Letters to the Editor

Tramadol-Induced Mania

TO THE EDITOR: Stimulants and antidepressants have been shown to induce manic episodes in patients with bipolar disorder (1). Tramadol is a novel centrally acting synthetic analgesic unrelated to opiates. Its proposed mechanism of action is the binding of μ-opioid receptors and the inhibition of serotonin and norepinephrine reuptake. To our knowledge, no cases of mania associated with tramadol have been reported; we report here a possible episode of tramadol-induced mania.

Ms. A was a 27-year-old woman with a 2-year history of bipolar disorder. Her initial manic episode occurred while she was taking prednisone, 20 mg/day, for an upper respiratory infection. She was hospitalized for 4 weeks; her manic symptoms were managed with a regimen of valproic acid, 2000 mg/day; carbamazepine, 400 mg/day; and haloperidol, 8 mg/day. The valproic acid and haloperidol regimens were tapered and discontinued 6 months after discharge because of nausea, sedation, and concern about extrapyramidal side effects. Ms. A was maintained on a regimen of carbamazepine for 15 months and experienced no manic or depressive symptoms. She subsequently discontinued carbamazepine treatment because of mild sedation and denial of illness. She remained asymptomatic for 5 months, during which time she received no mood stabilizers or nonpharmacological therapy.

After a motor vehicle accident, Ms. A was prescribed tramadol, 100 mg t.i.d., as needed for relief of lower back pain. She had previously taken opioid analgesics and had experienced no adverse effects. By the fourth day of tramadol treatment (200 mg/day), she had marked insomnia that was characterized by only 3 hours of sleep each night. In addition, she described feeling "hyper" and "keyed up." She also demonstrated rapid speech, euphoric mood, grandiose delusions, and greater psychomotor activity. She agreed to discontinue the tramadol treatment and restart a regimen of carbamazepine, 400 mg/day. Within 2 weeks, she was completely euthymic.

It is possible that this patient experienced a natural recurrence of her mania. The temporal relationship of her symptoms to the tramadol use is intriguing, particularly since her initial manic episode was apparently medication induced. Pre-clinical studies have demonstrated that tramadol blocks norepinephrine reuptake (2). In addition, the manufacturer's product information indicates that tramadol is a serotonin reuptake inhibitor. Thus, it is possible that tramadol might induce mania in a manner similar to that of antidepressants. A recent open investigation suggested that tramadol may be a useful antidepressant augmentation agent, particularly in patients with pain syndromes (unpublished 1996 study of J. Fanelli and C. Montgomery). Although a recent case report suggested that tramadol worsened depressive symptoms in a patient who had been previously stabilized with antidepressant medication (3), we believe the pharmacological effects of tramadol, including its potential to induce mania, may be important when considering its use in patients with mood disorders.

REFERENCES

1. Wehr TA, Goodwin FK: Can antidepressants cause mania and worsen the course of affective illness? Am J Psychiatry 1987; 144: 1403–1411
2. Reimann W, Hennies HH: Inhibition of spinal noradrenaline uptake in rats by the centrally acting analgesic tramadol. Biochem Pharmacol 1994; 47:2289–2293
3. Pinkofsky HB, Woodward RA, Reeves RR: Mood alterations and tramadol (letter). Am J Psychiatry 1996; 153:843–844

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Psychotic Episode Associated With Dexfenfluramine

TO THE EDITOR: Dexfenfluramine has recently been introduced for the treatment of obesity. Dexfenfluramine exerts its anorectic effect by causing a blockade of presynaptic serotonin release and reuptake. Although other adverse central nervous system effects such as fatigue, drowsiness, impaired concentration, insomnia, and headache have been reported (1–5), a literature search revealed no cases of psychotic episodes associated with dexfenfluramine use; we report here such a case.

Mr. A was a white, 48-year-old man with major depression. He had a history of alcohol and cannabis abuse but had been sober and drug free for 2 years. He had never experienced major depression until 1 year earlier, at which time he experienced feelings of hopelessness, impaired concentration, and severe fatigue. He also gained more than 35 pounds within several months. He was placed on a regimen of doxepin, 200 mg/day, to which he responded well, without any side effects. Two months before admission to our unit, Mr. A's primary care physician had placed him on a regimen of dexfenfluramine, 30 mg/day, for treatment of obesity after several unsuccessful dieting attempts. Although he initially experienced insomnia for about 2 weeks, Mr. A's sleep pattern eventually had returned to normal.

On the day that Mr. A was admitted to the psychiatric emergency room, he had become increasingly paranoid. He blamed family members and the government for taking part in a conspiracy against him and the citizens of the United States. He eventually walked into a federal building in the neighborhood and asked to see the visitor's log. When his request was denied by the security personnel, he became violent. Mr. A's extreme agitation and combativeness at the time of admission necessitated the use of four-point restraints. He was responding to internal stimuli and blaming the medical staff for being a part of the conspiracy against him. He had pressured speech and exhibited grandiose and

paranoid delusions, e.g., "I knew the truth about what happened to that airplane." He responded well to a 5-mg intramuscular dose of droperidol that was administered shortly after admission. A urine toxicology screen and a blood alcohol test revealed that no alcohol or illicit drugs had been ingested. No electrolyte imbalance was determined from routine laboratory tests. There was no history of psychotic disorder in the family. Mr. A was given intramuscular haloperidol, 2 mg b.i.d., during the subsequent 24 hours of his hospital stay. He was discharged, and his symptoms resolved completely within 72 hours after dexfenfluramine discontinuation. He was able to go back to work within a week.

In this report we described a psychotic episode associated with dexfenfluramine use in a patient with major depression. The patient never had any psychotic disorder before the current episode. This episode could have been a rare adverse effect of dexfenfluramine alone or the result of an interaction between dexfenfluramine and doxepin, a tricyclic antidepressant agent that inhibits the reuptake of norepinephrine into the nerve terminals at the synaptic level. Considering the size and the high motivation of the target population, the pharmacologic actions, drug interactions, and adverse effect profile of dexfenfluramine need to be reevaluated. Even more important is the responsibility of all physicians to closely follow their patients after prescribing a newly approved drug.

REFERENCES

1. Turner P: Dexfenfluramine: its place in weight control. Drugs 1990; 39(suppl 3):53–62
2. Campbell DB: Dexfenfluramine: an overview of its mechanisms of action. Reviews in Contemporary Pharmacology 1991; 2:93–113
3. Garattini S, Mennini T, Samanin R: From fenfluramine racemate to d-fenfluramine: specificity and potency of the effects on the serotoninergic system and food intake. Ann NY Acad Sci 1987; 499:156–166
4. McTavish D, Heel RC: Dexfenfluramine: a review of its pharmacological properties and therapeutic potential in obesity. Drugs 1992; 43:713–733
5. Finer N, Craddock L, Lavielle R, Keen H: Prolonged weight loss with dexfenfluramine treatment in obese patients. Diabete Metab 1987; 13:598–602

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Treatment of Psychotic Depression

TO THE EDITOR: A number of case reports have suggested that clozapine, with its broad pharmacological activity, may be useful in the treatment of psychotic depression. Parsa et al. (1) found clozapine to be useful and well tolerated in a patient with Parkinson's disease for whom the use of a standard neuroleptic had been problematic. Likewise, Dassa et al. (2) found that clozapine was effective in treating psychotic depression in a 40-year-old patient for whom conventional treatments, including ECT, had been ineffective. Olanzapine shows many of the pharmacological properties of clozapine but has a better side effect profile (3). We present a case that suggests that, like clozapine, olanzapine monotherapy may be an effective treatment for psychotic depression.

Mr. A was a white, 59-year-old man whose approximately 6-year history of depression (characterized by despondency, fatigue, anhedonia, crying bouts, and anorexia) followed the death of his mother; he had never sought treatment. Approximately 2 months before seeking consultation, he began to experience increasingly severe paranoid delusions that he was being monitored by video cameras in his home, that his food was being tampered with, that he was being followed, and that his life might be in danger. His baseline scores on the 21-item Hamilton depression scale and the Brief Psychiatric Rating Scale (BPRS) were 33 and 73, respectively.

Results of routine laboratory analyses revealed no abnormality, and the medical history was unremarkable. Various treatment options were reviewed, and olanzapine was suggested for treatment of the prominent psychotic symptoms because of its favorable side effect profile. The plan was to initiate olanzapine treatment and subsequently add an antidepressant as needed. Mr. A had been reluctant to take medication but consented to start a regimen of olanzapine, 5 mg/day. There was no significant change in the depressive or psychotic symptoms after 1 week, so the dose was increased to 10 mg/day. The drug was well tolerated, with only mild dizziness and nausea noted at the higher dose. Orthostatic blood pressure was within normal limits. Four weeks later, Mr. A began to question if he was really being monitored by other people or whether it was all in his mind. He was less motorically retarded and more verbal and reported feeling better. By week 6, his Hamilton depression scale score had dropped to 13, and his BPRS score fell to 38. Beck Depression Inventory scores fell approximately 34% during this period. His family confirmed substantial improvement in Mr. A's activity level, mood, and paranoia.

