## Quetiapine for Treatment-Resistant Mania

Antipsychotic medication is safe and effective in the treatment of mania (1). When compared to conventional antipsychotics, atypical antipsychotics carry a lower risk of extrapyramidal side effects and tardive dyskinesia (2). Some of these agents appear efficacious in the treatment of mania (3–5). Quetiapine is an atypical antipsychotic that is effective for the treatment of schizophrenia (6). However, to our knowledge, there are no published reports of quetiapine used in the treatment of bipolar disorder. We report the use of quetiapine as an adjunctive therapy in combating treatment-resistant bipolar disorder.

**Ms. A was a 39-year-old married woman who was hospitalized for worsening mania after reduction of her trifluoperazine dose from 15 to 12 mg/day. Her symptoms included insomnia, racing thoughts, sexual preoccupation, impulsivity, irritability, increased energy, pressured speech, flights of ideas, paranoid ideation, auditory hallucinations, and suicidal ideation. She was also taking valproic acid, 2000 mg/day, and lithium carbonate, 1200 mg/day. Her blood levels of these drugs were 116 μg/ml and 1.2 meq/ml, respectively. Ms. A's bipolar disorder had begun during her 20s, and she had initially responded to treatment with lithium carbonate. After several relapses, she started experiencing breakthrough symptoms while taking therapeutic doses of lithium, which required augmentation with both valproic acid and antipsychotic agents. These medications were poorly tolerated, causing weight gain, alopecia, hirsutism, mild oral tardive dyskinesia, and parkinsonism, both with standard antipsychotics and with olanzapine and risperidone. ECT had been minimally effective.**

**At admission Ms. A began treatment with quetiapine, which was titrated to 75 mg t.i.d., while she continued maintenance treatment with valproic acid, lithium carbonate, and 6 mg/day of trifluoperazine. Her manic symptoms decreased rapidly with minimal sedation. After discharge she was unable to immediately follow up with outpatient treatment and was readmitted 10 days later with an exacerbation of mania. Her quetiapine dose was increased to 150 mg b.i.d. and 200 mg at bedtime over 4 days. The trifluoperazine and valproate doses were decreased to 4 mg and 1500 mg at bedtime, respectively, to minimize sedation and sialorrhea. Ms. A was discharged after 8 days of hospitalization with full remission of her manic and psychotic symptoms. Over the next 6 months her doses of trifluoperazine and valproic acid were tapered off and discontinued. Her quetiapine dose was adjusted to 200 mg in the morning and 400 mg at bedtime, resulting in weight loss and a decrease in sedation. She has remained clinically stable with combined lithium and quetiapine therapy.**

This case suggests that quetiapine can be safe and effective in the treatment of the manic and psychotic symptoms of bipolar disorder. Further clinical trials are needed to confirm its value in the treatment of affective disorders.

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## Irritable Bowel Syndrome and Mirtazapine

Irritable bowel syndrome is a clinical entity in which symptoms of abdominal discomfort and altered bowel movements occur in the absence of any structural pathology. It can have debilitating effects in the 8%–17% of the general population whom it affects (1). The available literature reports that irritable bowel syndrome is associated with a higher lifetime prevalence of psychiatric illness, predominantly anxiety and mood disorders (2). Medical intervention is typically tailored to the patient's predominant symptom and includes dietary changes, anticholinergics, motility inhibitors, smooth muscle relaxants, and psychotropic medications (3). I report the efficacy of mirtazapine in the treatment of irritable bowel syndrome.

**Ms. A was a 35-year-old divorced woman with a history of recurrent depression, panic disorder, and posttraumatic stress disorder (PTSD) who was referred for what her gastroenterologist had diagnosed as irritable bowel syndrome. She reported a 7-month history of severe abdominal cramping, bloating, and constipation. These symptoms were accompanied by a 20-lb weight loss. The results of an extensive medical evaluation were negative. Her gastroenterologist had treated her with diazepam, 5 mg t.i.d., and cisapride, 10 mg q.i.d., with minimal improvement. The symptoms of irritable bowel syndrome had been present episodically throughout her life but had been virtually unremitting for the past 7 months. In the 6 months before her referral, Ms. A had a total of 10 visits with her primary care physician or a specialist. She also had missed at least 15 days from work and had planned most of her days around access to a restroom.**

**When Ms. A was seen by a psychiatrist, her diagnosis of irritable bowel syndrome was confirmed by the use of ICD-9 criteria. Ms. A also met the criteria for major depression, panic disorder, and PTSD. She began treatment with mirtazapine, 7.5 mg q.i.d., which was increased in 7.5-mg increments every 2 weeks up to a daily dose of 30 mg. At 12 weeks she was significantly improved. Ms. A stated that her bowel movements were normal, and she had a marked decrease in all gastrointestinal symptoms. In addition, she had gained 20 lb, thereby attaining her target**

**weight of 120 lb. She had missed no days from work in 2 months and had not seen a physician other than myself in that period. She reported that this was the first medication that had helped her.**

Other antidepressants reported as effective for irritable bowel syndrome include selective serotonin reuptake inhibitors and tricyclic agents. It is not known whether the site of action is enteric, central, or both. But the fact that patients with irritable bowel syndrome often have poor results with a purely medical approach indicates that psychiatric treatment could play an important role in improving outcome.


