## Letter to the Editor

# **Comments on "The Effect of Mindfulness on Quality of Life among Women with Premature Ovarian Insufficiency: A Randomized Clinical Trial"**

### Dear Editor,

We have read the article "The Effect of Mindfulness on Quality of Life among Women with Premature Ovarian Insufficiency: A Randomized Clinical Trial," published in your esteemed journal with great interest.<sup>[1]</sup> We congratulate the authors for the article and for understanding the needs of this group and addressing them. However, a few issues and concerns need to be addressed.

The author has highlighted in the introduction section that changes in hormone levels, hot flashes, and sleep disruptions are associated with women's quality of life; however, the study cited for this claim gives no such insight.<sup>[2]</sup> We would also like to request the author to throw some light on how they arrived at that particular sample size, as the paper they referred to for sample size calculation does not report S = 18.65. It is also necessary to clarify on what grounds they expected a 15-unit increase in the mean score of quality of life in women with premature ovarian insufficiency. Furthermore, we would also like to know how the allocation concealment was ensured before randomization, as it is not mentioned in the method section.

In the result section, also there are multiple issues, including no statistically significant results (P values) are reported in Table 1, in the demographic characteristics of the participants. Second, the authors have failed to report the appropriate statistical tests in the legend of Table 2, specifically regarding the P values reported in column number 5. Third, the study reports an increased frequency of hot flashes in the control group; conversely, Table 3 represents a reduction in the hot flashes. We also assume that there is a possibility of skewness despite the reported ShapiroWilk test of normality, especially in the 3<sup>rd</sup> month follow-up data of the intervention arm, which reports approximately equal mean  $\pm$  standard deviation (6.74  $\pm$  6.34). To add on, the baseline hot flashes frequency per week was not comparable statistically (P = 0.01), and the authors should have performed an adjusted analysis to nullify the baseline imbalance. Finally, the effect size and confidence interval could have been reported to know the relative effect of intervention and preciseness of the estimates,

respectively. Considering the multiple errors in the methodology and results in part, we express serious concerns regarding the replicability and generalizability of the conclusion of this article.

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#### **Conflicts of interest**

There are no conflicts of interest.

Payal Sharma, Sriloy Mohanty, Aman Agarwal, Ranjana Rana<sup>1</sup>

Center for Integrative Medicine and Research, All India Institute of Medical Sciences, <sup>1</sup>Department of Reproductive Biology, All India Institute of Medical Sciences, New Delhi, India

Address for correspondence: Dr. Sriloy Mohanty, Center for Integrative Medicine and Research, All India Institute of Medical Sciences, New Delhi, India. E-mail: sriloy21@gmail.com

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