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Very Small p-values

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To the Editor: We are deeply concerned about the article by Hartz et al¹ 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 inside 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?