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.

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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.

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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).

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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