There are several possible explanations for the efficacy of olanzapine monotherapy in this patient. Olanzapine's pharmacological properties include antagonism of dopamine (D_1–D_4) receptors, as well as serotonin ($5\text{-}HT_2$), muscarinic, α-1, and histamine receptors (3). It is conceivable that olanzapine, with its moderate $5\text{-}HT_2$ antagonism and broad dopamine antagonism, has both primary antidepressant effects and antipsychotic effects.

On the other hand, the marked improvement in depression may have been largely secondary to the improvement in psychotic symptoms. As the paranoia gradually subsided, the patient became less anxious and more hopeful and generally felt better. In contrast to the rater's assessment, the patient felt that "thinking" had benefited more than mood but endorsed improvement in both areas. It is conceivable that continued benefits will result with longer treatment. Given its broad pharmacological activity and relatively benign side effect profile (4), olanzapine appears to warrant further research in the treatment of psychotic depression.

REFERENCES

1. Parsa MA, Ramirez LF, Loula EC, Meltzer HY: Effect of clozapine on psychotic depression and parkinsonism (letter). J Clin Psychopharmacol 1991; 11:330–331
2. Dassa D, Kaladjian A, Azorin JM, Giudicelli S: Clozapine in the treatment of psychotic refractory depression. Br J Psychiatry 1993; 163:822–824
3. Moore NA, Tye NC, Axton MS, Risius FC: The behavioral pharmacology of olanzapine, a novel "atypical" antipsychotic agent. J Pharmacol Exp Ther 1992; 262:545–551
4. Baldwin DS, Montgomery SA: First clinical experience with olanzapine (LY 170053): results of an open-label safety and dose-

ranging study in patients with schizophrenia. Int Clin Psychopharmacol 1995; 10:239–244

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High-Dose Olanzapine for Treatment-Refractory Schizophrenia

TO THE EDITOR: Olanzapine, an atypical antipsychotic, has been found superior in efficacy to haloperidol for the treatment of schizophrenia, with fewer extrapyramidal side effects (1, 2). The current guidelines for olanzapine use in schizophrenic patients suggest a maximum dose of 20 mg/day; the safety of doses above this level have not yet been evaluated in clinical trials. However, many patients, including those classified as having responded to olanzapine, continue to have substantial psychopathology after 6 weeks of treatment. The primary purpose of this letter is to describe the treatment-emergent side effects that result from the administration of high doses of olanzapine to patients with treatment-refractory schizophrenia.

Eight male patients with chronic schizophrenia and refractory symptoms were treated with a regimen of olanzapine that began at 5 mg/day and was titrated up to 30 mg/day in five patients and to 40 mg/day in three patients. The patients' mean age was 43 years (range=29–61), and the mean length of the current hospitalization was 96 months (range=9–211). Before assessment, patients had been receiving high-dose olanzapine for at least 12 days. Extrapyramidal side effects were assessed by using the Modified Simpson Angus Scale (3); other side effects, including blood dyscrasias, were assessed by patient interview and chart review.

Of these eight patients, five were found to have acute extrapyramidal side effects (akathisia [N=1], parkinsonism [N=2], both akathisia and parkinsonism [N=2]). All symptoms were mild; only one patient required treatment with benztropine and propranolol. One additional patient had questionable parkinsonism. Mild daytime drowsiness and dry mouth were reported by five and three patients, respectively. No other side effect was reported by more than one patient. When questioned about how side effects with olanzapine compared with all previous standard antipsychotic medications, four patients reported feeling better, three felt the same, and one felt worse.

The treating psychiatrists reported that all patients continued to manifest prominent psychotic symptoms at these doses. However, social functioning improved in four of the eight patients, with two being markedly more organized in thought and behavior.

Overall, olanzapine treatment at doses greater than 20 mg/day was well tolerated, although a greater prevalence of extrapyramidal side effects was found at these doses (4). The efficacy of olanzapine at high doses for some refractory schizophrenic patients is suggested by these cases but remains to be firmly established.

REFERENCES

1. Tollefson GD, Beasley CM Jr, Tran PV, Street JS, Krueger JA, Tamura RN, Graffeo KA, Thieme ME: Olanzapine versus haloperidol in the treatment of schizophrenia and schizoaffective and schizophreniform disorders: results of an international collaborative trial. Am J Psychiatry 1997; 154:457–465
2. Beasley CM Jr, Tollefson G, Tran P, Satterlee W, Sanger T, Hamilton S: Olanzapine versus placebo and haloperidol: acute phase results of the North American double-blind olanzapine trial. Neuropsychopharmacology 1996; 14:111–123
3. Simpson GM, Angus JWS: A rating scale for extrapyramidal side effects. Acta Psychiatr Scand Suppl 1970; 212:1–58
4. Beasly CM: Safety of olanzapine. J Clin Psychiatry Monograph 1997; 15(2):19–21

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Androgenic Activity in Autism

TO THE EDITOR: In four of 12 prepubertal autistic children (6–10 years old) in our inpatient child psychiatry department, we have observed precocious secondary sexual characteristics (growth of pubic hair, increase of testis volume) that suggest high androgenic activity in infantile autism. In addition, there are four times more male than female autistic patients (1). These observations are consistent with Geschwind and Galaburda's theory (2): excess testosterone in the fetus may cause anomalies in the development of the left hemisphere, which in turn provoke cognitive dysfunction such as that found in subjects with autism.

To test our hypothesis of a hyperandrogeny and autism association, we measured plasma testosterone and adrenal androgen in nine drug-free inpatients with DSM-IV autism and 62 normal subjects of same age, sex, weight (within 2 kg), and stage of puberty. Written informed parental consent was obtained for all subjects. The autistic group consisted of five prepubertal boys and two prepubertal girls (8–10 years old), one pubertal boy (17 years old), and one pubertal girl (13 years old). Some studies have shown a positive correlation between testosterone and aggression (3). In order to control for this variable, we divided the nine children into three groups according to their aggressive behavior exhibited during the several months before the blood drawing. Three of the children had exhibited explosive aggression against others (anger, broken objects, violence toward others). Three engaged in self-mutilations, and three demonstrated no aggression and were in a severe state of autistic withdrawal. The appearance of aggression against others was associated with having fewer of the main symptoms of autism (autistic withdrawal, stereotypies, language dysfunctions).

The comparison group, chosen from a pediatric unit, consisted of 21 prepubertal boys and 19 prepubertal girls (9–10 years old), nine pubertal boys (16–17 years old), and 13 pubertal girls (12–13 years old). All blood samples were collected at 10:00 a.m. Measures of plasma testosterone concentration and adrenal androgen were made by immunoradiometry.

Results showed that three of the nine autistic subjects had an abnormally high plasma testosterone concentration (over two standard deviations above the mean for the comparison subjects), with values above that of the highest in the comparison subjects. Among the autistic subjects, plasma testosterone concentration values (ng/ml) were 0.64 for a prepubertal 10-year-old boy, 8.8 for the pubertal 17-year-old boy, and 0.5 for the pubertal 13-year-old girl, whereas the appropriate comparison group means were 0.06 (SD=0.03, range=0.01–0.15), 5.51 (SD=1.27, range=0.27–7.50), and 0.12 (SD=0.09, range=0.01–0.25), respectively. These three children all showed aggression against others. Thus, high plasma testosterone concentrations were present in all autistic subjects who exhibited aggression against others. The 10-year-old boy exhibited pubic hair growth. The 13-year old girl had a level of adrenal androgen (4.40 ng/ml)

that was 500% higher than the mean level of the comparison subjects (mean=0.88, SD=0.39, range=0.36–1.70). The other autistic subjects showed normal adrenal androgen levels.

This is the first report of an association between abnormally high androgenic activity and aggression in subjects with autism. Although a previously reported study did not find group mean elevations in plasma testosterone in prepubertal autistic subjects (4), it appears here that in certain autistic individuals, especially those in puberty, hyperandrogeny may play a role in aggressive behaviors. Also, there appear to be distinct clinical forms of autism that are based on aggressive behaviors and are not classified in DSM-IV. Our preliminary findings suggest that abnormally high plasma testosterone concentration is associated with aggression against others and having fewer of the main autistic symptoms. Obviously, further studies are required with larger samples and longitudinal assessments.

