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Dr. Stein and Colleagues Reply

TO THE EDITOR: We thank Drs. Jainer and Chawla for their interest in our work. We agree with the gist of their letter emphasizing the importance of detailed reporting of methods for randomized controlled trials. The CONSORT guidelines provide an excellent framework for publication of the results of randomized controlled trials, and we agree that each of the elements recommended therein should ideally be provided. Earlier drafts of our article did, indeed, include more of these methodological details, but several pieces of information were deleted as we worked to make our manuscript fit the Brief Report format. We support provision of additional space to permit more rigorous adherence to the CONSORT guidelines in all future reports in the *Journal*. It will be up to the *Journal's* editors, of course, to determine if this is feasible.

We will use this opportunity (and space) to provide some of this additional information. The subjects included in this report were all 19 participants who met the entry criteria and agreed to participate; no other subjects were enrolled. We believe that we discussed the unique characteristics of our subjects (i.e., treatment-resistant male combat veterans) and the resultant probably limited generalizability of our results in sufficient detail in the report. Randomization was conducted by using a random-numbers table prepared by our research pharmacy. The code for randomization was also maintained in the pharmacy, where it could be broken in an emergency—which did not occur. Medications (active drug and identically appearing matching placebo tablets) were provided by the drug's manufacturer and dispensed by the research pharmacy. We have no reason to believe that the double blind was compromised at any point during the study. We did not report effect sizes in the report, but these were easily calculated from the data provided (i.e., mean change/standard deviation). The effect size (≈ 1.0) was actually large for the drug-placebo difference in change on the Clinician-Administered PTSD Scale, as it would have to be in order to be statistically significant with such a small group. Response rates were clearly indicated in the article.

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Mental Disorders Among Military Personnel

TO THE EDITOR: We greatly enjoyed the article by Charles W. Hoge, M.D., et al. (1). They made excellent points on mental

illness and mental health care in the military. Our one concern was a possible underreporting of outpatient substance abuse. Data collection systems for patient encounters in military outpatient settings challenge providers in keeping accurate accounts of client diagnosis or even encounters. As part of a quality-improvement program at military mental health clinics, 1 year's worth of charts (for nearly 900 individual patients) were reviewed for diagnosis, health care use, comorbid substance use disorders, severity of illness, and health care use. We generally agreed with the findings of the authors but note that our rate of substance use disorders, specifically alcohol abuse and dependence, was much higher and closer to 50% for all 900 clients. Other published studies have also reported fairly high rates (2, 3). This did not take into account the local drug and alcohol clinic's population, which the authors presumably included in their analysis. Our mental health clinic and drug and alcohol clinic, in which this quality assurance project was conducted, did not consistently use the collection systems referred to in the article until 1999 or later. It is likely that other clinics did not use the data systems accurately as well. Oftentimes providers code for only for one diagnosis (e.g., depressive or adjustment disorder); hence, substance use disorder is not recorded. We would be interested in the authors' comments on these issues and thoughts for future study.

The views expressed in this manuscript are those of the authors and do not reflect the official policy or position of the Department of the Army, the Department of Defense, or the U.S. government.

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TO THE EDITOR: As a former military psychiatrist and military psychiatric researcher with over 12 years of service, I read with interest the article by Dr. Hoge and colleagues. Their main conclusions 1) that mental disorders are common in military personnel as discharge diagnoses and 2) that far more attrition from military service occurs after mental disorder diagnoses than from physical disorders is far from "striking." What is surprising is the apparent lack of appreciation by the epidemiologist authors of the military medical regulations that specifically discriminate against service members with mental disorders to a far greater degree than those with primarily physical ICD-9 diagnoses. One could argue the merits of this discriminatory approach for some personnel in sensitive positions (a point not made in the article), but the fact remains that such regulations would readily account for this differential attrition rate. The authors cited the widespread use of antidepressants by military personnel as an ex-