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## Antipsychotics During Pregnancy

Although data regarding conventional antipsychotics and pregnancy are available (1), reports about the course of pregnancy in women treated with atypical agents are sparse. A literature search revealed case reports and letters to the editor on the use of clozapine during pregnancy (2–4). Olanzapine use during pregnancy has also been reported; however, the woman in question chose to abort the fetus (5). We know of no published reports of women conceiving who were treated with risperidone or quetiapine. To our knowledge, this is the first case report of olanzapine used throughout pregnancy.

**Ms. A was a 40-year-old obese woman (gravida I, para 0) with intermittent hypertension, a family history of diabetes, and a 24-year history of schizophrenia. She had had more than 30 psychiatric hospitalizations and two suicide attempts. Previous pharmacological treatment included nine oral conventional antipsychotics, fluphenazine decanoate, haloperidol decanoate, and risperidone. She had never taken clozapine. After receiving a full explanation and giving her written informed consent, she enrolled in an open-label olanzapine trial and was stabilized with 20 mg/day of olanzapine. Over the next 12 months her psychopathology significantly improved, and she began to work part-time while attending adult education classes. She experienced normal menstruation and used condoms for birth control. Ms. A had been in a stable marital relationship for the past 5 years, and she and her husband wanted to have a child. They received counseling regarding the risks versus benefits of antipsychotic therapy during pregnancy.**

**Fifteen months after beginning treatment with olanzapine, Ms. A became pregnant and withdrew from the trial. The decision was made to maintain olanzapine therapy throughout the pregnancy as the risk to the mother and fetus from her schizophrenia were felt to exceed the risk of drug treatment. After 1 month Ms. A's olanzapine dose was decreased to 15 mg/day because of excessive sedation, and that dose was maintained throughout her pregnancy. She gained 36 lb in the first trimester, with a total of 79 lb gained by delivery. Gestation was complicated by the development of pregnancy-related hypertension at 24 weeks and gestational diabetes at 26 weeks. Preeclampsia was diagnosed at 29 weeks because of persistent hypertension, substantial proteinuria (3+ to 4+ on a urine glucose scale of 0=negative to 4+=severe), and elevated liver function test results. Ms. A was hospitalized and a primary low-transverse caesarean section was performed 5 days after admission. At 30 weeks a viable female infant was delivered, weighing 4 lb, 11 oz, with Apgar scores of 7 at 1 minute and 9 at 5 minutes. Ms. A did well postoperatively and was discharged 4 days after delivery. Although her obstetrical team did not attribute her difficulties to olanzapine treatment, the possible contribution of the medication to her complications of pregnancy cannot be ruled out. It is noteworthy that Ms. A experienced no exacerbation of psychosis throughout gestation, during hospitalization, or in the postpartum period.**

With the widespread use of atypical antipsychotics, the issue of prescribing, withholding, or substituting conventional antipsychotics for atypical medications when mentally ill women become pregnant is a pressing clinical issue. We hope our case report contributes to the existing body of knowledge on this topic.


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## Acupuncture and Neuropathy

Acupuncture, initially developed in Chinese medicine in the fifth century B.C., has been increasingly applied to the alleviation of pain, particularly in the presence of cancer (1, 2). Serotonergic pathways have been implicated in pain relief, and they have been useful in relieving discomfort in both fibromyalgia and neuropathy (3, 4). Thus, it was hypothesized that acupuncture might work synergistically with serotonergic therapy for pain relief in neuropathy. Here, three cases show the possible synergism of serotonin (5-HT) effects induced by nefazodone with acupuncture. Each patient underwent six acupuncture treatments, two at each visit. Baseline platelet 5-HT content was measured once at baseline, once after 8 weeks, and three times during the acupuncture series. The patients were not specifically followed beyond the course of the acupuncture treatments.

**Mr. A was a 57-year-old man who improved somewhat after treatment with 450 mg/day of nefazodone over 8**

**weeks for diabetic neuropathy. He obtained substantial further benefit with the addition of acupuncture: his self-ratings of pain, paresthesia, and numbness all fell from 50 to 5 on a visual analog scale. Physician ratings for paresthesia fell from 1.5 to 0; ratings for numbness decreased from 1.5 to 0.5. His baseline platelet 5-HT content was 40.1 ng/$10^8$ platelets and increased to 73.7 by the end of 8 weeks of nefazodone treatment. During acupuncture treatment, it continued to rise to 95.1 and 102.4 before falling to 49.8 some time after completion of the series.**

