Review of Clinical Psychology will include a chapter that reviews publications on the use of the structural analysis of social behavior as an assessment instrument.

A second misrepresentation is the following: "Unfortunately, Benjamin oversells her treatment. She claims it has empirical support, but this is limited to a few case studies and testimonials from former students and supervisees."

When the book was published, the limited nature of supporting data was basically as described by Dr. Wetzler (1), and his summary comes from the book. After reporting pilot data, I added, "This list of results—some of which are based on objective, symptom-oriented data gathered before and after treatment, hardly constitutes a formal clinical trial. But the data are a step above the 'testimonial' or isolated 'case report' methods of validation.…Clearly, formal clinical trials are needed next" (4, p. 343). I have maintained that interpersonal reconstructive therapy is "empirically informed," meaning that its theory and methods draw heavily on published research. These claims do not represent "overselling."

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On Intercountry Adoptions

TO THE EDITOR: The report of Wendy Tieman, M.S., and colleagues (1) on psychiatric disorders in intercountry adoptees was a useful review of the Netherlands experience. Although the authors found a higher risk of mental disorders in adoptees, they noted that "The majority did not show serious mental health problems," and they commented that "This is surprising, given the adverse circumstances in which the majority of these children lived the first part of their lives" (p. 597). Such comments are consistent with the findings from an Australian study of adolescents (mean age=15 years and 2 months) who had been adopted from Indonesia and who had no increase in psychiatric symptom profiles compared with a random community sample of similarly aged adolescents (2). Furthermore, there was no correlation between the measure of psychopathology and the age at which the adolescents had been adopted. Although the findings are unexpected, they may reflect the care with which intercountry adoptions are undertaken.

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Dr. Tieman and Colleagues Reply

TO THE EDITOR: In his letter, Dr. Goldney presents results of an Australian study in which he and his colleagues found no differences in psychiatric symptom profiles between adoptees and nonadoptees in adolescence (his reference 2). However, contrary to the results of the Australian study, we found that more adoptees (29.6%) than nonadoptees (21.6%) had a psychiatric disorder in adulthood. Our study is one of the few that investigated the mental health problems of adoptees in adulthood. There are a number of studies that reported on the adjustment of adoptees in adolescence. The results of our study corroborate the results of studies in which adoptees had more mental health problems than nonadoptees in adolescence, as reported in two reviews (1, 2). Despite the higher level of psychiatric morbidity that we reported for adult adoptees compared to that for nonadopted individuals from the general population, it is equally true that the majority of the adoptees had no serious mental health problems, although many of them had adverse early experiences. Therefore, we agree with Dr. Goldney that many intercountry adoptions result in good outcomes for many adoptees. To what extent this is due to the care with which intercountry adoptions are undertaken or to the individuals' resiliency cannot be concluded from our data. We are currently studying the outcomes of international adoptees in their social functioning, including their education, work, and relationships.

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Benefits of Light Treatment for Depression

TO THE EDITOR: It is gratifying that the APA Committee on Research on Psychiatric Treatments (in a report by Robert N. Golden, M.D., et al. [1]) concluded that light therapy is beneficial and that effect sizes are comparable to those found in antidepressant drug trials.

A similar conclusion on the effects of bright light was reached in our Cochrane review of light treatment of nonseasonal depression (2). There are many complexities in deciding which studies to include in meta-analyses. The Cochrane review used extensive search strategies to retrieve all relevant randomized studies and included many more randomized studies of light treatment for nonseasonal depression than the recent meta-analysis by Dr. Golden et al. (1). Differing from the conclusion of Dr. Golden et al., the Co-

chrane review found a slightly greater benefit of light therapy in studies of subjects receiving concomitant drug treatment than among those who were drug free. The recent large study of Martiny (3) added new evidence for the adjunctive use of bright light. A forthcoming update of our systematic Cochrane review (unpublished) will include this and other new studies on the topic.

The analysis of Dr. Golden et al. questioning the effectiveness of adjunctive light treatment for nonseasonal depression may have included some minor errors. The two least-positive reports that were included (their references 25 and 26) used the same study twice. Also, the two less encouraging studies included for drug-free treatment of nonseasonal depression appear to have used the same study twice (their references 16 and 18). An erroneous negative impression of light treatment may have arisen from the nine publications of the nonsignificant contrasts with control observed in these two studies counted twice, whereas most positive-outcome studies were published in only one place.

The APA Committee on Research on Psychiatric Treatments pointed out correctly that many studies of bright light have been small and have not received the financial backing that has been devoted to clinical trials of antidepressant drugs. There is already some evidence to endorse the use of bright light treatment for nonseasonal depression, but further studies evaluating the use of bright light treatment as an adjunct to pharmacotherapy are clearly needed. Neither bright light nor pharmacotherapy for depression produces a high enough remission rate, so the combination would improve our therapeutic approach. If we are to better understand the optimal doses of bright light and the circumstances in which it is beneficial, a number of larger-scale multicenter trials must be supported.

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Dr. Golden and Colleagues Reply

TO THE EDITOR: We appreciate the interest in our work and the thoughtful comments made by Dr. Kripke and his colleagues. We agree that in establishing a priori criteria for study inclusion we "set the bar" a bit higher than in their Cochrane review. We believe our study inclusion criteria, as described in the article, are reasonable and consistent with conventional standards for clinical trial study design (1). Also, in contrast to the Cochrane review, we focused on a more homogeneous diagnostic group (e.g., we did not include studies of subsyndromal depression, schizoaffective disorder, or bipolar disorder) and age range. In addition, we set parameters for a minimum therapeutic dose of active treatment, as well as a maximum amount for placebo conditions.

The Martiny report (your reference 3), published after the completion of our study, is an important new addition to the evidence base. We want to especially thank Dr. Kripke et al. for pointing out the two sets of reports that were referenced twice. We had detected (and corrected for) a third set but did not realize that there were two additional duplicate reports (perhaps because each article had different first and last authors). Still, our conclusions remain the same. There is a clear need for further study in this important area, including larger-scale multicenter trials, and an even greater need for all studies to conform to the principles of clinical trial research design and established standards for scientific reports in depression (2).

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Correction

The corresponding author's e-mail address was incorrect in an article in the October issue titled "Continuities Between Emotional and Disruptive Behavior Disorders in Adolescence and Personality Disorders in Adulthood" by Margareth I. Helgeland, Ph.D., et al. (Am J Psychiatry 2005; 162:1941–1947). The correct e-mail address is tyra@chello.no.