REFERENCES

1. McBride PA, Anderson GM, Shapiro T: Autism research: bringing together approaches to pull apart the disorder. Arch Gen Psychiatry 1996; 53:980–983
2. Geschwind N, Galaburda AM: Cerebral lateralization: biological mechanisms, associations and pathology, II: a hypothesis. Arch Neurol 1985; 42:521–552, 634–654
3. Archer J: The influence of testosterone on human aggression. Br J Psychology 1991; 82:1–28
4. Tordjman S, Anderson GM, McBride PA, Hertzig ME, Snow ME, Hall LA, Ferrari P, Cohen DJ: Plasma androgens in autism. J Aut Dev Disord 1995; 25:295–304

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Compliance Complications in Cardiac Patients

TO THE EDITOR: The article by Ranjit C. Chacko, M.D., and colleagues (1) on psychiatric and psychometric predictors of heart transplant survival confirms and extends findings that we reported in 1995 (2). By using prospectively rated psychiatric, social, and demographic data obtained before the transplant, we evaluated the impact of a number of psychosocial variables on mortality, rejection episodes, and compliance in 75 heart transplant recipients followed for a mean of 13.9 months. We identified substance abuse history, personality disorder, and the psychiatrist's global rating of risk for posttransplant psychosocial problems that affect management (a measure that incorporated qualitative dimensional ratings of coping and social support along with a number of other items) as factors strongly associated with posttransplant noncompliance and the number of rejection episodes. However, mortality was not significantly associated with any of the variables we studied, which was not surprising given the limited cumulative mortality in a study with brief follow-up and limited group size.

Because of its design the study by Chacko et al. might be characterized as hypothesis generating rather than hypothesis testing. A prospective study of the identified coping, support, and psychiatric diagnostic factors in a new cohort would be invaluable. Nevertheless, as the authors note, the results to date provide important confirmation for the "medical" value of psychiatric and psychosocial assessment of heart transplant candidates. In doing so, they also heighten the need to establish that targeted psychosocial interventions with high-risk patients can have a beneficial effect on outcome. Otherwise, because of the societal pressures on transplant programs to maximize outcomes, these findings may be used punitively to exclude some dying patients who might benefit from life-saving treatment. Many patients already view the psychiatric evaluation before the transplant as a test that must be passed to gain access to the procedure. We can hardly expect candor from our patients if we are not in the position of trying to help them in their struggle to survive.

REFERENCES

1. Chacko RC, Harper RG, Gotto J, Young J: Psychiatric interview and psychometric predictors of cardiac transplant survival. Am J Psychiatry 1996; 153:1607–1612
2. Shapiro PA, Williams DL, Foray AT, Gelman IS, Wukich N, Sciacca R: Psychosocial evaluation and prediction of compliance problems and morbidity after heart transplantation. Transplantation 1995; 60:1462–1466

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Dr. Chacko and Colleagues Reply

TO THE EDITOR: We welcome the opportunity to discuss Shapiro et al.'s findings in relation to ours. Both investigations linked psychiatric morbidity (e.g., personality disorder and substance abuse) with compliance behavior and demonstrated that clinician judgment of compliance behavior and psychosocial risk and support can be related to different aspects of health behavior. We share a common view that identification of risk should lead to interventions to enhance outcome and prolong life. Our philosophy is that certain candidate behaviors (e.g., smoking or drinking during end-stage lung and liver disease) may preclude transplant but only until behavior change can be demonstrated. Patients remain in control of their own destiny provided that they demonstrate responsibility and seek help in overcoming serious personal obstacles destructive of the organs they seek to replace.

We are puzzled by Shapiro et al.'s characterization of our study as "hypothesis generating." We would reserve hypotheses for experimental manipulations or groundbreaking theoretical formulations, not verification of relationships properly credited to the earlier research that we cited in our article. Shapiro et al. also cite a briefer follow-up interval as a possible reason for the failure to relate their variables to mortality. However, they appear to have inappropriately used correlation analysis rather than proportional hazard model statistics to evaluate this relationship. They actually identified a larger group (125 cases) than we studied but obtained ratings on only 75. They did not report what portion of the 33 survivors were included in their analysis, nor did they describe needed comparisons between survivors and nonsurvivors for illness severity before transplant. It is also notable that axis I conditions were not considered or mentioned. We would encourage the authors to consider this variable and reanalyze their data by using survival analysis.

In addition to identifying treatable psychiatric conditions, our findings suggest that evaluation needs to go beyond psy-

chiatric diagnosis and consider other psychological behavioral risk factors in order to properly forecast treatment outcome, including health care utilization and survival time. Beyond encouraging replication investigations, we hoped that our study demonstrated the feasibility of *routine* evaluation of all relevant (medical and behavioral health) factors with this population as part of a continuous outcomes management process integral to the clinical service. Our findings and those of our predecessors should make a persuasive case for continued coverage of consultation evaluation and treatment efforts, since psychiatric and behavioral health factors are clearly important components of the "major medical" condition (organ failure) and treatment intervention (transplant). While mental health services may have only recently gained parity with other medical conditions, legislators, health benefit administrators, and our own medical brethren still need to recognize that there is a continuum of behavioral and psychosocial aspects of health and illness that are, in fact, inseparable from physical illness.

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Consistency of Traumatic Memories

TO THE EDITOR: We commend Steven M. Southwick, M.D., and colleagues on their empirical investigation of the consistency of retrospective reports of combat-related traumatic events (1). We concur that the field of traumatic stress all too often treats retrospective reports as objective measures of exposure, despite a lack of evidence to support their accuracy. We have conducted similar research and have also found systematic inconsistencies over time in the retrospective accounts of war-zone events among Somalia veterans (2). These findings challenge the validity of the assumption that a significant association between degree of combat exposure and posttraumatic symptoms represents a unidirectional relationship in which exposure predicts posttraumatic stress disorder (PTSD). In addition to emphasizing the need to show caution when using retrospective reports of events as necessarily objective measures of exposure, these studies point to the importance of expanding the simple dose-response theory that depicts the relationship between exposure and symptoms in causal models of PTSD (3).

Dr. Southwick and colleagues appropriately frame their study within the larger context of current debate in the field regarding the accuracy or inaccuracy of traumatic memory. They conclude accurately that their findings "do not support the notion that memory for traumatic events is fixed, indelible, or stable over time." However, given the enormous attention and furor surrounding issues of recovered memories and false memories, we feel it is important to clarify that neither these findings, nor the findings from our research, speak directly to the issue of recovered memories. As Southwick et al. note, memories of events may have been "repressed" during the first assessment and then recalled for the second one. Conversely, reports of occurrence might have been inflated by symptomatic individuals. Also, Southwick et al. assessed reports of exposure among individuals who were verifiably exposed to a stressful situation. Thus, although these data confirm that the reported frequency and intensity of known exposure may change over time, they do not at all address the issue of whether the occurrence of a potentially traumatic event might be falsely reported by an individual. Finally, a statistical difference in reports of the frequency of events at different time periods does not necessarily correspond to a clinically significant difference in the total impact of exposure to potentially traumatizing events. We hope that researchers will continue careful investigation of the nature of inconsistencies in memory for potentially traumatizing events and the functional impact of these inconsistencies.

REFERENCES

1. Southwick SM, Morgan CA III, Nicolaou AL, Charney DS: Consistency of memory for combat-related traumatic events in veterans of Operation Desert Storm. Am J Psychiatry 1997; 154:173–177
2. Roemer L, Litz BT, Orsillo SM, Ehlich PJ, Friedman MJ: Increases in retrospective accounts of war-zone exposure over time: the role of PTSD symptom severity. J Trauma Stress (in press)
3. Hales RE, Zatzick DF: What is PTSD? Am J Psychiatry 1997; 154:143–145

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TO THE EDITOR: The finding by Dr. Southwick and colleagues that PTSD is not directly related to the memory of traumatic events does not surprise me. After working with Vietnam veterans and adult survivors of child abuse for more than 20 years, I have become convinced that PTSD and dissociative disorders arise in response to confusion, shame, isolation, and possibly survivor guilt rather than trauma, pain, and abuse.

In one case a Vietnam veteran related his PTSD to an incident in which he thought he would be point man on a patrol. However, because of events outside his control this assignment was given to another member of his unit, who was then killed during the patrol. The subject felt that he was a coward because he had not insisted on retaining the point assignment, but he was too ashamed to share his feelings with other members of his unit. He felt isolated through the rest of his service time; PTSD subsequently developed. When the sequence of events and their connection to his PTSD became clear to him, he was able to share his memories with members of a PTSD group that he was attending. His level of functioning subsequently returned to normal.

Similarly, survivors of child abuse have told me again and again that they could have dealt with the actual abuse but could not deal with the sense of shame and guilt, the confusing physical sensations that result from sexual abuse, and the confusing multiple binds in which they were placed.

