experimental condition (e.g., in this instance, watching videos is an everyday activity in a State hospital ward, yet working on a computer might be relatively unique).

The authors themselves point out several other difficulties with the control task. For example, the control condition does not involve exposure to a computer yet both the experimental treatment and the criterion measure do, suggesting that treatment effects could be entirely attributed to a learned facility with the computer rather than attentional functioning per se. The authors also note that the experimental task was associated with a sense of status and privilege on the part of the subjects. These differences between the experimental and the control conditions could result in systematic differences in the subjects' overall enthusiasm for participating in the project. Test performance can be influenced to some degree by contextual factors that can affect motivation and interest. The authors provide no information about whether the control group had a higher incidence of extended periods of random responding, a frequent occurrence with computerized tests in the absence of motivation to perform the task. Such information might clarify whether and to what degree observed differences could be attributed to differences in test motivation versus the presumed remediative benefits for attention.

According to Medalia et al. (1998), the observed computerized CPT improvement suggests a generalizability of therapeutic effect from a computerized attention training task. Generalizability to attentional behavior in real-life situations is the ideal goal, albeit more difficult to study. However, in our view, reliable claims of generalizability must be based on a more substantial transfer of therapeutic effect than that observed in this study. Even generalizability to other measures of attention using alternative response methods (e.g., one not dependent on motor speed) would increase our confidence in the true generalizability of any cognitive effect.

Finally, the authors claim that their findings support the generalizability of treatment effects to the realm of psychopathology and suggest specific benefits for somatization, emotional withdrawal, and hallucinatory behavior. However, the data support only a small, clinically insignificant difference in the magnitude of Brief Psychiatric Rating Scale (BPRS; Overall and Gorham 1962) improvement between the two groups (2.8 and 1.6 point decrease for the experimental and control groups, respectively) and no significant group differences on individual items. Thus, these further claims to generalizability of treatment effect have no apparent basis in the data reported. Moreover, although ratings made from videotapes of BPRS interviews were done blindly, the interviewers themselves were not blind to group membership. Because the BPRS interview calls for considerable clinician judgment and because elicited psychopathology depends largely on the clinical interview, the possibility that a nonblind interviewer could have differentially influenced the ratings of the two groups cannot be ruled out.

References


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