**Mr. B was a 56-year-old man who also improved somewhat after treatment with 450 mg/day of nefazodone over 8 weeks for diabetic neuropathy. The addition of acupuncture to his nefazodone treatment produced additional improvement. His visual analog scale rating for pain fell from 55 to 25, and his rating for paresthesia fell from 80 to 55. Physician ratings for pain fell from 1.5 to 0.5, and ratings for paresthesia fell from 1.5 to 1.0. Mr. B's baseline platelet 5-HT content was 44.7 and rose only to 47.6 during his initial nefazodone treatment. It increased to 110.9 and 124.4 when acupuncture treatment was added but fell to 51.0 after treatment.**

**Mr. C was a 61-year-old man who obtained minimal benefit from an initial course of nefazodone at a dose of 450 mg/day for diabetic neuropathy. During his acupuncture treatment, he obtained little added benefit. His visual analog scale ratings decreased only from 60 to 50 for pain, paresthesia, and numbness. Physician ratings for pain and paresthesia fell only from 1.5 to 1.0. Mr. C's change in platelet 5-HT content was completely different from those of Mr. A and Mr. B. His baseline platelet 5-HT content was 28.3, at the end of the first 8 weeks of treatment it was 12.3, and during acupuncture treatment it was 11.9, 10.6, and 12.6.**

Thus, two of the three patients showed increased benefit when a series of six acupuncture sessions was added to ongoing nefazodone therapy for the treatment of diabetic neuropathy. It was reported during a follow-up telephone conversation that acupuncture benefits for the first two patients lasted at least an additional 6 months. The maximum benefit was shown by the individual who showed a platelet 5-HT content pattern of gradual increases, intermediate benefit was obtained by the individual whose platelet 5-HT content increased only during acupuncture treatment, and the least benefit was received by the individual whose platelet 5-HT content remained low during treatment. This effect may have significant implications for the effect of nefazodone on postsynaptic 5-HT receptors in alleviating pain, in conjunction with serotonin's facilitatory role in acupuncture analgesia (5). More study is required in this area.

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## Limbic Encephalitis and Late-Onset Psychosis

The patient with late-onset psychosis described in a recent Clinical Case Conference (1) had a constellation of signs and symptoms suggestive of the syndrome of paraneoplastic encephalomyelitis and sensory neuropathy. One of its signs is limbic encephalitis. This may manifest in hallucinations, paranoia, or mood disorders with or without memory loss and complex partial seizures. Paraneoplastic encephalomyelitis is often associated with sensory neuropathy with distal symmetric sensory loss, as suggested by the results of this patient's neurological examination. Paraneoplastic encephalomyelitis/sensory neuropathy is often associated with the presence of the anti-Hu antibody in serum and CSF. The syndrome may be progressive, or it may stabilize. It has in some cases been reported to remit with the treatment of an underlying malignancy (2).

Of 71 patients with paraneoplastic encephalomyelitis/sensory neuropathy and anti-Hu antibodies, 77.5% (N=55) were eventually diagnosed with small-cell lung cancer. In 12.7% (N=9), no tumor was found. The remainder of the patients (N=7) had a variety of other tumors, including prostate cancer, one case of which was discovered only at autopsy (3).

I would not agree that the neurologic findings as described were generally consistent with age. Tremor, rigidity, and postural instability are likely attributable to examination after treatment with neuroleptics (although symptoms of parkinsonism have also been reported in paraneoplastic encephalomyelitis/sensory neuropathy) (3). However, decreased distal sensation and lower extremity reflexes in the setting of "sensory ataxia" and a "slapping gait" are highly suggestive of peripheral neuropathy and are not attributable to neuroleptics or to normal aging. Also, the snout reflex is not a normal reflex in an adult. The temporal characteristics of symptom onset are not explicit in the report, but a clear subacute onset would favor the diagnosis of paraneoplastic encephalomyelitis/sensory neuropathy. The duration of symptoms is neither supportive nor incompatible with such a diagnosis.

Abnormalities may be found from magnetic resonance imaging (in a minority of cases), EEG, electromyogram, and CSF studies and serum tests for the anti-Hu antibody (3). (Since the submission of my original letter, several other antineuronal antibodies have been identified, and they are described in the second 2000 edition of *Neurobase.*) Syndromic presentation should prompt a workup for malignancy. Treatment should be directed at any underlying malignancy, seizures, or psychiatric symptoms. Also, some patients, particularly those with limbic encephalitis, may benefit from steroids, plasmapheresis, or intravenous immunoglobulin.

In this patient's case, it is likely that he would decline invasive diagnostic evaluation. The capacity to decline evaluation and treatment requires patient insight into his or her condition and an understanding of the consequences of refusing

treatment. The patient is described as understanding the risks and benefits of declining evaluation of his prostate nodule. However, the delusional nature of his paranoia might be considered to preclude capacity for evaluation of his abnormal mental status. Obviously, these ethical issues are complex.

