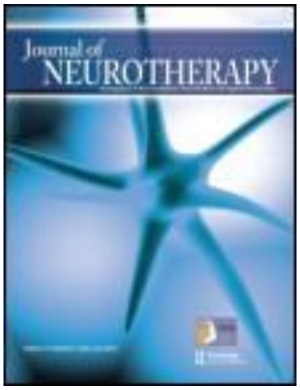

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A Neurotherapy Research Network—Its Time Has Come

David L. Trudeau MD

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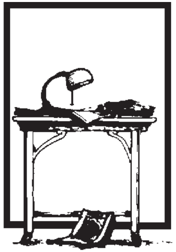

A Neurotherapy Research Network–Its Time Has Come

In this issue of the *Journal of Neurotherapy*, Lonnie Nelson (2003) proposes a neurotherapy research network "in which practitioners would agree to conduct standardized assessments of their clients before, at certain time points during therapy, at termination, and at follow up periods" effectively making the practice of the participating clinicians part of a huge laboratory. The aim of this consortium would be to provide effectiveness data to find out how the interventions were working in the field. This is the type of information (clinical effectiveness of treatment strategies) urgently needed by practitioners for formulating individual treatment planning. Nelson cites the arguments that have been advanced favoring observational studies. Not only are observational studies well suited to measuring the clinical effectiveness of a therapy, but also (if well designed) can provide substantial, large n, multi-center outcome data to existing randomized controlled trials (RCTs), bolstering the validity of neurotherapy overall. In several recent commentaries

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(Trudeau 2001, 2002) I have discussed one of the oft-cited shortcomings of neurotherapy: there are few published reports of RCTs, especially large n studies and multi-center studies, and the underutilization of observational studies. This paucity of papers is due in large part to the difficulty of implementing random assignment to blind study conditions in clinical practice situations involving biofeedback. Evidence for the validity of observational studies is supportive. Nelson suggests that substantial and relevant neurotherapy research can be accomplished by interested clinicians who pool their data, following case protocols that they agree on, comparing results to historical, case cohort, or other suitable matched controls.

Nelson's proposal has far reaching potential for clinical research in neurotherapy. As Nelson points out, using commonly accepted criteria, evidence exists for efficacy and specificity of neurotherapy in Seizure Disorder and Attention Deficit/Hyperactivity Disorder (ADHD), efficacy in Substance Abuse Disorder, and possible efficacy in Traumatic Brain Injury (TBI) and Post-Traumatic Stress Disorder (PTSD). But beyond the big picture question of neurotherapy validity (a moot point with practitioners, who already are true believers in their art) comes the everyday practical question of what works best for what patient in what circumstance. To provide some attempt at answering this question, the *Journal of Neurotherapy* regularly carries the featured "Clinical Corner," ably edited by Associate Editor Cory Hammond. The information presented in "Clinical Corner," though of high interest to clinicians, is by design anecdotal. Less formally presented clinical discussions are found in International Society of Neuronal Regulation (ISNR) meetings and online discussion groups. I find these wonderful online (and late night in the hot tub at the conference) lively debates between advocates of theoretically based neurotherapies both stimulating and perplexing. They are stimulating because they propose models and hypotheses, but perplexing because these debates do not rise to the challenge of "show me the data, the research, the literature to back your opinions." If only the eloquence of therapist-experts (who express high enthusiasm for their unique theory and experience derived protocols and methods) were matched by scientific rigor. But questions posed regarding relative clinical effectiveness remain unanswered for lack of meaningful comparison data. The arguments I have heard for this willingness to rely on data-less conjecture are: "I am a clinician, not a researcher," "No one is paying me to do research," "My practice is way too busy to allow me the time to gather data," and "It works for me, that's all I'm interested in."

Nelson's proposal suggests that busy practitioners might have the time and resources to find meaningful answers to questions that have to do with what types of neurotherapy approaches will work best for what types of patient problems. They can do this by pooling data that is collected in a uniform manner from practice to practice, using a minimum of their own resources. They can participate in large n studies with a few cases each. Not all clinicians will be interested in doing this. But enough will be willing to make small sacrifices of time and convenience, driven by the belief that collaborative research is the only way to settle issues of high clinical importance, and driven by the ethic that their patients/clients deserve the best proven therapies.

Furthermore, a research consortium would be capable of conducting RCTs around many of the oft-debated issues by doing head-to-head comparisons. Some questions that could be answered by this type of research (for instance) might be any of the following. Is qEEG guided therapy for ADHD (or any other condition) more effective than "out of the box" (standard protocol based) therapy, and in what circumstances? Does frontal slow wave or broad band down training have an advantage in ADHD? Does it hold potential for memory or other problems? Is neurotherapy based on linear models more or less efficacious than neurotherapy based on non-linear models? Does audiovisual stimulation (AVS) augment brain wave biofeedback? Is AVS, or Hemoencephalography (HEG) feedback or any type of brain wave biofeedback more efficacious for any particular condition or in any combination? Does neurotherapy of any particular type augment medication response in refractory situations (i.e., treatment resistant depression)? The main difficulty with conducting randomized clinical trials (RCTs) in practice settings is the reluctance of neurotherapy practitioners to essentially "turn away" self-paying clients by offering them randomization to non-treatment conditions, or alternative therapies. But is it conceivable that some neurotherapist clinicians might feel comfortable in offering randomized treatment conditions to their patients with the humble understanding that they, the therapists, do not really know for sure what works best for whom? In other words, the therapist could say to the patient, "we believe this works on the basis of our experience and many published reports–what we don't know is which of several approaches works best. To learn more and to help others, we invite you to participate in a study where we will randomly assign you to one of two neurotherapy treatments that are similar."

The formation and nurturance of a neurotherapy research network is a long-term project. Will the majority of clinicians be likely to participate? Maybe not. But certainly there are those who will, and who will commit a certain amount of their practice time to furthering their science and their art of healing. These are the clinicians who attend conferences and workshops, read this and other journals, write up case studies for review and publication. The leadership of a neurotherapy research consortium will be time consuming and will require patience with a long-term vision to development. It will also require the ability to engender enthusiasm among clinicians, and enlist their participation. To encourage participation, members will need special recognition and a high degree of ownership. What is exciting to me is to see young researchers, such as Nelson, take real leadership in our field with exciting and new ideas for developing research. It is very important that this proposal for a research network be carried out.

David L. Trudeau, MD
Editor

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