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Over-Optimism of Cognitive Behavior Therapy for Schizophrenia

TO THE EDITOR: The review article by Douglas Turkington, M.B.Ch.B., F.R.C.Psych., and colleagues (1) makes the assertion that cognitive behavior therapy for treating patients with schizophrenia has been accepted in the United Kingdom and that evidence supporting this treatment for schizophrenia validates a similar uptake in the United States. In the editorial accompanying the article by Turkington and colleagues (2), it is accepted that cognitive behavior therapy is a promising treatment but that there is a need to avoid overpromising. To this caution, two points must be made.

First, the evidence base of cognitive behavior therapy for schizophrenia consists of heterogeneous models of cognitive behavior therapy delivered to heterogeneous diagnostic cohorts, with some studies only having 60% of subjects with schizophrenia. This diagnostic heterogeneity leads to possible systematic bias, and the outcome variables can be clinically misleading (3). The aim of cognitive behavior therapy for treating schizophrenia is to decrease the distress associated with symptoms, but by denying the prognostic implications of diagnosis, the validity of this therapy becomes an oxymoron.

Second, the evidence base shows the greatest effect to be associated with the poorest methodology (4), and therefore the validity of the combination of the current reported trials for meta-analysis is doubtful. A recent United Kingdom National Health Service report on long-term follow-up trials of cognitive behavior therapy for treating psychosis found that there was generally a poor outcome with no superiority on clinically significant change and no economic advantage regardless of treatment modality (5).

As a cognitive behavior therapist, I feel that the lack of scientific rigor from the findings of the evidence base needs to be challenged but should not necessarily change the approach to treating patients with schizophrenia. Tarrier and Wykes (4) suggest that the component analysis of cognitive behavior therapy for schizophrenia may not be the way to settle the theoretical arguments, but it may be that an approach within the "spirit of cognitive behavior therapy" is more important, with an analogy being motivational interviewing. The debate in the United Kingdom over the effectiveness of cognitive behavior therapy for treating schizophrenia is far from over, but most psychiatrists would agreed that interacting with patients and enhancing collaboration is universal, good clinical practice.

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Dr. Turkington Replies

TO THE EDITOR: Dr. Marlowe makes a number of valid points. We agree that the dissemination of cognitive behavior therapy for treating schizophrenia in the United States and elsewhere deserves careful consideration and further research. The accepted practice of cognitive behavior therapy for treating schizophrenia in the United Kingdom has been endorsed by the National Institute for Clinical Excellence (1), which sets standards for the National Health Service and is monitored by the health commission, but this does not mean that it is equally relevant in other countries. Studies have been similar in their diagnostic inclusion criteria, which consist of patients from the schizophrenia group and therefore include patients with schizoaffective and delusional disorders. None of the major studies include patients outside this diagnostic cluster. I have one question pertaining to Dr. Marlowe's comment that studies with the poorest methodology have reported the highest effect sizes: Is this not a recognized phenomenon throughout clinical research?

There are indeed some differences between the cognitive behavior therapy manuals currently in use (2, 3), but there is a consensus around the key components and order of application of techniques. The basic cognitive model for positive symptoms of schizophrenia has been developed

and described by Garety and colleagues (4) and has been widely accepted. Long-term follow-up of studies, after therapy has been discontinued, has not yet demonstrated an enduring effect, but neither has this been the case with other interventions, whether pharmacological or psychosocial.

In terms of prognosis, we refer Dr. Marlowe to the 20-year follow-up study conducted by Harrison and colleagues (5) in which the outcome for patients with schizophrenia was nowhere near as negative as that implied by shorter follow-up periods and gives reasonable hope for recovery for many, especially to those who are offered treatments as promising as cognitive behavior therapy for schizophrenia.

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Blinding in Psychotherapy Plus Medication Trials

TO THE EDITOR: We support the call by Glen O. Gabbard, M.D., and Robert Freedman, M.D., for more rigorous trials of psychotherapy (1). However, we wish to call attention to an important, underappreciated bias in trials that investigate both psychotherapy and pharmacotherapy. Allocation concealment is inherently easier for the medication treatment arms (through pill placebos) than the psychotherapy treatment arms, which may result in favorable response rates for psychotherapy compared to pharmacotherapy.

While the challenge of blinding in psychotherapy trials has been long recognized, we wish to point out that the problem intensifies in trials using both psychotherapy and medication arms. This is because some participants are more thoroughly blinded than others. In essence, two trials exist within one: a psychotherapy trial, which is often somewhat of an open-label "effectiveness"-type trial, and a medication trial, which is a double-blind "efficacy"-type trial. Thus, psychotherapy response rates may be inflated due to greater participant knowledge of their treatment assignment and their expectations. Because many of us are conditioned by randomized medication trials to expect that all arms are similarly blinded, it is easy to overlook this potential bias.

Two recent obsessive-compulsive disorder trials are illustrative. Foa and colleagues (2) found that their exposure and ritual prevention produced better rates of "excellent" responses, when compared to clomipramine, and more respondents completing the trial. However, there was a crucial difference between the psychotherapy and medication arms: participants were not blinded to psychotherapy assignment—although raters and researchers were—while participants, as well as raters and researchers, were blind to medication assignment. Nakatani and colleagues (3) deserve praise for including a placebo psychotherapy arm (relaxation training) in their study of behavioral therapy versus fluvoxamine. However, in light of the highly positive response in the behavioral therapy arm, the authors could have considered administering a measure, such as a "guess test," to estimate whether participants could discern if their assignment was to active psychotherapy treatment. A similar approach could have been undertaken in the study by Foa and colleagues to determine whether raters remained blinded effectively.

Because of the intrinsic difficulties in blinding psychotherapy interventions compared to medication interventions, we suggest that trials involving psychotherapy and medication arms could make some attempt to estimate how effectively blinding is maintained. It would also be beneficial if, in the reports of such trials, language could be included indicating that allocation concealment is likely to be more difficult in some treatment arms than in others, which might influence response rates.

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Do Symptom Dimensions of Depression Following Myocardial Infarction Relate Differently to Physical Health Indicators and Cardiac Prognosis?

TO THE EDITOR: Peter de Jonge, PH.D., and colleagues (1) report that the measurement of somatic symptoms of depression following myocardial infarction is confounded by physical health and that somatic symptoms, but not cognitive symptoms, predict cardiac prognosis. Their results, however, do not appear to support these conclusions, primarily because the method used to delineate somatic and cognitive