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## Dr. Ho and Colleagues Reply

We thank Dr. Boylan for a very interesting perspective on our Clinical Case Conference, which described a patient with late-onset psychosis. This patient had no psychiatric history until age 60, when he developed paranoid delusions and auditory hallucinations. He has been followed for several years. He continues to meet the DSM-IV criteria for schizophrenia but not for psychosis secondary to a general medical condition or any other DSM-IV category. Repeated neuropsychological testing has not suggested dementia. Recently the patient was found to have a prostate nodule and an elevated prostate-specific antigen count; however, he has refused further workup. Dr. Boylan suggests that he may have paraneoplastic encephalomyelitis/sensory neuropathy on the basis of a neurological examination and a possibility of prostate cancer. Dr. Boylan presents an excellent brief review of the condition. As a result of her letter, the original three authors asked an independent neurologist (G.J.H.) to review the patient's records for a neurological diagnosis. Unfortunately, the patient was unavailable for an examination or laboratory workup at this time.

Paraneoplastic neurologic syndromes are rare and occur at a rate of 1% or less (1; Dalmau et al., 1992). We believe that it is difficult to make a diagnosis of paraneoplastic encephalomyelitis/sensory neuropathy, which is characterized by the presence of small-cell lung cancer, along with clinical signs and symptoms of progressive dysfunction in various parts of the nervous system, including the cerebral hemispheres, brainstem, spinal cord, dorsal root ganglia, and nerve roots. It is believed that such dysfunction is a result of inflammatory changes associated with deposits of the anti-Hu antibody (Dalmau et al., 1992). Nonetheless, as Dr. Boylan points out, the clinical presentation of paraneoplastic encephalomyelitis/sensory neuropathy may vary considerably.

If our patient were to have paraneoplastic encephalomyelitis/sensory neuropathy, there could be two possible diagnoses. One possibility is that the entire clinical presentation, including late-onset psychosis, could be secondary to paraneoplastic encephalomyelitis/sensory neuropathy; alternatively, the patient may have late-onset schizophrenia along with paraneoplastic encephalomyelitis/sensory neuropathy. The first scenario is highly unlikely since the patient has not manifested signs of progressive encephalopathy over the follow-up period of several years. His clinical course has been remarkably similar to that of most patients with chronic paranoid schizophrenia. Although psychotic symptoms have been reported in paraneoplastic encephalomyelitis/sensory neuropathy, other symptoms of encephalopathy, such as dementia, confusion, and complex partial seizures, commonly accompany them (2, 3). The presence of a snout reflex, one of the so-called frontal release signs, is abnormal but is seen commonly in elderly individuals for a multiplicity of reasons, including accumulated frontal lacunar infarcts. As such, it is not pathognomonic of any specific neurological disorder. It is conceivable that our patient has late-onset schizophrenia along with paraneoplastic encephalomyelitis/sensory neuropathy secondary to a prostate neoplasm (4). Continued follow-up is necessary to rule out this possibility.

In sum, we think it is unlikely that our patient's psychosis is secondary to paraneoplastic encephalomyelitis/sensory neuropathy given the lack of progressive neurological deficits over several years. Nonetheless, we will follow him closely and, with his permission, do a workup to rule out malignancy.

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## Stressful Life Events and Depression

We read the article by Kenneth S. Kendler, M.D., et al. (1) with great interest. The authors addressed the important issue of the extent to which stressful life events cause the onset of depression and concluded that "about one-third of the association between stressful life events and onsets of depression is noncausal." Using a database derived from the Virginia Twin Registry, the authors provided two basic results to support their conclusion: 1) dependent stressful life events (resulting from the subject's behavior) are more strongly associated with depression than are independent events (unrelated to a subject's behavior) and 2) the risk for depression associated with personal stressful life events appears to be higher in a sample of female twins than in a subsample of monozygotic twins. Although these findings appear to provide general support for the article's conclusion, the methods used raise two concerns.

First, despite the value of using the co-twin control method for assessing genetic influences, it still appears necessary to provide some formal test of significance for the observed dif-

ferences in risk among the general population, dizygotic twins, and monozygotic twins. In particular, if the difference between the reported estimated risk for depression associated with stressful life events in monozygotic twins (3.58) and the estimated risk for depression associated with stressful life events in the general population (5.64) is not statistically significant, then how can the ratio of 3.58 to 5.64 be a meaningful estimate of the degree of the causal relationship between stressful life events and depression?

Second, it appears that the technique used to assess dependent events might largely serve to relabel interpersonal events as dependent events because "for stressful life events involving interpersonal difficulties, interviewers were instructed to assume that the events were dependent unless convincing evidence to the contrary was presented." If so, then the substantial risk that the authors associated with a rating of dependence might have reflected a substantially greater risk for depression associated with interpersonal events than with noninterpersonal events. If, on the other hand, a substantial proportion of the interpersonal events were rated as independent, then the authors could have tested explicitly for differences in risk between interpersonal independent events and interpersonal dependent events. Without presenting some evidence that the risk associated with dependent events was distinct from the risk associated with interpersonal events, it does not seem possible to infer that the association between life events and depression was partially noncausal because the dependence of events was substantial.