I postulate that there is a strong sense of shame associated with events during which PTSD symptoms arise. As a consequence, persons exposed to such events will not or cannot share these experiences with anyone, especially those closest to them (e.g., parents, other members of a combat unit). The fear of exposing a shameful secret makes these people feel that social situations are dangerous to them. They become hyperalert for fear that another might discover their secret.

The findings of Litz and colleagues (1) that generic rewards of military service negatively predict PTSD could be reinterpreted by using the aforementioned hypothesis. I suspect that the positive feeling is not protective but, rather, can be developed only because the soldier was not exposed to any incidents

that made him or her feel ashamed and consequently isolated. Of course, people who can deal with their shame and guilt by disclosing actions of which they are ashamed—as occurs in psychotherapy, especially in groups—would not develop PTSD or would be able to emerge from it. This may explain Litz and colleagues' finding that adult women, who are more prone to discuss their feelings with other women, do not suffer from PTSD as often as men (1).

Finally, if my views are correct, memory of a traumatic incident is immaterial to the development of PTSD, dissociative identity disorder, or dissociative identity disorder not otherwise specified, since these conditions are the result of feelings, not memories. Indeed, since confusion plays a major role in the genesis of these conditions, memories may well be confused, distorted, or absent at some time and present at others.

REFERENCE

1. Litz BT, Orsillo SM, Friedman M, Ehrlich P, Bartres A: Posttraumatic stress disorder associated with peacekeeping duty in Somalia for US military personnel. Am J Psychiatry 1997; 154:178–184

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Dr. Southwick and Colleagues Reply

TO THE EDITOR: We read with interest the letter by Dr. Roemer and colleagues that described systematic inconsistency in retrospective accounts of combat-related events among Somalia veterans that resembled our findings among Desert Storm veterans. We agree with Roemer et al.'s observation that the findings in both studies do not directly address the issue of recovered memories. As we noted in our article, there were multiple possible explanations for the observed inconsistencies in memory. Further, because the study involved retrospective accounts, it was not possible to know whether events recalled at 1 month were more or less accurate than events recalled at 2 years. However, in both our study and in the study by Roemer et al., it is clear that memory changed or was inconsistent over time. Such inconsistencies suggest that recall at any one time point may be inaccurate.

Dr. French's comments also were of interest. In several different veteran populations we too have found confusion, shame, isolation, and guilt to be major concerns for combat veterans with PTSD. However, we do not agree that PTSD arises in response to these concerns "rather than" in response to trauma, pain, and abuse. Further, we do not believe that memories of a traumatic incident are immaterial to the development of PTSD. Instead, we agree with Roemer et al. who note that a simple dose-response model of the relationship between trauma experiences and symptoms is insufficient. It appears that multiple factors, including degree of traumatic exposure, are related to level of PTSD symptoms.

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Borderline-Dissociation Comorbidity

TO THE EDITOR: The recent article by James J. Hudziak, M.D., and colleagues (1) reported that female patients with borderline personality disorder had high rates of comorbid Briquet's syndrome (hysteria), somatization disorder, antisocial personality disorder, and substance abuse disorders. Somatization disorder was emphasized, since patients were evaluated by using both the DSM-III-R criteria for somatization disorder and the criteria for Briquet's syndrome (2), which formed the basis for the DSM-III-R definition of somatization disorder. Not surprisingly, all patients with somatization disorder also met the criteria for Briquet's syndrome.

However, the authors failed to mention possible comorbidity with dissociative disorders. Both borderline personality disorder and dissociative disorders tend to occur in female patients who have experienced severe physical, emotional, or sexual abuse during childhood. Saxe et al. (3) described a high rate of comorbidity with borderline personality disorder in psychiatric inpatients with dissociative disorders. It would therefore have been of considerable interest if Hudziak et al. had evaluated their patients with borderline personality disorder for the presence of dissociative symptoms.

Dissociative disorders are often underrecognized (3), and studies such as that by Hudziak et al. that ignore the range of psychopathology present in patients with borderline personality disorder further contribute to this lack of recognition. Correct diagnosis and appropriate treatment of dissociative disorders have been shown to be effective, both in reducing the high levels of distress experienced by these patients and in achieving substantial savings in social welfare and mental health service expenditure (4).

REFERENCES

1. Hudziak JJ, Boffeli TJ, Kriesman JJ, Battaglia MM, Stanger C, Guze SB: Clinical study of the relation of borderline personality disorder to Briquet's syndrome (hysteria), somatization disorder, antisocial personality disorder, and substance abuse disorders. Am J Psychiatry 1996; 153:1598–1606
2. Perley MJ, Guze SB: Hysteria: the stability and usefulness of clinical criteria. N Engl J Med 1962; 266:421–426
3. Saxe GN, van der Kolk BA, Berkowitz R, Chinman G, Hall K, Lieberg G, Schwartz J: Dissociative disorders in psychiatric inpatients. Am J Psychiatry 1993; 150:1037–1042
4. Loewenstein RJ: Diagnosis, epidemiology, clinical course, treatment, and cost effectiveness of treatment for dissociative disorders and MPD: report submitted to the Clinton Administration Task Force on Health Care Financing Reform. Dissociation: Progress in the Dissociative Disorders 1994; 8:3–11

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Urge to Splurge

TO THE EDITOR: We read with interest the article on uncontrolled buying by Michel Lejoyeux, M.D., Ph.D., and colleagues (1). They review a topic of growing importance but failed to note our previous work in the area (2). In a group of 46 persons with compulsive buying, we found substantial comorbidity (mood, anxiety, substance use, and impulse control disorders). Over one-half met criteria for a personality disorder, but a special personality profile as suggested by Lejoyeux et al. was not found. We believe that "primary" uncontrolled buyers (i.e., without comorbidity) must be rare.

Lejoyeux and colleagues will be interested to learn that we have recently presented preliminary data from an uncontrolled trial in which 10 nondepressed subjects were treated with fluvoxamine (mean dose=205 mg/day) (3). Nine subjects had greater than 50% improvement in scores on the Yale-Brown

Obsessive Compulsive Scale—Shopping Version, a modification of the original instrument that we have shown to have excellent reliability and validity (3, 4). A randomized, double-blind trial of fluvoxamine versus placebo is now underway. On the basis of prior case reports, Lejoyeux et al. suggest that the effect of antidepressant treatment is "especially marked when uncontrolled buying was associated with a depression." In fact, these medications have a variety of effects that have little to do with mood, and, as we have shown, improvement with fluvoxamine was unrelated to mood state.

REFERENCES

1. Lejoyeux M, Adès J, Tassain V, Solomon J: Phenomenology and psychopathology of uncontrolled buying. Am J Psychiatry 1996; 153:1524–1529
2. Schlosser S, Black DW, Repertinger S, Freet D: Compulsive buying: demography, phenomenology, and comorbidity in 46 subjects. Gen Hosp Psychiatry 1994; 16:205–212
3. Black DW, Gabel JM: Treatment of compulsive buying with fluvoxamine, in New Research Program and Abstracts, 148th Annual Meeting of the American Psychiatric Association. Washington, DC, APA, 1995, p 189
4. Monahan P, Black DW, Gabel J: Reliability and validity of a scale to measure change in persons with compulsive buying. Psychiatry Res 1996; 64:59–67

DONALD W. BLACK, M.D.
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Dr. Lejoyeux and Colleagues Reply

TO THE EDITOR: We completely agree with Dr. Black and colleagues' suggestion that "primary" uncontrolled buyers are rare. The effect of serotonergic agents on compulsive buying in nondepressed subjects raises very interesting clinical and phenomenological questions. This effect can be understood as an "anti-obsessive-compulsive action." It corroborates a previously expressed hypothesis that suggests compulsive buying is related to the obsessive-compulsive disorder spectrum (1).

REFERENCE

1. Lejoyeux M, Hourtané M, Adès J: Compulsive buying and antidepressants (letter). J Clin Psychiatry 1995; 56:38

MICHEL LEJOYEUX, M.D., PH.D.
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Repetitive Behaviors and D8/17 Positivity

TO THE EDITOR: Susan E. Swedo, M.D., and colleagues recently reported that a trait marker for rheumatic fever (D8/17) could identify children with pediatric autoimmune neuropsychiatric disorders (obsessive-compulsive disorder [OCD] and tic disorders) associated with streptococcal infections (PANDAS) and Sydenham's chorea (1). Eighty-five percent of children with PANDAS, 89% of children with Sydenham's chorea, and only 17% of healthy comparison subjects were D8/17 positive (≥12% D8/17+ cells).

