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Science, Pseudoscience and Politics

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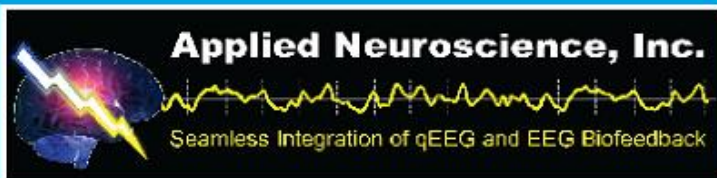
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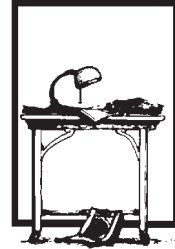
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EDITORIAL



Science, Pseudoscience and Politics

Many of our readers are aware that the field of neurofeedback was recently attacked by a group of individuals associated with the Association for the Advancement of Behavior Therapy (AABT). The first assault was in the form of a paper placed on a pseudoscience web site. Then, some of the same material appeared in a non-peer reviewed article (Lohr, Meunier, Parker, & Kline, 2001) in the AABT newsletter, *The Behavior Therapist*. A joint committee representing SNR and AAPB (Hammond, Sterman, La Vaque, Moore, & Lubar, 2002) made a formal response in a published letter to the editor in their newsletter. It is interesting that when the authors of the critique were asked if they wanted to respond to our letter, none of them chose to do so. However, individuals that seem to be associated with this same “pseudoscience” group have now started their own peer-reviewed journal with the seeming agenda to be able to launch attacks on any therapeutic modalities that do not fit within their conceptual framework. In this editorial, I will first summarize our letter to the editor and then elaborate more fully on these developments.

Our response noted the selective bias of the review of literature contained in the critique, particularly in not reviewing the most rigorous evidence in our field, which is in the area of uncontrolled epilepsy (mostly summarized in Serman, 2000). We briefly reviewed this literature and noted that nine epilepsy studies had randomized control conditions, including an ABA crossover design study, and that there were blinded placebo control studies. We, therefore, concluded that this body of research met the American Psychological Association criteria for the highest level of validation, “efficacious and specific” (Chambless et al., 1998; Chambless & Hollon, 1998). We further chastised their excessive, absolute requirement of EEG normalization at the end of studies. We demonstrated that most often this is shown to occur. However, neurofeedback may also promote greater cognitive flexibility in response to mental demands or changes in variables not measured (e.g., coherence, phase) and, therefore, an absence of EEG normalization at the end of a study with positive outcomes may not always mean that the results were due to nonspecific factors. We further cited three neurofeedback studies (Garrett & Silver, 1975) that had also been neglected in their review. These studies included random assignment, alternative treatment control groups, and a wait list control group, making this an area qualifying for possibly efficacious status. Their space limitations did not permit us to make a full response to their critique of ADD/ADHD. Nonetheless, we emphasized that Lohr et al. (2001) were seeking to impose their own excessive standards for acceptable research standards that went beyond the APA task force guidelines. For instance, they sought to require placebo control conditions in our research. We pointed out that both medical ethicists and the Declaration of Helsinki of the World Medical Association deemed placebo controls unethical when there was a known effective treatment available. I would add that Linden et al. (1996) noted that they considered including a placebo condition in their study, but the university human subjects committee deemed it unethical.

At this time, I would like to elaborate more fully on these issues. The APA position paper does not require placebo controls for a treatment to be identified as efficacious. Concerning the task force deliberations, Chambless et al. (1998) noted:

Our group members do hold different views about whether specificity should be necessary for us to consider a treatment well-established. Some consider the question of the mechanism by which treatment works to be separate from efficacy considerations, whereas

others believe it is essential for psychological interventions meeting or exceeding the standards for pharmacological interventions to be identified and highlighted. That is, comparisons to a waiting list control for the passage of time and the effects of assessment, but they do not control for so-called nonspecific factors like expectancy of change and contact with a supportive professional. (p. 6)