We agree that determining the extent to which stressful life events are causally related to depression is an important clinical and genetic question. However, before even a rough estimate regarding the degree to which events are noncausal can be made, it seems essential to have a presentation of statistical tests for differences in risk for depression associated with life events among the general population, dizygotic twins, and monozygotic twins. Similarly, a clarification concerning life events classified as dependent is also fundamental to the understanding of the importance of the reported findings.

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## Dr. Kendler and Colleagues Reply

Drs. Maciejewski and Mazure raise two relevant issues about our recent article, in which we attempted to estimate the proportion of the relationship between stressful life events and the onset of major depression that was causal. First, they ask whether the odds ratios (not, as they state, the estimated risk for depression) for the association between stressful life events and major depression were significantly different between the monozygotic twins and the general population. It is not possible to formally test these two odds ratios because they were derived from quite different calculations—the first from a co-twin control analysis of twin pairs and the second from an individual-wise analysis of the entire sample, which included the monozygotic twin pairs. We can, however, compare the standard error of these estimates or, more precisely, of the regression coefficients (βs) from which the odds ratios were calculated. For the monozygotic twins, the β for predicting a depressive onset from the occurrence of a personal stressful life event was 1.28 (SD=0.20), whereas the parallel estimate for the entire sample was 1.73 (SD=0.13). Thus, the standard errors of the regression coefficients were far from overlapping, suggesting that these parameter estimates are meaningfully different.

Second, Drs. Maciejewski and Mazure are concerned that our method of assessment may serve to label interpersonal events as dependent. Our instructions to the interviewers were merely commonsense advice: 1) it usually "takes two to tango," in that most interpersonal conflicts emerge from active interactions between two parties, and 2) when describing interpersonal conflict, many reporters have a bias toward thinking that it is entirely the other person's fault. This did not result in our interviewers being unwilling to ascribe interpersonal events as independent. For example, in the categories of "serious trouble getting along with an individual" in their network and "serious marital problems," 9.6% and 14.9% of the events, respectively, were rated as probably or definitely independent.

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## More Questions About Recovered Memories

A recent article (1) purported to "provide further evidence supporting the occurrence of amnesia for childhood traumatic experiences and the subsequent recovery of memory" (p. 754). In a key but methodologically problematic finding, "[a] majority of participants were able to find strong corroboration of their recovered memories" (p. 749). This "strikingly high" corroboration rate, however, was based on self-reported information recalled by the participants and accepted as recounted. Among 19 participants claiming complete amnesia who had attempted to confirm memories of sexual abuse, 89% (N=17) provided "corroborations" consisting of their memory of "verbal validation" alone (p. 753).

Although the authors acknowledged that a "major methodological limitation" of the study was the fact that "retrospective…self-reports were potentially subject to distortion and inaccuracies" (p. 754), there was no assessment describing the nature and quality of the self-reported corroborations, which would appear crucial to drawing conclusions about the veridicality of the recovered memories. Since retrospective verbal self-reports might have included pseudocorroborations representing confirmation bias, suggestion and belief paradigms, situational demand characteristics, and source amnesia (2, 3), the high corroboration rates could bespeak pseudomemories or screen memories masking other trauma (4).

Furthermore, even if "grossly improper therapeutic practices" (1, p. 754) were not a significant factor in memory recovery, unintended suggestive influences within the study itself may have biased the findings. Participants were asked "if there was a period during which they 'did not remember that this [traumatic] experience happened'" (p. 751). With this question alone, the actuality of the traumatic experience was

inherently validated by the investigators, and the experience of not remembering it was implicitly suggested. The fact that participants were recruited from a unit specializing in the treatment of posttraumatic and dissociative disorders could mean that suggestive influences and affiliative needs swayed group answers (3, p. 58). Questions about the "circumstances of first recovered memory" (1, p. 751) may have elicited autosuggestive responses. There apparently were no control questions or conditions. Ordinarily, patients might be confused about whether their recall of early traumatic experience is veridical (2, 4), yet the report does not indicate if participants ever had any doubt whether the events of the recovered memories actually occurred as remembered.

That "the vast majority of participants…did not recall any overt suggestion before the first recovered memory" (1, p. 752) does not rule out direct or indirect suggestive influence, whether inside or outside therapy sessions (2). Reading popular books, viewing or reading media, or talking with others on the subject of recovered memory may have influenced recollection. The actual time of suggestive effect could have followed the recalled time of recovery, which unwittingly may have been temporally displaced for narrative consistency.

These comments do not dispute the possibility of amnesia for traumatic experience that is later recalled or the discovery of information that confirms the veridicality of the memory. Consistent with suitable clinical technique, the retrieval of independent data is essential for investigating the objective-versus-subjective truth of early memories (5). However, without corroborative detail for readers to trace the study's conclusions, generalizations about recovered memories hinging solely on self-reported "actual independent confirmation" (p. 753) should be viewed with scientific skepticism.