This is an important finding that has potentially far-reaching consequences with respect to identification of subtypes of OCD (2), understanding the relationship between Sydenham's chorea and OCD (3), defining the role of poststreptococcal autoimmune factors in OCD (4), and developing new therapeutic strategies for these disorders (2). However, alternative theoretical perspectives and additional methodological descriptions may be helpful in gauging the full impact of these important findings.

The childhood-onset variants of obsessive-compulsive and tic disorders are known to have a marked male predominance (5). Thus, gender may conceivably be a factor in the expression of D8/17 positivity. Since the groups appear to differ by sex, with more male subjects in the PANDAS group (70.4%, N=19 of 27) than in the healthy comparison group (29.2%, N=7 of 24) (χ^2=8.63, df=1, p=0.003), exploration of an overall sex effect on D8/17 positivity would be of interest. Likewise, mention of the prevalence of antistreptococcal antibodies (i.e., anti-streptolysin O and anti-DNase B) in the groups would be helpful in determining the relative rates of recent streptococcal infection in each of the groups.

It is unknown whether D8/17 positivity is specific for PANDAS, Sydenham's chorea, and rheumatic fever or if it also occurs in other neuropsychiatric disorders. Studies of psychiatric disorders without poststreptococcal symptom exacerbation would help clarify whether D8/17 either is involved in an autoimmune response or serves as a genetic marker for select neuropsychiatric disorders. If D8/17 level were found to be correlated with repetitive behaviors as measured by Yale-Brown Obsessive Compulsive Scale severity, this might support a dimensional approach to D8/17 mediation of compulsive symptoms across traditional diagnostic boundaries.

REFERENCES

1. Swedo SE, Leonard HL, Mittleman BB, Allen AJ, Rapoport JL, Dow SP, Kanter ME, Chapman F, Zabriskie J: Identification of children with pediatric autoimmune neuropsychiatric disorders associated with streptococcal infections by a marker associated with rheumatic fever. Am J Psychiatry 1997; 154:110–112
2. Allen AJ, Leonard HL, Swedo SE: Case study: a new infection-triggered, autoimmune subtype of pediatric OCD and Tourette's syndrome. J Am Acad Child Adolesc Psychiatry 1995; 34:307–311
3. Swedo SE: Sydenham's chorea: a model for childhood autoimmune neuropsychiatric disorders (clinical conference). JAMA 1994; 272:1788–1791
4. Swedo SE, Leonard HL, Kiessling LS: Speculations on antineuronal antibody-mediated neuropsychiatric disorders of childhood. Pediatrics 1994; 93:323–326
5. Swedo SE, Rapoport JL, Leonard HL, Lenane M, Cheslow D: Obsessive compulsive disorder in children and adolescents: clinical phenomenology of 70 consecutive cases. Arch Gen Psychiatry 1989; 46:335–341

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Dr. Swedo Replies

TO THE EDITOR: My colleagues and I appreciate the comments of Dr. Hollander and his colleagues and welcome the opportunity to provide clarification of the results described in our recent article.

As we reported, D8/17 was first identified as a trait marker of rheumatic fever susceptibility and has been widely tested in a variety of patient groups and in various epidemiologic samples throughout the world. There is no evidence to suggest that attack rates of acute rheumatic fever differ between genders (although Sydenham's chorea is slightly more common among female adolescents with rheumatic fever than among male adolescents). Similarly, there has been *no* evidence for male-female differences in the rates of D8/17 positivity in previous investigations (1–3), nor did we find differences in relative rates of D8/17 positivity among male and female patients in our study. Thus, at present, there is no evidence to suggest that gender is related to D8/17 status.

In interpreting the results of recent neuropsychiatric investigations (4), it is important to remember that D8/17 was developed as a *trait* marker of rheumatic fever susceptibility. Numerous rheumatic fever investigations and our increasing experience with longitudinal D8/17 assessments in patients with OCD and tic disorders (including Tourette's disorder) clearly demonstrate that D8/17 is not a state marker of streptococcal reactivity. Subjects who are initially identified as being D8/17 positive remain in that category even when their antistreptococcal titers fall to normal levels; conversely, numerous subjects have been found to be D8/17 negative despite markedly elevated antistreptococcal antibody titers, as seen in the patients with well-documented acute poststreptococcal glomerulonephritis, in which all patients had decreased complement, high anti-streptolysin O or anti-DNase B titers, and urinary signs of disease, yet had low D8/17 values (1). Because the relative percentage of D8/17+ cells remains constant among individuals across time, it is highly unlikely that the percentage of D8/17+ cells will be found to correlate with symptom severity. In fact, since D8/17 status is reported as a dichotomous variable (positive or negative), it is difficult to envision how it might be used as a "dimensional" variable.

We agree with Dr. Hollander and colleagues that D8/17 is an interesting biologic marker worthy of further investigation. Studies that examine rates of D8/17 positivity in various neuropsychiatric disorders will help determine whether the marker is related only to poststreptococcal immune dysfunction (as postulated) or if it may also serve as a marker of neuropsychiatric vulnerability. The recent report by Dr. Murphy and colleagues suggests that at the least, D8/17 is able to identify an unselected group of patients with childhood-onset obsessive-compulsive disorder (4).

REFERENCES

1. Khanna AK, Buskirk DR, Williams RC Jr, Gibofsky A, Crow MK, Menon A, Fotino M, Reid HM, Poon-King T, Rubinstein P: Presence of a non-HLA B cell antigen in rheumatic fever patients and their families as defined by a monoclonal antibody. J Clin Invest 1989; 83:1710–1716
2. Rodriguez RS, Ontiveros P, Torres S, Zabriskie JB: Rheumatic fever associated B-cell alloantigen in 1854 school-aged healthy children. Pediatric Rev Communication 1994; 7:259–263
3. Gibofsky A, Wetzler LM, Zabriskie JB: Rheumatic fever, in Rheumatology Textbook. New York, Oxford University Press, 1994, pp 613–620
4. Murphy TK, Goodman WK, Fudge MW, Williams RC Jr, Ayoub EM, Dalal M, Lewis MH, Zabriskie JB: B Lymphocyte antigen D8/17: a peripheral marker for childhood-onset obsessive-compulsive disorder and Tourette's syndrome? Am J Psychiatry 1997; 154:402–407

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High Sucrose Preference in Alcoholic Men

TO THE EDITOR: Alexey Kampov-Polevoy, M.D., Ph.D., and colleagues (1) demonstrated an enhanced preference for sweet solutions in alcoholic men and suggested that this may indicate a generalized alteration in rewarding response to hedonic stimuli in those with alcohol dependence. Instead, their findings may reflect a chemosensory adjustment to the effect of alcohol on the olfactory system. Both acute alcohol intoxication (2) and chronic alcoholism (3) are associated with an impaired olfactory ability. Smell is approximately 90% of what is described as "taste" or flavor; hyposmic individuals perceive food as bland or tasteless (4). In order to compensate, spices and enhanced true taste (e.g., sugar) are added to food (5). Therefore, through a learned response paradigm, these alcoholic men may have developed preference for a higher concentration of sugars, even in the absence of other foods. Alternatively, because of chronic excess daily use of sugars, they may have induced an up-regulation of their sweet taste receptors, raising their sucrose threshold, and the associated sucrose hedonic curve (6).

Thus, preference for higher sucrose concentration in alcoholic men may just represent a behavioral compensatory response for those with alcohol-induced olfactory loss.

REFERENCES

1. Kampov-Polevoy A, Garbutt JC, Janowsky D: Evidence of preference for a high-concentration sucrose solution in alcoholic men. Am J Psychiatry 1997; 154:269–270
2. Hirsch AR, Russell GS: Effects of inebriation on olfaction. Investigative Med 1995; 43:422A
3. Mair RG, Doty RL, Kelly KM, Wilson CS, Langlias PS, McEntree WJ, Vollmecke TA: Multimodal sensory discrimination deficits in Korsakoff's psychosis. Neuropsychologia 1986; 24:831–839
4. Hirsch AR: Olfaction in migraineurs. Headache 1992; 32:233–236
5. Ferris AM, Schlitzer JL, Schierberl MJ, Catalanotto FA, Gent J, Peterson MG, Bartoshuk LM, Cain WS, Goodspeed RB, Leonard G, Donaldson JO: Anosmia and nutritional status. Nutr Res 1985; 5:149–156
6. Contreras RJ: Gustatory mechanisms of a specific appetite, in Neural Mechanisms in Taste. Edited by Cagan RH. Boca Raton, Fla, CRC Press, 1989, pp 119–145

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Dr. Kampov-Polevoy and Colleagues Reply

TO THE EDITOR: We would like to thank Dr. Hirsch for the interesting suggestion that alcoholics' preference for stronger sucrose solutions may result from chemosensory adjustment of the olfactory system to excessive alcohol intake. Although we did not have the opportunity to review this issue in our article, we agree that it is an important consideration.

