probability scale of Naranjo et al., the case was ranked as a probable adverse drug reaction (5).

This case illustrates that the risk of developing extrapyramidal symptoms is still present, even with the newest of atypical antipsychotics. Clinicians should vigilantly monitor all patients for the emergence of symptoms, regardless of which antipsychotic a patient is receiving.

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Fatal Agranulocytosis 4 Years After Discontinuation of Clozapine

TO THE EDITOR: Clozapine is currently considered the most efficacious antipsychotic for the treatment of schizophrenic symptoms (1), but its use is limited because of the risk of agranulocytosis. Although the risk of this life-threatening adverse event is highest during the first 4 months of administration, it was recently reported that it can occur even after 11 years of continuous treatment (2). To our knowledge, it has not been reported that agranulocytosis can occur several months or even years after the discontinuation of clozapine. Here is the case of a mentally retarded patient who developed agranulocytosis after 7 years of clozapine treatment and then continued to suffer continuously from severe neutropenia, which developed into fatal agranulocytosis more than 4 years after the discontinuation of clozapine.

Mr. A was a 49-year-old man of Finnish origin who had been treated in a local nursing home because of behavioral problems associated with moderate mental retardation. Severe aggressive behavior was a major problem in his daily life; therefore, clozapine treatment was started. Clozapine, 450 mg/day, resulted in a marked reduction in his aggressive behavior, but it was discontinued because of agranulocytosis. Mr. A started to suffer from severe recurrent infections, and treatment with granulocyte-colony-stimulating factor gave only temporary benefits. His hematologist concluded that his blood dyscrasia was chronic because his total WBC count fluctuated from 0.5 to 1.5 $\times$ 10^9/liter (normal WBC count range=4.0–10.0 $\times$ 10^9/liter). At that time, it was decided to administer palliative treatment in the familiar environment of his nursing home. Mr. A's prognosis did not improve, and he died a few weeks later. A forensic autopsy concluded the cause of death to be clozapine-induced myelodysplasia of the bone narrow. Because of a complaint by a relative, the National Authority for Medico-Legal Affairs asked for an expert's opinion on whether the pharmacological treatment was adequately administered.

To our knowledge, this is the first reported case of clozapine-induced agranulocytosis to have occurred several years after the discontinuation of clozapine treatment. In this case, agranulocytosis was not caused by a reversible, acute toxic or immunological reaction in bone narrow but was a consequence of a permanent change in the maturation of blood cells, leading to myelodysplastic syndrome.

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Review of *Interpersonal Reconstructive Therapy*

TO THE EDITOR: The book review of *Interpersonal Reconstructive Therapy: Promoting Change in Nonresponders* (1) made the point that both the structural analysis of social behavior and my therapy approach—interpersonal reconstructive therapy—have some substance but are too complex to be understandable or useful. The reviewer is not alone in reaching that conclusion. However, the same sentiment about the structural analysis of social behavior was expressed more positively by Jerry Wiggins (2), who wrote that the structural analysis of social behavior "is the most detailed, clinically rich, ambitious, and conceptually demanding of all contemporary models." Of course, reviewers are free to express any opinions they wish, but if their conclusions are to be fair and if readers are to be able to determine whether they agree or not, readers deserve an accurate representation of the material.

Please consider the following two distortions of fact in the review. The first is the following:

> Despite Benjamin's efforts to use the structural analysis of social behavior for research purposes, it has proven too complicated and cumbersome and never gained widespread application. The few research studies using Benjamin's coding system focused primarily on interactions in psychotherapy. I too have felt that the structural analysis of social behavior is an intriguing foreign language, but not terribly practical; and I never did learn the language.

There have been many publications based on the structural analysis of social behavior, and the range of topics is broad. A list of known publications is available from the University of Utah (http://www.psych.utah.edu/benjamin/sasb/index.html) by request. One published review of uses of the structural analysis of social behavior involves a series of articles that appeared in the *Journal of Consulting and Clinical Psychology* in December 1996. Another is a review of articles about the structural analysis of social behavior focused on psychotherapy by Constantino (3). In April 2006, the *Annual*

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-