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The article by Dr. Chu et al. suffers from several methodological deficiencies. Several assertions merit responses.

1. The authors asserted, "Clinical research generally has supported the concepts of dissociative amnesia…in relation to traumatic events" (p. 750). This statement is simply untrue. Pope and colleagues (1) have demonstrated that not a single study in the literature provides methodologically sound evidence for this phenomenon.

2. Terr's investigations of traumatized children demonstrate the differential effects on memory, depending on the chronicity of abuse. The authors failed to note, however, that Terr's chronically abused group was significantly younger than her group with a single episode of abuse (2). Her results may have thus merely reflected normal childhood amnesia.

3. The authors said that they independently confirmed the participants' abuse. This is inaccurate.

First, answers to the question "Have you had anyone confirm these events?" (p. 751) are meaningless unless respondents know exactly what kind of information from others would—and would not—serve as confirmation. The authors failed to indicate what they told their participants about this critical point. Second, the authors believe that bodily scars could confirm a history of suspected abuse. This argument is illogical; a scar can result from a wide variety of injuries. Third, because Dr. Chu and colleagues mentioned nothing about personally examining the participants' physical evidence, the reader can only wonder whether the authors simply accepted the participants' word for the existence and content of these records. (Kluft [3], who is cited in the present study, "confirmed" patients' allegations by accepting the patients' own accounts of their confirmations and of the confessions abusers had allegedly made.)

These deficiencies severely undermine the authors' contention that their "criteria for confirmation were relatively stringent" (p. 753). Rather, the authors provided no specific, operationally defined criteria for confirmation at all.

4. The authors state that losing memory for whole periods of the respondents' lives indicated a "massive failure to integrate entire periods of childhood" (p. 753). This assertion violates the time-honored maxim that novel explanations for a phenomenon must not be advanced unless well-established, simpler explanations have been excluded. Here, there is a simple and entirely reasonable explanation for the participants' lack of recall: namely, that nothing particularly memorable occurred during their childhoods. In other words, demonstrating traumatic amnesia requires excluding the possibility that the participants merely forgot unremarkable events. Dr. Chu and colleagues failed this requirement.

Finally, the authors implied that self-reported amnesia is synonymous with dissociative amnesia. But as numerous writers have noted, nonreporting of an event does not necessarily mean it was forgotten. Dr. Chu and colleagues have therefore failed to prove their participants were amnestic.

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In their recent study of 90 female patients suffering from trauma-related psychopathology, Dr. Chu et al. noted that "higher dissociative symptoms were correlated with early age

at onset of physical and sexual abuse and more frequent sexual abuse." As in many previous studies, the authors employed the Dissociative Experiences Scale to measure dissociative symptoms. The authors also found that self-reports of partial or complete amnesia for traumatic experiences were related to elevated scores on the Dissociative Experiences Scale. Although the authors did acknowledge that their study relied on patients' self-reports of dissociation, trauma, and amnesia, my impression is that they underestimated the potential problems that may occur with such measures. Of particular relevance in this context is recent work on the psychological correlates of the Dissociative Experiences Scale (1). This work shows that there is a substantial overlap between the Dissociative Experiences Scale and questionnaires measuring proneness to fantasy (1). As well, there is now solid evidence indicating that high scores on the Dissociative Experiences Scale are closely related to a positive response bias in retrospective self-report scales asking for trivial "bad things" (e.g., "I have been shortchanged in stores") (2) or even relatively neutral, but highly specific life events (e.g., "I went with my school to Disneyland") (3). Furthermore, individuals with high scores on the Dissociative Experiences Scale are more receptive to subtle misinformation when answering questions about a narrative that they heard earlier than are individuals scoring low on the scale (4). Similarly, the Dissociative Experiences Scale appears to be a powerful predictor of the vulnerability to developing pseudomemories in response to misleading autobiographical cues (5). Finally, receiving high scores on the Dissociative Experiences Scale correlates positively with reports of supernatural experiences (e.g., telepathy, precognition) (6). Taken together, these findings point to the conclusion that self-reports of individuals scoring high on the Dissociative Experiences Scale may contain exaggerations, distortions, and confabulations. This may explain why at least one study (7) found high scores on the Dissociative Experiences Scale to be related to self-reports of trauma but not to sexual abuse ratings based on hospital records.

In my opinion, then, the findings reported by Dr. Chu and co-workers are difficult to interpret precisely because all their pertinent comparisons and correlations involved individuals with high scores on the Dissociative Experiences Scale (8). Although it should be admitted that it is often impossible to avoid retrospective self-reports, studies like that of Dr. Chu and associates would allow for more convincing conclusions if they included a measure of response bias. With such a measure, it would be possible to statistically correct for the effects of liberal reporting criteria.