The main finding of our pilot study was that a majority of alcoholic subjects have a preference for stronger sucrose concentrations, significantly more than what we found in nonalcoholic comparison subjects. We analyzed several mechanisms that may underlie this phenomenon, including the possibility that it may be a "consequence of heavy drinking that has altered taste sensitivity." Although this factor, including possible alterations in smell and taste, needs to be further evaluated, we cited evidence in support of the hypothesis that sweet liking in alcoholism may represent an

underlying neurobiological trait. First, animal experiments have indicated an association between preference for concentrated sweets and alcohol use in rats known to have a genetically determined propensity to prefer or reject alcohol before exposure to it (1). Second, 35% of our alcoholic subjects showed a preference for lower sucrose concentration, but there was no difference in the pattern or duration of alcohol intake between "sweet-liking" and "sweet-disliking" alcoholics. Finally, 16% of our nonalcoholic subjects had a preference for the most concentrated sucrose solution despite their minimal alcohol consumption. This last observation is in agreement with the results from work in healthy subjects, which indicated that sweet liking or sweet disliking are psychophysical traits that are relatively stable over time (2–4).

Nevertheless, we agree with Dr. Hirsch that further studies are needed to fully explain the mechanism of our observed association between alcoholism and a preference for stronger sweet solutions and that investigations of olfactory and taste sensitivity are important. One strategy that we are currently using to address the potentially confounding effects of heavy drinking is to study subjects at high risk or low risk for alcoholism.

REFERENCES

1. Sinclair JD, Kampov-Polevoy AB, Stewart R, Li T-K: Taste preferences in rat lines selected for low and high alcohol consumption. Alcohol 1992; 9:155–160
2. Looy H, Weingarten HP: Effects of metabolic state on sweet taste reactivity in humans depend on underlying hedonic response. Chemical Senses 1991; 16:123–130
3. Looy H, Callaghan S, Weingarten HP: Hedonic response of sucrose likers and dislikers to other gustatory stimuli. Physiology and Behavior 1992; 52:219–225
4. Thompson DA, Moskowitz HR, Campbell RG: Effects of body weight and food intake on pleasantness ratings for a sweet stimulus. J Applied Physiology 1976; 41:77–83

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Compulsive Sexual Behavior Characteristics

TO THE EDITOR: We commend Donald W. Black, M.D., and colleagues (1) for their publication of empirical data that describe the phenomenology and axis I and II comorbidity associated with "compulsive sexual behavior." The authors overlooked, however, our prior report that described DSM-III-R axis I comorbidity in 60 male subjects with paraphilias (N=34) and paraphilia-related disorders (N=26) who were seeking psychiatric treatment (2). Our study both complements and contrasts with the Black et al. findings.

As Black et al. noted, nonparaphilic sexual impulsivity disorders are understudied, and uniform operational criteria to characterize them are lacking. Their study used Coleman's compulsive sexual behaviors typology (3), which includes some behaviors that are not clearly genital or sexual but are more relational and romantic (e.g., compulsive fixation or erotomania, multiple love relationships). We propose that the term "paraphilia-related disorders" be used to describe nonparaphilic sexual disorders, since this term does not implicitly characterize the form of these disorders (e.g., as impulsive, compulsive, or addictive). Paraphilia-related disorders are sexual disorders that, like paraphilias, are repetitive, intrusive, persist at least 6 months, and are accompanied by psychosocial distress and impairment, but the specific behaviors are not currently considered as socially anomalous or deviant. In our research, the predominant paraphilia-related disorders are ego-dystonic compulsive masturbation, protracted promiscuity, and dependence on pornography. Less prevalent forms include phone sex dependence, severe sexual desire incompatibility, and dependence on sexual accessories such as objects (e.g., dildos) or drugs (e.g., amyl nitrate, cocaine) (2, 4). Although it is not clearly noted in Black et al.'s report, our studies have consistently found that male subjects with paraphilias, including sex offenders, commonly have paraphilia-related disorders as well.

In our comorbidity study, 28% of male subjects with paraphilias and paraphilia-related disorders described a history of physical or sexual abuse, and the lifetime prevalence of mood disorders (76.7%) (especially early-onset dysthymic disorder [53.3%]), psychoactive substance abuse (46.7%) (especially alcohol abuse [40%]), and anxiety disorders (46.7%) (especially social phobia [31.6%]) did not readily distinguish the paraphilic from the nonparaphilic group. The different prevalence rates of specific axis I disorders in our report, in contrast with Black et. al., could be related to differing methodologies used to establish axis I diagnoses and differing methods of subject ascertainment.

REFERENCES

1. Black DW, Kehrberg LLD, Flumerfelt DL, Schlosser SS: Characteristics of 36 subjects reporting compulsive sexual behavior. Am J Psychiatry 1997; 154:243–249
2. Kafka MP, Prentky RA: Preliminary observations of DSM-III-R axis I comorbidity in men with paraphilias and paraphilia-related disorders. J Clin Psychiatry 1994; 55:481–487
3. Coleman E: Is your patient suffering from compulsive sexual behavior? Psychiatr Annals 1992; 22:320–325
4. Kafka MP, Prentky R: A comparative study of nonparaphilic sexual addictions and paraphilias in men. J Clin Psychiatry 1992; 53:345–350

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HIV and Depression

TO THE EDITOR: I appreciated reading the article by Judith G. Rabkin, Ph.D., M.P.H., and colleagues (1). Their finding, that HIV-infected men were not more depressed as their illness progressed, is consistent with research on the relationship between negative life events and depression. As Kendler and colleagues have shown (2), the relationship is complex and modulated by genetic predisposition. To that equation, Klerman would add such variables as early life experiences, personality traits, coping styles, and social supports (3). Rabkin et al.'s cohort was predominantly white, well educated, generally middle class, and part of a supportive community. Thus, these subjects were probably less likely to be predisposed to depression.

As Rabkin and Struening have previously shown (4), the association between negative life events and various illnesses is generally weak. Perhaps we are getting beyond the stage of looking for simple cause and effect relationships between life stress and mood. We might instead look at HIV cohorts at

greatest risk for depression: the poor, substance abusers, and women. In our experience, baseline rates of depression in these groups are quite high, and we are investigating how HIV progression affects these premorbid factors.

Treating depression in HIV groups at risk is important. Depression is a critical variable when one considers compliance with treatment and immune system functioning (5). These issues are particularly important given recent data on the need for strict compliance when using protease inhibitors (in which viral resistance can result from intermittent dosing). Thus, the importance of treating depression in this population may go beyond issues of quality of life. It may be critical to the prevention and management of HIV progression.

REFERENCES

1. Rabkin JG, Goetz RR, Remien RH, Williams JBW, Todak G, Gorman JM: Stability of mood despite HIV illness progression in a group of homosexual men. Am J Psychiatry 1997; 154:231–238
2. Kendler KS, Kessler RC, Walters EE, MacLean C, Meale MC, Heath AC, Eaves LJ: Stressful life events, genetic liability, and onset of an episode of major depression in women. Am J Psychiatry 1995; 152:833–842
3. Klerman GL: Discussion, part II, in Stress and Mental Disorders. Edited by Barrett JE, Rose RM, Klerman GL. New York, Raven Press, 1979, pp 111–120
4. Rabkin JG, Struening EL: Life events, stress, and illness. Science 1976; 194:1013–1020
5. Wolpe PR, Gorton G, Serota R, Sanford B: Predicting compliance of dual diagnosis inpatients with aftercare treatment. Hosp Community Psychiatry 1993; 44:45–49

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Dr. Rabkin and Colleagues Reply

TO THE EDITOR: Dr. Boland suggests that our finding that HIV-positive men were not more depressed as their illness progressed was related to their being predominantly white, well educated, and in relatively secure economic and social circumstances. He proposes that study of groups at greater risk for depression among HIV-positive patients, such as the poor, substance abusers, and women, might produce different results.

During the years that we conducted our longitudinal study of HIV seropositive and seronegative gay white men, we conducted a parallel longitudinal study of HIV seropositive and seronegative minority substance-abusing men and women (1). We modified measures to accommodate the possibility of shorter attention spans and lower literacy levels but otherwise used parallel assessments and design.

We followed 121 men (69 HIV-positive, 52 HIV-negative) and 66 women (36 HIV-positive, 30 HIV-negative) semiannually for seven visits. Attrition, unrelated to sex or serostatus, was 33%. At study baseline, rates of major depression and dysthymia ranged from 15% (HIV-negative men) to 33% (HIV-negative women). Global impairment at baseline was in the range found among psychiatric patients. Rates of syndromal disorders did not show any consistent variation over time by HIV status, although HIV-positive men had higher levels of distress and greater impairment than HIV-negative men. Serostatus did not predict distress levels among women.

