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Fwd: Decision on your manuscript #MIFU-D-24-00436R1

Ilaria Colpizzi <ilaria.colpizzi@unifi.it>
A: Corrado Caudek <corrado.caudek@unifi.it>

20 gennaio 2025 alle ore 09:02

----- Forwarded message -----

Da: **"Christian U." Krägeloh** <em@editorialmanager.com>

Date: mer 18 dic 2024 alle ore 21:18

Subject: Decision on your manuscript #MIFU-D-24-00436R1

To: Ilaria Colpizzi <ilaria.colpizzi@unifi.it>

Dear Dr. Colpizzi,

Thank you for submitting your manuscript, "State Self-Compassion Dynamics: Partial Evidence for the Bipolar Continuum Hypothesis" for review by Mindfulness.

Your revised manuscript was reviewed by two of the three original reviewers, and I have read the work myself. Although the reviewers and I saw significant improvements, some minor lingering concerns remain. Therefore, based on my own review and the comments by the reviewers, I believe further work is needed and I invite you to revise and resubmit the paper. There are a number of specific recommendations made by the reviewers for you to address in your revision and, since these are delineated clearly in their comments, I will not reiterate each of them here. Please take care to enumerate each of these in your response letter and explain how you have addressed them in your revision. Please note that submitting a revision of your manuscript does not automatically guarantee eventual acceptance, and that your revision will be subject to re-review before the final publication decision is made.

Your revised manuscript is due on 16 Feb 2025

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Your username is: ilaria_colpizzi

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Please click "Author Login" to submit your revision.

We look forward to receiving your revised manuscript.

Sincerely,

Adrian J. Bravo, PhD
Associate Editor
Mindfulness

Reviewers' Comments:

Reviewer #1: I reviewed the original submission of this manuscript, and I appreciate the authors' responsiveness to feedback. I think the revision provides a stronger test of the hypotheses and is generally clearer, but there are some remaining significant and minor issues, as described below.

1. Please define "ecological momentary assessment" when it is first introduced in the introduction.
2. I found the statement that the current study improves on existing EMA studies in that prior study durations are

shorter (< 7 days) to be not particularly compelling, especially since the studies presented here only include 10 and 16 days (albeit over longer periods). Why does having 3-9 more days make a big difference in "capturing the full complexity of state self-compassion dynamics"? More broadly, the sampling design of the current study is unusual (1 day per week over several months); a rationale should be provided, along with consideration of the strengths and limitations of this approach.

3. The first hypothesis on p. 6 was very vague ("...expected to display pronounced temporal dynamics")-- what would this mean, and how would it be operationalized? Isn't this already known from prior EMA studies?

4. While I think the additional analyses of structure and idiomonic analyses strengthen the paper, I found their presentation to be lacking in detail and presented ineffectively. They were simply tacked on to the end of the paper, after the Study 2 discussion, without even an indication of which sample was analyzed and no foreshadowing in the study set up, which was quite odd and confusing. Instead, they should be integrated into the relevant study from the beginning, set up in the introduction, included in hypotheses, described in data analysis, etc., just like any other analysis would be. In addition, there is so little detail provided in the main text as to be mostly uninformative, especially for the multilevel CFA. It is essential to describe and consider factor loadings (particularly in the bifactor solution, which are notable), specific values of fit indices, factor correlations (i.e., the two factors are so strongly correlated so as to suggest redundancy, even if they give better fit), and consideration of indices such as omega hierarchical in the bifactor analysis. Right now, important information is overlooked and the reader cannot understand the main points without reading all of the supplemental materials. For the idiomonic analyses, there is not a single statistic reported in the main text to support the conclusions. While I understand there are space limitations, I think it is essential to include sufficient detail in the main text for the reader to understand and evaluate each analysis, and I think other sections could likely be reduced.

5. Related to point #4, it is striking that there are no tables reporting results and just a single figure in the main text. While I appreciate the detail in the online supplement, I think too much has been offloaded there and that the authors should report results for the main analyses in tables or figures in the main text.

6. For the finding that a subset of people had positive associations between UCS and CS, one methodological explanation could be careless responding/response styles (e.g., selecting the same response for all or nearly all items would lead to a positive correlation that is not substantively meaningful). Could the authors evaluate whether this may explain the finding? Please also describe the procedures they used to detect any careless or otherwise invalid responses, which are particularly likely in EMA assessments and require identification and removal.

Reviewer #2: Overall this revision seems good. There is one thing that is problematic, however. In several places the authors describe Neff as having a unidimensional view of self-compassion. For instance, on page 33 the authors refer to "Neff's conceptualization of self-compassion as a singular, bipolar construct (Neff, 2022, Neff, 2023)." This is not an accurate characterization of Neff's model. In Neff's publications, self-compassion is described as a multi-dimensional rather than singular construct. Saying that CS and UCS operate in tandem along a bipolar continuum is not the same as saying that self-compassion is unidimensional. Rather, Neff argues that "self-compassion represents a dynamic system in which the various elements of self-compassion are in a state of synergistic interaction" (Neff, 2016a, 2016b, Neff & Toth-Kiraly, 2022), and that this system is best conceptualized as moving along a continuum from UCS to CS (Neff, 2023). This needs to be clarified throughout the manuscript. Rather than setting up a simplistic straw man argument, the authors would be better served by discussing the difficulties of measuring dynamic systems, the pluses and minuses of using a total score rather than two (including loss in variance when using CS and UCS separately which greatly reduces predictive power), and the challenges and complexities of trying to use statistical modeling to capture messy real life processes.

There is additional documentation related to this decision letter. To access the file(s), please click the link below. You may also login to the system and click the 'View Attachments' link in the Action column.

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