Army service) and half were women (compared with roughly 15% in the Army), leading to a substantially inflated rate of depression compared with the rate that would have been obtained if the survey composition had been adjusted to be comparable with that of the Army. By contrast, our comparison sample was carefully constructed from the nationally representative National Comorbidity Survey Replication to be identical to the active-duty Army population on the joint distributions of sociodemographic variables and to exclude people with serious health problems that are exclusions for Army service. Soldiers in the Army STARRS survey had substantially higher rates of current mental disorders than respondents in that representative comparison sample.

We also reported that most active-duty soldiers with current mental disorders had onsets of their first mental disorders before their age at enlistment.[1] Hoge et al based their criticism of this conclusion on our use of retrospective reports to define age at onset. This criticism ignores 2 important points. First, an extensive literature cited in our article, but ignored by Hoge et al, documented that prospective data converge with retrospective data in finding early age at onset distributions of mental disorders consistent with those found in our report.[4] Second, Army STARRS used the same assessment methods in a separate survey of approximately 57 000 Army recruits who had just begun Basic Training.[5] We found high rates of prior lifetime mental disorders in that survey of new recruits of the sort implied by the retrospective results in the articles critiqued by Hoge et al, providing strong support for our claim that most soldiers with current mental disorders had first onsets before enlistment.

Finally, Hoge et al stated incorrectly that we claimed Army suicides are a "direct result" of deployment. We made no such claim.[3] Indeed, we stated clearly that causal interpretations of suicide trends cannot be made from the naturalistic data we reported. In his editorial about our articles, Friedman[6] emphasized that he was clear on this point. We noted that the overall Army suicide rate over the 2004-2009 period we studied was higher among the currently and previously deployed than the never deployed, but we also noted that the increase in the Army suicide rate over that period occurred not only among the currently and previously deployed, but also among the never deployed. And we reported in another Army STARRS article that the association between deployment history and suicide varies with rank and time in service.[7] For example, the suicide rate of officers is actually higher among the never deployed than the currently or previously deployed. The criticism by Hoge et al misrepresents our findings and interpretations.

Suicides and mental disorders among servicemembers are serious issues that require serious scientific investigation. The challenges involved in research aimed at elucidating the causal mechanisms underlying these outcomes and designing interventions to prevent them from happening are great owing to the complexity and rarity of the phenomena and the difficulties in making plausible causal inferences from data with the range of potential selection biases found here (most notably that risk factors for these outcomes might be related to volunteering for Army service, selection out of deployment once in the Army, exposure to a variety of experiences thought to be risk factors for suicide, and early attrition from Army service). Awareness of these complexities underlies the logic of our analyses and interpretations.

We welcome thoughtful commentaries on this work and are eager to learn of genuine problems with our logic or interpretations, as well as to hear suggestions for better ways to produce actionable recommendations for effective interventions.

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Disclaimer: This letter does not necessarily reflect the opinions of the National Institute of Mental Health or the US Department of Health and Human Services.

Correction: This article was corrected online August 6, 2014, to omit information from the first paragraph.

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Very Small *P* Values

To the Editor We are deeply concerned about the article by Hartz et al[1] published in *JAMA Psychiatry*. Most of our concerns have to do with the extremely small *P* values reported in the text proper and in the tables. Several of them appeared to be beyond the precision capability of the statistical software (SAS) that was used. For example, in their Table 3, an odds ratio of 3.96 (95% CI, 3.61-4.35) is said to have an associated *P* value of 1.2×10^{-188}. The other 4 *P* values in that table were even smaller. Whether or not those *P* values have been correctly calculated, there is no reason for reporting anything other than $P < .0001$, which is the default of SAS for very small *P* values. (The authors also vacillated between *P* being equal or less than a certain value.) Furthermore, there is no need for both confidence intervals and *P* values. If an odds ratio of 1 is not in-

side the confidence interval, the obtained odds ratio is statistically significant. The odds ratio is the measure of the effect size; the *P* value is not. And the former takes precedence over the latter.

There is more. The operational definition of age is also a problem. In the Statistical Analysis subsection of the Methods section, age was trichotomized; in the Results section, it was a dichotomy. And elsewhere, the reader is led to believe that it was treated as a continuous variable. Furthermore, the average age of the cases was much greater than the average age of the controls, and there were many more European American and male cases than controls. The authors claimed to have adjusted for those differences but that is very difficult to do when the disparities are so large.

In the Discussion section, the authors claimed that the study was not a population survey and the individuals were not randomly sampled. Then why all of the inferential statistics?

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1. Hartz SM, Pato CN, Medeiros H, et al; Genomic Psychiatry Cohort Consortium. Comorbidity of severe psychotic disorders with measures of substance use. *JAMA Psychiatry*. 2014;71(3):248-254.

In Reply Our study showed that, among individuals with severe mental illness, the rates of tobacco, alcohol, and other substance use are dramatically higher than in the general population.[1] The criticisms made by Knapp and Hayat highlight long-standing statistical controversies: effect size vs *P* value and proper adjustment for confounding variables.

Small *P* Values | While effect sizes are important to evaluate the clinical significance of a result, it is essential to include *P* value estimates to evaluate whether the observed association could be explained by chance. Some researchers find that a 95% CI is adequate. In this era of large databases with complex data, which can include numerous individual statistical tests (eg, genetic or imaging data), we feel that it is important to highlight both the effect sizes and the calculated *P* values to help researchers and clinicians integrate results across studies.

The calculation of *P* values uses asymptotic estimates of the normal distribution. As with all estimates, there is a threshold at which the estimates are no longer precise. The SAS Institute uses 10^{-325} as the cutoff for precision (which is what we used in the study), while Knapp and Hayat use .0001. We chose the 10^{-325} cutoff so that researchers who understand the computation limitations of normal approximation estimates and feel there is a substantive difference between .0001 and 10^{-8} and 10^{-100} have the opportunity to make this distinction.

Adjustment for Demographics (Age, Sex, and Race) | Properly adjusting for demographics is important for all studies. However, because humans cannot be randomized to demographic groups, there can never be exact adjustment for these confounders. Given the limitations of human data, the best way to adjust for demographics is to perform multiple analyses with various codings of the demographics in the study. Age is particularly tricky because it is a continuous variable that often has nonlinear effects. In this study, we performed multiple analyses (including multiple codings for age) to ensure that there was no evidence the observed association could be explained by the available demographics. All analyses produced the same result. Because of space constraints, we did not present or describe all permutations of these analyses.

In this study, we identified important substance use disparities between our case participants, who have severe mental illness, and our control participants, who have neither personal nor family history in first-degree relatives of schizophrenia or bipolar disorder. We agree with Knapp and Hayat that our control group did not match our population of individuals with schizophrenia and bipolar disorder as closely as we would have liked in terms of their ages and racial/ethnic backgrounds. Nevertheless, we believe that the results of these comparisons are highly informative and provide an important impetus to improve prevention and treatment efforts for participants with severe mental illness.

Although many additional statistical approaches could be used to analyze these data, we present robust evidence that the use of tobacco, alcohol, and other substances is substantially higher among individuals with severe mental illness than among individuals without severe mental illness—a difference that is both clinically and statistically significant. From a clinical perspective, these findings underscore the importance of recognizing and treating the comorbidity between severe mental illness and substance use to decrease morbidity and mortality in this vulnerable population.

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1. Hartz SM, Pato CN, Medeiros H, et al; Genomic Psychiatry Cohort Consortium. Comorbidity of severe psychotic disorders with measures of substance use. *JAMA Psychiatry*. 2014;71(3):248-254.