These rates of psychopathology and impairment are much higher than we found among the gay cohort but did not increase over time. In fact, all groups showed statistically significant but clinically modest reductions in measures of distress, rates of psychopathology, and drug use over time. Although all indices remained elevated, this is true for both HIV-negative and HIV-positive men and women; HIV status is therefore not the primary determinant.

In studying the roles of social support and stress in this cohort, we found that higher levels of social support and lower levels of social conflict were independently associated with less distress and improved global functioning among both men and women. For men, more social support and less social conflict was associated with less stress, independent of time and HIV status or their interaction. For women, stress declined over time and was exacerbated by social conflict, independent of HIV status.

As in the gay cohort, no correspondence was observed between laboratory markers of HIV progression and distress severity. However, we did observe an interaction between HIV symptoms and distress level: with the passage of time, those with fewer HIV symptoms showed more psychological improvement than those with more HIV symptoms.

Although both of our cohort studies had limitations in terms of relatively small group sizes, nontrivial attrition, and relatively modest HIV disease progression, their results are similar and support our fundamental point that depression is not an inevitable or even probable consequence of living with a progressive, incurable, and possibly fatal disease.

Additional studies of such questions and further work with HIV-positive minority populations are certainly indicated, and we are pleased to hear that Dr. Boland's group is pursuing such research.

REFERENCE

1. Rabkin JG, Johnson J, Lin SH, Lipsitz JD, Remien RH, Williams JB, Gorman JM: Psychopathology in male and female HIV-positive and negative injecting drug users: longitudinal course over 3 years. AIDS 1997; 11:507–515

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Disputing Psychiatry's Redefinition

TO THE EDITOR: The APA Commission on Psychotherapy by Psychiatrists has major points of disagreement with the redefinition of psychiatry put forward by Jeffrey A. Lieberman, M.D., and A. John Rush, M.D. (1). Drs. Lieberman and Rush outline a residency that would eliminate substantive training in skills essential to the basic psychological understanding and management of patients, as well as the complexities of psychotherapy.

Drs. Lieberman and Rush have highlighted the economic forces driving many of today's medical decisions. We are all struggling to do more with less. The authors have seen in this struggle a need to redefine our specialty and our patient population by extending our specialty to include all patients, including those in neurology and internal medicine who have behavioral disturbances. Residents would spend 1 year in general medicine and a second year focused on neurological dis-

orders—and a third year might combine inpatient and outpatient experience in psychiatry. Starting a psychiatric residency with 2 full years in other medical specialties strikes us as an extremely poor way to facilitate identification with the distinctive role of the psychiatrist.

The strength of psychiatry lies largely in its expertise in the integration of the biological and the psychological in diagnosis and treatment. Drs. Lieberman and Rush lean heavily on the biological side of the balance, and their recommendations would almost certainly result in producing residents who were poorly prepared in such areas as the psychodynamics of prescribing, the art of forming a therapeutic alliance, and the necessary expertise to conduct competent psychotherapy.

A recent study (2) demonstrated that psychotherapy changes brain function. Psychotherapy is no less a "medical" treatment than pharmacotherapy. The great strength of psychiatry and its appeal to future trainees lie in the emerging possibilities for integrating psychological and biological knowledge in an exciting new science of mind/brain unity. If psychiatry abdicates psychotherapy to nonmedical professions, there will be no discipline that can carry out this integration.

We foresee that in the future psychiatrists will need to be increasingly knowledgeable about every aspect of their specialty. They will be dealing with the most difficult and challenging patients. They will be delegating, supervising, and organizing. One cannot supervise a treatment in which one has no experience or competence.

If psychiatrists are to remain leaders in the treatment of the mentally ill, psychiatric training and education must be comprehensive. The foundation must include a solid understanding of the most current information about "mind" (learning theory, psychoanalytic theory) and "brain" (molecular neurobiology, neurotransmitters). From this basis, the fundamentals of diagnosis, psychopharmacology, and psychotherapy must be taught and clinically applied.

In order to become a specialist, rather than a technician with only cookbook knowledge, residents must learn psychotherapy firsthand. Psychodynamic, cognitive/behavioral, family, and group theories and treatments provide the fundamental elements for the interpersonal and communication skills that psychiatrists use in every clinical situation. Extensive clinical experience over several years is necessary in order to incorporate this knowledge base fully into the matrix of psychiatrists' clinical armamentarium.

Residents choose the field of psychiatry for a mélange of humanitarian, intellectual, and philosophical reasons. Our tradition works uniquely at gaining understanding of the human condition by using all of our available perspectives in the service of healing. Our residents do not enter psychiatry to become partially trained internists or neurologists—thereby doing neither well and blurring their primary professional identities for themselves and for colleagues in the other medical specialties.

In Lieberman and Rush's model, psychiatry's wealth of well-founded knowledge of human psychology and psychotherapy would be lost to our future residents, and with it would be lost much that attracts physicians to our field.

REFERENCES

1. Lieberman JA, Rush AJ: Redefining the role of psychiatry in medicine. Am J Psychiatry 1996; 153:1388–1397
2. Schwartz JM, Stoessel PW, Baxter LR Jr, Martin KM, Phelps ME: Systematic changes in cerebral glucose metabolic rate after successful behavior modification treatment of obsessive-compulsive disorder. Arch Gen Psychiatry 1996; 53:109–113

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Drs. Lieberman and Rush Reply

TO THE EDITOR: We appreciate the comments by the APA Commission on Psychotherapy by Psychiatrists—especially in light of our desire to construct a dialogue around critical issues that need urgent attention by the field. It is particularly gratifying that this letter should come from distinguished colleagues who serve on a component of the APA and whose leadership is sorely needed. Although we respect their opinion of our article, we were disappointed that it focused solely on the proposed training model and did not address the main issue of the article (which was its major impetus): the current crisis in academic psychiatry. Moreover, their disputation of our proposed training program did not provide any alternative ideas (in terms of policy, training, or service) for how to deal with the challenges that confront our field as did a recently published article by distinguished colleagues (1), which proposed modifications in psychiatry's orientation that were similar to ours.

In terms of the proposed training model, we must admit that there is a disagreement between us and the Commission with regard to conceptualizing and specifying education that anticipates the role of psychiatric graduates in the next millennium. Our proposal calls for psychiatrists to continue to have sufficient training in the fundamentals of psychotherapy and human psychology to be able to conduct psychotherapeutic intervention should they so desire. It is our contention, however, that psychotherapy already is and certainly can be conducted by nonphysicians at a level of competence equal to that of psychiatrists for the so-called "worried well" patients and the general (but not severely ill or comorbidly complicated chronic) patients. In these circumstances, residents should learn how to do these therapies in a briefer and more efficient time interval than is currently provided. In fact, their identity as psychiatrists would be strengthened rather than weakened by our proposal, since they would avoid the common internal debate that typically occurs in the second year of residency: "Am I a social worker? Am I a psychologist? Am I a doctor? What is a psychiatrist?" We view psychiatry as a subspecialty of internal medicine and a specialty that deals with psychopathology and all forms of disturbed affect behavior, cognition, and motivation whether from genetic, environmental, general medical, or other etiologies. In this context we feel that the greater time that trainees would spend in neurology and general medicine recommended in our proposal would facilitate the identification of trainees as physicians, rather than impair their identification as psychiatrists. The art of forming the doctor-patient relationship, a therapeutic alliance, and the psychodynamics and psychology

of managing people with psychiatric symptoms or syndromes can and should be learned in general medical, neurological, and psychiatric patients.

We recommended that the first 2 years of residency should not be restricted to inpatients, since length of stay is remarkably short and not expected to increase, but should include outpatient experiences in general medical and neurological practices, with appropriate psychiatric supervision to provide learning of the key elements suggested by Dr. Clemens and colleagues.

Clemens et al. also state that psychotherapy is no less a medical treatment than pharmacotherapy. If that were true, that would mean that social workers, psychologists, and others are performing medical treatments. We respectfully disagree with this point. Psychotherapy is a treatment that can be provided by physicians and nonphysicians alike. It is medical in that psychotherapeutic methods have been used and should be used to treat various general medical as well as psychiatric conditions. However, there is nothing inherently medical in its theory or methods.

Our recommendations certainly do not preclude a continuing emphasis and experience in the psychological aspects of psychopathology—including learning psychotherapy firsthand by doing it. It is our contention, however, that trainees can still learn "the fundamental elements" of psychodynamic, cognitive, behavioral family, and group theories and treatments while increasing emphasis is placed on neurobiological, neuroanatomic, neuroscientific, and general biological systems as they affect learning and psychopathology. We also recommended a specialty track option for residents who wish to concentrate specifically and become in-depth experts on the conduct of psychotherapy and psychosocial rehabilitation programs for the severe, complex, or chronically mentally ill patients. Dr. Clemens and colleagues note that psychiatrists of the future will be dealing with the most difficult and challenging patients—a prediction with which we agree.