In addition to problems with the Declaration of Helsinki, there are other problems with seeking to imitate pharmacological research that utilizes placebo controls. Recent meta-analysis reviews (Antonuccio et al., 1999; Greenberg et al., 1992; Kirsch & Sapperstein, 1998; Moncrieff, 2001; Walach & Maidhof, 1999) of antidepressant medication research and placebos point out the interesting role of research bias and politics in mental health research. These studies reveal:

1. Most double or single-blind, placebo controlled studies prescreen subjects in a manner that often screens out subjects who are placebo responders within the first week or two or who show improvement after being taken off a currently used antidepressant. Nonetheless the placebo response rate in double-blind placebo-controlled research is still usually 30-50%.
2. Placebo-controlled studies commonly use inert placebos that have no side effects. The result is that many patients and raters can correctly discern the group assignment, basically unblinding the study. When studies used an active placebo (e.g., atropine, which causes anticholinergic side effects) so that we would expect less of a positive expectancy to have been created in the medication treatment group, reviews (Greenberg et al., 1992; Moncrieff et al., 1998) found only 14% and 22% of the antidepressant studies to have superior effects to a placebo.
3. Kirsch and Sapperstein's (1998) meta-analysis documented that an inactive placebo produced 75% of the response found with antidepressant medication, and that placebo and drug effects were highly correlated (.90), suggesting that perhaps only 25% of the change produced by antidepressant medications results from the unique properties of the medication instead of placebo. In the meta-analyses using active placebos, effect sizes were an almost negligible .2, representing a 10% difference between the response to a placebo and antidepressant medication. Furthermore, antidepressant studies typically rely on clinician ratings of improvement, rather than patient self-reports which only show small improve-

ments in outcomes. Drug studies further tend to drop from their analysis subjects who quit early due to side effects, providing another bias in their favor. Additionally, there are accusations of publication bias wherein when a drug industry sponsored study does not produce positive outcomes, the research is buried and never published. In another meta-analysis, Freemantle et al. (2000) discovered that the greatest predictor of antidepressant efficacy was the trial sponsor!

Thus, the integrity of double-blind medication studies, especially that do not use active placebos, is seriously in doubt. Does the mental health field really want to model our research after such scientifically biased studies, as Lohr et al. (2001) seem to want? I find it profoundly disturbing that such seriously flawed and systematically biased research on the efficacy of antidepressant medication is what insurance company and medical industry data bases regularly rely on in documenting what treatments are efficacious.

With regard to bias in science, a member of the APA task force that drafted their efficacy criteria published an expose (Beutler & Harwood, 2001) on some of the anti-scientific attitudes among academic psychologists who sought to apply lobbying pressure on the task force. He said that “scientists, themselves, are far from being consistent in their own applications of value-free scientific criteria of empirical evidence” (p. 47). As an example, he detailed how the Task Force on Empirically Supported Treatments concluded that current research on eye movement desensitization and reprocessing (EMDR) had accumulated sufficient research support for it to be categorized as “probably efficacious.” This resulted in intense lobbying from academics who did everything from attacking the originator of EMDR personally to threatening to resign APA membership if the position of the task force was not reversed. As we have recently witnessed in the attacks on the scientific status of neurofeedback, proponents of the pseudoscience position protested that more rigorous research designs should be required. “These arguments ignored the fact that this criterion was not included for any of the more conventional therapies that were reviewed, for which a comparison with a no-treatment or placebo-treatment control was sufficient” (Beutler & Harwood, 2001, p. 49). When still another group from the task force reviewed the findings and reached the same conclusion about the “probably efficacious” status of EMDR, personal attacks were even leveled at task force members. The panel witnessed how respected academic psychologists who disliked the technique or its originator would claim that

the research results must have been faked and then “ignored positive findings or distorted the presentation of findings, ostensibly in order to cast EMDR in the worst possible light” (p. 49).

It is deplorable that politics and the kinds of bias that I have noted are so widespread within the scientific mental health community. In our recent letter to the editor (Hammond et al., 2002) we emphasized that we wholeheartedly agreed that many areas of neurofeedback do indeed require improved research validation and higher quality research. We must do this to obtain scientific respect and acceptance. However, we must also be aware of bias and political forces that are at work and vigorously resist and expose efforts by academics who seek to label as “pseudoscience” any brand of treatment that does not fit within their conceptual framework of what is acceptable.

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