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Letter to the Editor: Effectiveness of Family-Focused Therapy in Bipolar Disorder: A Randomized Controlled Trial

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Dear Editor,

We were highly interested in reading the article by Yosefi Tabas et al. (1), titled "Effectiveness of Family-Focused Therapy in Bipolar Disorder: A Randomized Controlled Trial," which was recently published in your prestigious journal. We extend our congratulations to the authors for their significant work and publication. However, we wish to highlight several points:

The efficacy of Family-Focused Therapy (FFT) in managing bipolar disorder is well-documented and supported as a supplementary therapy in numerous psychiatric textbooks (2, 3). Therefore, the primary research question introduced by the authors does not seem to unveil a significant gap in the existing body of knowledge despite being presented as the main research objective.

Moreover, the study's design falls short of fulfilling the objective outlined in the article's Objectives section, which aims to evaluate the effectiveness of training patients and caregivers together versus training them separately.

Additionally, the article would benefit from clearer articulation of its central objective and more detailed methodological explanations. For example, it lacks information on the analytical strategies and software employed. The methods of randomization and blinding, as well as the validity and reliability of the Farsi versions of the scales, are not disclosed.

The criteria for inclusion merely require a diagnosis of bipolar disorder by a psychiatrist without reference to standardized diagnostic guidelines (e.g., DSM-5). The report fails to account for critical confounding factors – such as medication stability/history, bipolar subtype, duration of illness, and current mood episodes – in both groups, which are essential for evaluating their impact on the study outcomes. Furthermore, while the educational levels are initially categorized as "higher school," Table 2 reveals three distinct levels. Clarifying the specific type of relationship of the affected relative with Bipolar Disorder (BPD), or restricting the scope to only first-degree relatives, would have been more appropriate, considering their significance.

While the authors have adapted the therapy model originally designed by Goldstein (4), they reduced the number of sessions from 21 to 15 without providing a rationale for this decision or referencing studies that support an intervention format with 12 sessions. The only cited article employed 21 sessions, as originally proposed. In reality, the justification for reducing treatment sessions is primarily found in studies focused on young adults and adolescents at high risk for BD or psychosis, which utilized 12 (5) or 18 sessions (6). Thus, the decision to reduce the session count lacks solid precedents for the targeted adult BPD demographic in this study. The objective could have been to investigate the impact of a reduced number of therapy sessions on adults with BPD.

Additionally, the manuscript requires clarification in several areas, including the discrepancies observed between the final analyzed sample mentioned in the Results and the figures provided in Figure 1, Tables 3 and 4. Furthermore, the last 6 paragraphs of the Results section, which discuss the interpretation of the findings, would be more appropriately placed in the Discussion (7, 8).

We hope our feedback contributes to strengthening this important area of research. Once again, we applaud the authors for their contribution to expanding the knowledge base on psychosocial interventions in bipolar

disorder.

Footnotes

Authors' Contribution: Study concept and design: M. R. F. B. and S. H.; drafting of the manuscript: M. R. F. B. and S. H.; critical revision of the manuscript for important intellectual content and Study supervision: M. R. F. B.

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