Finally, the Commission contends that our recommendations to increase general medical and neurological perspectives and to reduce (but *not*, it is important to add, eliminate) emphasis on psychological, psychotherapeutic, and psychodynamic factors in understanding psychopathology or procedures in dealing with those conditions would reduce the attraction of psychiatry to medical students. Clemens et al. state that psychiatric residents would be partially trained internists and neurologists, which would blur professional identity and potentially attract fewer physicians to our field. To this point we suggest that the degree to which applications to psychiatric training programs would be affected by the proposed changes is at best speculative. Moreover, the current situation is already worrisome with numbers of medical students who choose careers in psychiatry being at near historic lows (2). One could say that the level of current training is such that we already are turning out partially trained psychologists. Finally, we would foresee a time much as Dr. Shore has suggested (3) in which the general medical and neurological problems of psychiatric patients would be managed by psychiatrists as their principal caregivers.

To put this argument succinctly into context, we would view psychiatry as standing on three pillars of knowledge: general medicine, neurology, and psychology. We see the current emphasis in training to be largely on psychology with too little on general medicine and neurology. We are not proposing the removal of one of the three pillars on which the field rests but rather are recommending a modest shift in the emphasis in training and a subsequent shift in practice that psychiatrists would be undergoing, which is not to be dictated by economic forces but rather by optimal treatment for our patients.

REFERENCES

1. Detre T, McDonald MC: Managed care and the future of psychiatry. Arch Gen Psychiatry 1997; 54:201–204
2. Sierles FS, Taylor MA: Decline of US medical student career choice of psychiatry and what to do about it. Am J Psychiatry 1995; 152:1416–1426
3. Shore JH: Psychiatry at a crossroad: our role in primary care. Am J Psychiatry 1996; 153:1398–1403

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Obsessive-Compulsive Symptoms in Schizophrenia

TO THE EDITOR: When does the presence of symptoms warrant a diagnosis? This is an important question raised by Jane L. Eisen, M.D., and colleagues (1). Obsessive-compulsive symptoms occur in schizophrenia, as do depressive symptoms, manic symptoms, and phobic symptoms among others. The emergence of obsessive-compulsive symptoms after infections and other processes that may impinge on brain function makes it reasonable to hypothesize that the same could be the case with schizophrenia. Identification of obsessive-compulsive symptoms in schizophrenia allows treatment to be modified accordingly. However, we need to address the implications of making a second diagnosis, establishing obsessive-compulsive disorder (OCD) as a comorbid condition rather than considering the symptoms as associated with schizophrenia.

DSM-III-R addressed this issue through the concept of diagnostic hierarchies. Relevant to this article, it stated: "When a more pervasive disorder, such as Schizophrenia, commonly has associated symptoms that are defining symptoms of a less pervasive disorder . . . only the more pervasive disorder is diagnosed."

It would be conceptually more sound to have an obsessive-compulsive symptom modifier to add to the schizophrenia diagnosis than have a separate diagnosis of OCD. Obsessive-compulsive behavior may represent a final common pathway for disorders with multiple etiologies, and for that reason splitting off this symptom complex when it is associated with a pervasive disorder may be unwise. Parsimony in diagnosis has its logic.

REFERENCE

1. Eisen JL, Beer DA, Pato MT, Venditto TA, Rasmussen SA: Obsessive-compulsive disorder in patients with schizophrenia or schizoaffective disorder. Am J Psychiatry 1997; 154:271–273

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Dr. Eisen and Colleagues Reply

TO THE EDITOR: Dr. Werner's letter raises a question about the usefulness of assigning an additional diagnosis of OCD to schizophrenic patients who also have obsessions or compulsions. While a discussion of the relative benefits of a hierarchical nosological system is beyond the scope of this reply, we would like to comment on the difficulties that might arise from Dr. Werner's suggestion to add an obsessive-compulsive symptom modifier to the diagnosis of schizophrenia. There has been much interest recently in ascertaining the frequency with which obses-

sive-compulsive symptoms co-occur in psychotic disorders such as schizophrenia. It has been suggested that as many as 40% of patients with schizophrenia or schizoaffective disorder have obsessive-compulsive symptoms. Our concern is that using an obsessive-compulsive symptom modifier without clearly defined criteria may lead to nonhomogeneous populations for study (e.g., patients who are overly preoccupied or "obsess" about their delusions or patients who check repeatedly to make sure their phones are not being tapped).

We may have underestimated the frequency of obsessive-compulsive symptoms in the patients with schizophrenia whom we assessed because we required patients to meet the clearly defined DSM-IV criteria for OCD. However, ensuring a homogeneous patient population allows further investigation of important questions such as treatment response, course of illness, and prognosis. Pursuit of these questions may be muddied by the use of an obsessive-compulsive symptom modifier that does not have specific criteria.

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Religion and Psychopathology

TO THE EDITOR: The article by Kenneth S. Kendler, M.D., and colleagues (1) examines the relationship among dimensions of religiosity and current psychiatric symptoms, current substance abuse, lifetime psychiatric disorders, and lifetime substance dependence and explores the stress-buffering properties of religiosity.

Subjects studied varied in their religious preference, and affiliations were ranked into five groups of decreasing conservatism. The ordering of institutional affiliations was then cross-validated with self-report measures of personal devotion and personal conservatism. The authors found that "high levels of personal devotion were associated with less of a response to the depressogenic effects of stressful life events." However, they also reported that "neither personal conservatism . . . nor institutional conservatism . . . were associated with a significant change in sensitivity to the depressogenic effects of stressful life events." Finally, Kendler et al. state that they found "little overall evidence for a relationship between religiosity and current psychiatric symptoms or lifetime psychopathology."

We studied a homogeneous group of "returnees" to the Lubavitch sect of Orthodox Judaism (2). Members of this community adhere closely to the strict interpretation of Jewish law and enjoy an intensive supportive social network. Twenty-nine subjects who were referred by a community mental health clinic were interviewed. Fifteen were in active treatment for a major affective disorder or psychosis, while 14 were receiving "supportive counseling" for adjustment disorders. Additional data were obtained by interviewing the clients' therapists. Instruments used included the Self-Image Questionnaire (3), which explored a wide range of subjective feelings including items that pertain to adolescent turmoil and self-image, and the Personal Resources Inventory (unpublished questionnaire by P. Clayton and N.M.A. Hirschfeld), which rated behaviors reflective of social interaction patterns, job and role functioning, and use of community supports.

Information was gathered from subjects' therapists, who rated the subjects' use of defenses (flexible, neurotic, or primitive), use of ritual (flexible, somewhat rigid, or extremely rigid), adherence to religious rules (just adhering or ego-gratifying), and degree of internalization of religious values (rote, variable, or internalized).

Subjects with a DSM-III diagnosis of major mood disorder or psychosis used more primitive defenses, adhered to rules in a rote-like way, and used ritual more rigidly and with less ego gratification. Despite seemingly high levels of "personal devotion," religiosity in these subjects did not serve as a buffer against stressful life events. Members of this group were receiving one or more public benefits, were less involved in community efforts, and were less adaptive in their household responsibilities.

For subjects in whom a major DSM-III diagnosis was not present, household income was much higher, subjects were more likely to be happily married, community involvement was greater and less need serving, and religious ritual was more flexible and ego enhancing.

Our conclusion was that subjects' ability to internalize religion and make effective use of a close community support system to buffer them against stressful life events appeared to have been influenced by genetic or environmental factors before their religious conversion.

REFERENCES

1. Kendler KS, Gardner CO, Prescott CA: Religion, psychopathology, and substance use and abuse: a multimeasure, genetic-epidemiologic study. Am J Psychiatry 1997; 154:322–329
2. Trappler B, Endicott J, Friedman S: Psychosocial adjustment among returnees to Judaism. J Psychology 1995; 129:433–441
3. Offer D: The Psychological World of the Teenager: A Study of Normal Adolescent Boys. New York, Basic Books, 1969

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Dr. Kendler and Colleagues Reply

TO THE EDITOR: We appreciate the thoughtful observations and comments by Drs. Trappler and Endicott. It is difficult to directly compare the conclusions that they reached with their clinically ascertained cohort with those taken from our epidemiologic samples. We would, however, concur that it is highly likely that the degree of stress-buffering provided by personal religious devotion is dependent upon a range of personal and historical characteristics. The work of Trappler and Endicott further demonstrates how much more we in the field of mental health have to learn about the role of religion in dealing with adversities.

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