

Key Issues Relevant to the Efficacy of Behavioral Treatment for ADHD

TO THE EDITOR: In their recent meta-analysis of non-pharmacological interventions for attention deficit hyperactivity disorder (ADHD), Sonuga-Barke et al. (1) concluded that limited evidence is available for behavioral interventions. We raise a number of key issues for consideration with respect to this conclusion.

Most importantly, ADHD symptoms were the primary outcome measure in this meta-analysis. Across disorders, however, functional impairment is what prompts treatment seeking, and reducing impairment is the desired outcome (2). ADHD symptom reduction does not necessarily equate to improvements in functioning (3); impairment is therefore a far more socially valid outcome than psychiatric symptoms per se. For behavioral interventions, the primary target of treatment has historically been impairment. For example, dysfunctional parent-child interactions as evaluated by blinded observational assessments determine larger effects on ADHD symptoms in parent training studies (4) and predict serious developmental outcomes for children with ADHD (5). ADHD symptoms are therefore by no means considered the only (and certainly not the most clinically important) target of treatment.

Additionally, the behavioral intervention studies included in this meta-analysis constitute only a small fraction of the larger treatment literature (4). Furthermore, combining diverse non-pharmacological interventions into a "behavioral intervention category" may grossly undermine the estimated effects of behavioral interventions. For instance, interventions such as direct child therapy and social skills training have little to no empirical evidence, whereas behavioral parent training and behavioral classroom interventions are well-established treatments (6). The approach taken by Sonuga-Barke et al. is analogous to a meta-analysis of pharmacological treatments for ADHD that combines stimulants, antidepressants, and atypical antipsychotics as a single category of "stimulant medications."

A key issue highlighted in this article is the need to include active treatment comparison conditions to obtain objective evaluations of treatment outcomes, which we fully support. Unfortunately, the approach taken by the authors often involved evaluating teacher ratings after home interventions, parent ratings after school interventions, or brief laboratory or clinic observations of the child's ADHD symptoms. Behavioral treatments demonstrate the largest effects in the setting in which they are implemented (6), and blinded assessments of behavior in these settings will likely yield a more valid evaluation of treatment effects.

We hope that this article encourages an investment in more rigorously designed outcome studies, but we have concerns about the potential implications of this meta-analysis despite more than 40 years of solid evidence for the efficacy of behavioral interventions.

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Evidence for Efficacy of Neurofeedback in ADHD?

TO THE EDITOR: A recent article by Sonuga-Barke et al. (1) presented the results of separate meta-analyses for nonpharmacological interventions of attention deficit hyperactivity disorder (ADHD). The authors considered randomized controlled neurofeedback trials, and they evaluated "most proximal" and "probably blinded" ADHD assessments separately. For neurofeedback, they reported a significant standardized mean difference of 0.59 for most proximal assessment, but they reported only a trend for probably blinded assessment (standardized mean difference, 0.29; 95% CI = -0.02 to 0.61). The authors concluded that further neurofeedback trials with probably blinded assessments were necessary before this treatment can be recommended for core ADHD symptoms. We will not comment on this here, because we have queries to the authors regarding study selection and criteria adherence for analyses that have to be answered before any conclusions about most proximal and probably blinded assessments can be drawn (also see reference 2 for a review).

In one of the studies referenced in the analysis (Steiner et al. [3]), three conditions were evaluated: a neurofeedback group, a cognitive training group, and a waiting list group. Sonuga-Barke et al. (1) stated that they selected control conditions “in the following order: sham/placebo, attention/active control, treatment as usual, waiting list.” However, they calculated the standardized mean difference for the Steiner et al. study against the waiting list, whereas they should have calculated it against the cognitive training condition. The null hypothesis for such a meta-analysis should be that all conditions have no effect; thus, interpreting the cognitive training as an “active ADHD treatment” (E.J. Sonuga-Barke, personal communication, February 2013) for one study but not for another study is inconsistent. Furthermore, the Steiner et al. study reported that 30% of patients in the neurofeedback and cognitive training conditions reduced their medication, whereas none of the individuals in the waiting list did, making a comparison against the waiting list group even more problematic. Therefore, Sonuga-Barke et al. should have compared the neurofeedback group with the cognitive training group.

Slow cortical potential and theta-beta training are well-investigated neurofeedback protocols (2). However, other protocols that have not been systematically evaluated were also included. We therefore question the selection of studies and control conditions in this meta-analysis. We recalculated the statistics (using the specifications provided in the article) by including only standard neurofeedback protocols (i.e., omitting the study by Lansbergen et al. [4]) and against stringent comparison groups (computerized attention training or electromyographic biofeedback; three studies). For parent ratings, we obtained a significant standardized mean difference of 0.58 (95% CI=0.12–0.94; $Z=3.52$; $p=0.0004$), and for teacher ratings we found a significant standardized mean difference of 0.39 (95% CI=0.07–0.70; $Z=2.39$; $p=0.02$). Therefore, based on standard neurofeedback protocols and strictly adhering to the criteria from Sonuga-Barke et al., the conclusion of the European Guidelines Group that neurofeedback cannot be supported as a treatment for core ADHD symptoms is not supported by the data.

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Response to Chronis-Tuscano et al. and Arns and Strehl

TO THE EDITOR: We are grateful to our colleagues for their letters. Overall, they raised seven substantial points. All of the issues raised had already been discussed in considerable detail during the planning of the review, the design of its protocol, and the process of interpreting the results.

Chronis-Tuscano et al. (1) raise a number of interesting and important points in their letter, specifically in relation to the analysis of behavioral interventions, although the issues raised are also relevant for the other intervention domains covered in the review.

Were we justified in restricting trials in the meta-analysis to a carefully selected subset of the wider literature? Chronis-Tuscano et al. highlight the fact that the “behavioral intervention studies included in this meta-analysis constitute only a small fraction of the larger treatment literature.” It is unclear whether this represents a comment on the restrictive nature of our protocol or the way it was implemented. With regard to the latter, given our exhaustive search and the rigor of our study selection procedures, we are confident that we have reviewed all the relevant literature in each domain (see the data supplement that accompanies the online edition of our article [2]). With regard to the protocol itself, we chose to include only trials that were directly relevant to ADHD and were undertaken using a randomized controlled design. To our surprise, only a small proportion of the trials constituting “more than 40 years of solid evidence for the efficacy of behavioral interventions” employed such a design, which is regarded by many as a minimum design requirement for inclusion in meta-analyses of treatment efficacy. Clearly we agree with Chronis-Tuscano et al. that the field of behavioral interventions for ADHD needs far more rigorously designed trials.

What is efficacy in relation to ADHD treatments? Claims made for the efficacy of interventions for ADHD are often expressed in rather general terms. Such claims rarely specify which outcome they are referring to, and it is quite reasonable that many assume that such claims are being made in relation to the treatment of ADHD per se and not some more general feature of child psychopathology (e.g., functional impairment). It was to start to clarify this question—efficacy in relation to what?—that we undertook what we agree were necessarily highly circumscribed analyses. In this regard,