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Dr. Chu and colleagues claimed that their study "strongly suggests…that independent corroboration of recovered memories of abuse is often present" (p. 749). What they actually found, however, was that many patients reported finding some type of corroborative evidence for at least some of their recovered memories. It remains plausible that the vast majority of recovered memories (some patients reported over 100 abusive episodes) were never corroborated. As well, much of the corroborative evidence that was obtained may have been largely circumstantial. For example, the authors accepted reports of physical scars as evidence of abuse, yet scars have also been offered as evidence for recovered memories of alien abductions (1)! Claims of corroborative evidence must therefore be examined carefully (indeed, even confessions by alleged perpetrators might sometimes be false if they have been obtained in a context of high stress and extreme social pressure [2]).

The authors also concluded that "psychotherapy usually is not associated with memory recovery" (p. 749), partly because few patients reported recovering memories during therapy sessions. But this conclusion is based on the unwarranted assumption that suggestive effects are immediate. Rather, experimental demonstrations of false memory induction indicate that several days are often required for such memories to become established [3]. These suggestions made during therapy could quite conceivably result in false memories arising outside of therapy.

Most patients also reported that before memory recovery, they had never received an explicit suggestion from anyone that they had been abused. If, however, the patients had already made a strong commitment to the validity of their memories, they may have been strongly motivated to deny any suggestive influence. Furthermore, explicit suggestions of trauma might actually be less effective in inducing false memories than more subtle suggestions (including such as might arise through exposure to movies or articles about recovered memories), in that explicit suggestions can be more easily identified by recipients as the cause of their recovered "memories" (1, 4). In other words, memories implanted through subtle suggestions are more likely to be perceived—by both therapist and patient—as internally generated and hence valid.

In conclusion, the study by Dr. Chu and colleagues does little to alleviate concerns that recovered memories of abuse are often false.

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Dr. Chu et al. wrote about dissociation, amnesia, and childhood abuse and mentioned a "heated debate" about these topics. However, they presented only one-half of that debate, largely omitted the serious criticisms that have been directed at the point of view they espouse, and blandly stated suspect opinions. This selective emphasis was particularly noteworthy in that the authors presented material regarding a patient population that might well have been subject to the same drawbacks as earlier studies in this field.

The article by Dr. Chu et al. stretches credibility by describing a remarkably skewed clinical population. Only 11 patients were said to have suffered from fewer than 10 episodes of sexual abuse, 28 suffered 10–100 episodes, and 31 suffered more than 100 episodes (their Table 3). Such cases may exist, but the authors must have gathered together a truly large number of exceptional individuals in order to accumulate so many with such long-sustained and repeated episodes of sexual abuse and amnesia for the events.

Dr. Chu et al. claimed a "corroboration rate" of 89% of the participants who "recovered" memories after complete amnesia for sexual abuse and said that this rate is similar to that reported by Herman and Schatzow (83%). The claims made by Herman and Schatzow have long been challenged. They were based on a heterogeneous group in which individuals who claimed continuous memory were mixed with others who were "finding" memories and then "confirming" them. Those patients had group treatment in which one of the goals was to "recover memories." The enormous harm done by that process has been thoroughly documented (1).

Another psychiatrist from Dr. Chu's institution has pointed to flaws in the notion of recovered memory (2). The failure of Dr. Chu et al. to answer the trenchant criticisms of Pope (2) is a telling omission. The description given by Dr. Chu et al. of the proof that they said their patients found indicates inadequate scrutiny of the claims made, whereas the strongest clinical association of their multiple claims of abuse seems likely to be suggestive therapy or consorting with those who have been submitted to it.

This deeply flawed article misrepresents the literature, misunderstands the nature of proof, and mistakes belief for evidence. We must assume that it passed peer review, which is a very damaging reflection on the quality and impartiality of your review process.

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## Dr. Chu Replies

Given the level of polarized controversy about the issue of recovered memory of childhood abuse, it is not surprising that there have been numerous commentaries on our research. Our study was motivated by the need to further investigate the circumstances of patients' reports of having recovered memories of childhood abuse. Contrary to the implications of at least one of the critics of our report, we attempted to find some middle ground between those who essentially reject any evidence of traumatic amnesia and recovered memory and those who uncritically accept and validate all patient accounts of childhood abuse. In our investigation we simply sought to replicate previous findings and to test the hypothesis that chronic abuse beginning in early childhood is related to higher levels of dissociative symptoms in adulthood, including amnesia. As in our previous review of this complex subject (1), we endeavored to present a balanced discussion of multiple viewpoints concerning traumatic amnesia.

We made no assertion that any of our research participants corroborated all of their abuse memories—a formidable task indeed, as patients in our programs frequently report long-standing childhood abuse that occurred over years (2, 3). We certainly do not claim to have "proved" that the remembered abuse occurred. This level of confirmation was not only beyond the scope of our investigation but in many cases would have been impossible in that intrafamilial abuse commonly occurs behind closed doors. Given these difficulties, we found it striking that such a high percentage of those who tried to obtain corroboration were able to do so for some of their abuse experiences. We did not rely primarily on scars as evidence for corroboration. In fact, the most common form of corroboration was verbal validation. We do believe that our criteria were relatively stringent, requiring that other individuals report that they knew (rather than believed) that the remembered abuse had occurred. The verbal corroboration was surprisingly high: 13 of 14 cases for physical abuse and 17 of 19 cases for sexual abuse.

Several of the letters regarding our article address the methodological difficulties of the use of participant self-reports in our research (which is one of the primary criticisms of Pope and Hudson [4] in their review of clinical research in this area). In our article we acknowledged the limitations of our study, including the reliance on patients' self-reports concerning possible abuse and corroboration and the difficulties of determining whether subtle suggestion had been a part of the patients' psychotherapy. However, the results of our study and similar studies should not be dismissed out of hand for methodological reasons. After all, self-report is a routine and accepted methodology for clinical research in which patients are asked describe a wide variety of variables such as mood and other psychiatric symptoms, perceptions, and life events. Although it is true that patients' self-reports can be influenced by errors in recall, suggestion, study design, and conta-

gion in treatment settings, we doubt that the cumulative clinical research in this area can be completely misguided and mistaken. As noted by Scheflin and Brown (5), who reviewed 25 studies of traumatic amnesia, "Partial or full amnesia was found across studies regardless of whether the sample was clinical, nonclinical, random or nonrandom, or whether the study was retrospective or prospective. Every known study has found amnesia for childhood sexual abuse in at least a portion of the sampled individuals" (pp. 178–179).

We were puzzled by one detractor of our study who contends that a more reasonable explanation for total amnesia for whole periods of childhood would be no recall because of no abuse and an unremarkable childhood. Such an explanation presupposes that all the reports of abuse were untrue and that it is normal for individuals to forget all the events for these periods. Although very few individuals have detailed memories of childhood events, we find it significant when patients report no memory of such important experiences as school, birthdays, holidays, and special occasions. We also do not agree that nonreporting of abuse was a factor in this study unless many of the participants, when asked directly, deliberately withheld information and misrepresented their previous inability to recall abuse experiences.

Our article reported not only that few of our participants were in therapy sessions when they first recovered memories of abuse but also that approximately half of the participants were not currently in any kind of mental health treatment when they first recovered memories, making suggestion unlikely in these cases. Numerous participants reported that they recovered memories of abuse before treatment and that these memories were the reason for beginning therapy.

Dr. Merckelbach's letter raises some interesting and pertinent issues concerning individuals who have elevated scores on the Dissociative Experiences Scale. As he notes, some recent studies have also demonstrated that such individuals are more fantasy prone and suggestible. These findings are entirely consistent with research that demonstrates that the innate capacity to dissociate varies considerably (6), and it may well be the case that those with a high ability to dissociate have a heightened ability to use fantasy and imagination. Our results imply that individuals with a high dissociative capacity maintain a high level of dissociative symptoms if they are subjected to chronic traumatization. If these individuals are also prone to the development of pseudomemories, it reinforces our cautions that "clinicians must be open to the possibility of real abuse but must allow patients to reconstruct—without suggestion—a credible personal history that is consistent with past and current symptoms" (our article, p. 754).

Response bias and the suggestion inherent in the questions we asked participants may indeed have been a factor in our results, as argued by Drs. Good and Merckelbach. However, we feel strongly that these factors do not diminish our findings. There is no evidence to suggest that a brief series of direct questions about the possibility of abuse can lead to the immediate creation of complex pseudomemories of such abuse. The format of our reporting did not permit inclusion of the richness of the participants' responses. For example, their description of confirmation by others frequently included accounts of the abuse being directly observed by others or admitted by the perpetrators, which left little doubt as to the validity of their memories.

As clinicians and clinical researchers, we are involved in the complex issues of trying to determine the etiology of the reports of child abuse that our patients present. There are certainly instances in which such reports stem from grossly inappropriate clinical practices, contamination or contagion, hysterical embellishment, or even malingering. Some such false positive reports may well have been included in our study, as we only recorded our participants' responses. However, in both our study and our clinical practice, many reports of abuse and recovered memories appear to be authentic, credible, and internally consistent with patients' past histories and current symptoms. In this context, our study adds some balance to the public and professional debate that sometimes seems to emphasize false memory more than true memory of childhood trauma.

It is striking that one letter expresses incredulity that a specialized trauma treatment unit might customarily house many chronically and multiply traumatized patients. Although it may not be commonplace, it is far from rare for some children from disrupted and chaotic families to have been assaulted and/or molested dozens or even hundreds of times. At a time when more than 1.5 million American children are documented to have been moderately or severely damaged by abuse and neglect each year (7), we feel strongly that research into the prevalence and effects of childhood abuse (including traumatic amnesia) is necessary and warranted. We hope that our study is only a preliminary step to further research on severe childhood trauma and the treatment of its sequelae. Although our study cannot be described as conclusive or definitive, it does underscore the presence and aftereffects of the still underreported and often-denied reality of child abuse in American society.

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