By Dr. Andrew Jones

13 Mar 2025

Revision round #1

Decision: *Revision needed*

Hi,

The reviewers have examined your stage 2 submission and I think we're pretty close to a recommendation. There are a couple of minor points raised that I'm hopeful you can address.

The point about effect sizes (and interpretations being somewhat arbitrary based on thresholds), it might be helpful to consider this paper:

https://journals.sagepub.com/doi/10.1177/2515245919847202

Or maybe even consider reporting common language effect sizes.

https://pubmed.ncbi.nlm.ni h.gov/34881941/

Hope this helps!

> **Authors**: Dear Dr. Jones,
>
> We thank you and the three reviewers for dedicating your time to reviewing our stage 2 submission, as well as the previous one at stage 1.
>
> We appreciate your helpful suggestions. In addition, we carefully considered all comments when revising the manuscript. This experience in PCI-RR has been a formative one.
>
> Thank you for your dedication.
>
> Sincerely,
>
> The authors

Review by Josip Razum

(For ease, from now on "R1")

R1: Thank you for the opportunity to review stage 2 of this interesting manuscript.
The authors did adhere to the procedures described in Stage 1.
Their introduction, rationale and stated hypotheses are the same as in Stage 1.
The data seem to be able to address the study aims.
There is an additional analysis not mentioned in the Stage 1 protocol, i.e., comparing overlapping HDA- and SUD-based PSMU cases with non-overlapping SUD-based cases, but this analysis is justified.
The conclusion that HDA, and especially the HDA2 scoring method, may represent a valid conceptual alternative to the GD-based scoring is supported by the data.

> **Authors' reply (RE) to R1:** Dear Dr Razum, we appreciate your time reviewing this stage 2 submission and your positive feedback overall. We addressed your comments point by point.
>
> Best regards, the authors

R1: In the discussion section, the relation of these findings to the ICD-11 criteria for gaming disorder could also be mentioned. I know this was already mentioned in the introduction, but it could be elaborated in the discussion as well. Which unique element(s) or advantages over ICD-11 does HDA bring to the table?

> **RE to R1:** Thank you for this encouraging suggestion. It was not possible to say much of substance about the ICD-11 definition of GD from the HDA perspective given that neither our data set not our analyses addressed this issue. However, in response to this comment, we further expanded the discussion (in the "Future directions …" section) of the point mentioned in the introduction about the potential for analyzing the conceptual validity of ICD criteria from the HDA perspective. The following was added to the text:
>
> "However, the HDA perspective may guide validity assessment of the ICD-11 GD definition. For example, despite trying to capture loss of control over the behavior, the essential feature "Increasing priority given to gaming behaviour to the extent that gaming takes precedence over other life interests and daily activities" might not necessarily reflect dysfunction and/or harm. Furthermore, the essential features "Continuation or escalation of gaming behaviour despite negative consequences" and "The pattern of gaming behaviour results in significant distress or impairment in personal, family, social, educational, occupational, or other important areas of functioning," are not sufficiently differentiated and may relate to harm that is not clearly a result of a dysfunction but may be socially generated. The attempt at specifying dysfunction via the "impaired control over gaming behavior" also warrants exploration, because habituated behavior supported by a social context can become difficult to moderate in normal as well as disordered conditions. In sum, the adoption of the HDA can raise novel questions about validity that suggest possible improvements in criteria, increasing precision in diagnostic conceptualization.".

R1: Finally, I agree with what the authors said in the Limitations section: the current analysis used existing data and not spefically tailored items for social-media-induced dysfunction and harm. Besides stating this in the Limitations section, I believe it should be mentioned at the beginning of the Future directions section as well and the section could be organized with this in mind. This section esentially discusses ways to better tailor HDA to PSMU (and to behavioral addiction in general) but this is not stated at the beginning, which is why it reads somewhat confusing. Especially since this is then stated for the first time in the Limitations section that follows. Also, the Future directions section could address PSMU more specifically.

> **RE to R1:** We agree with R#1 comment and added the following sentence to introduce the "Future directions" paragraph:
>
> "In the present study, we used existing data collected using items on social media that were not developed according to the HDA. We thus offer some suggestions for encouraging future research efforts to apply HDA to the study of PSMU and addictive disorders in general.".
>
> Please note that this section contains specific suggestions for expanding the study of HDA in PSMU, such as the proposal to study fear of missing out as an indicator of dysfunction, and body image dissatisfaction and related behaviors as indicators of harm. Considering this information, the fact that the manuscript is already dense enough, and the need to address other reviewers' comments as well, we leave this out for a future contribution and future research reflecting on the usefulness of the HDA approach.

Review by Gemma Lucy Smart

(R2)

R2: This paper represents a methodologically sound investigation of the Harmful Dysfunction Analysis (HDA) framework applied to problematic social media use. The authors have successfully executed their registered protocol, with the methods and analyses closely following their Stage 1 submission. The modifications made in response to Stage 1 reviews have strengthened the study's robustness.

The data quality is appropriate for testing the authors' hypotheses. The large sample sizes from both Switzerland (N=7,510) and Hungary (N=3,789) provide adequate statistical power, while the prevalence rates show good distribution without floor or ceiling effects. The validation measures examining physical and mental health indicators demonstrate appropriate variability, and the sensitivity analysis using Hungarian data provides valuable cross-validation of the findings.

One minor point is that while the team composition and expertise were detailed in the Stage 1 submission, this information could be restated in the final paper to provide readers with full context for the study's execution.

The authors have maintained transparency throughout, clearly identifying their single unregistered analysis comparing overlapping HDA and SUD-based cases. This exploratory analysis is both methodologically sound and adds valuable insight to the primary findings. The statistical approaches are appropriate and well-executed.

The conclusions drawn are measured and well-supported by the evidence presented. The authors acknowledge limitations appropriately, including the reliance on self-report data and the cross-sectional nature of the study. Their findings regarding HDA's utility in reducing false positives while maintaining validity are convincing, particularly the stronger validation results for HDA2.

While the varying prevalence rates (4.2% to 33.2%) across different criteria might raise questions about measurement consistency, this variation actually supports the authors' argument about the importance of careful diagnostic thresholds. The link between social media use and gaming disorder criteria could perhaps be strengthened, but this limitation does not significantly detract from the study's overall contribution to understanding behavioural addiction assessment.

In summary, this is a well-executed study that makes a valuable contribution to addressing over-pathologisation concerns in behavioural addiction research. The authors have adhered to their registered protocol while maintaining transparency about their methods and limitations.

> **RE to R2:** Dear Dr Smart, thank you for your meticulous review of our work and your positive appraisal. We added information about the authors' specific contribution in the revised version of the manuscript (on page 2) as recommended. Regarding the need to further strengthen the link between social media and gaming disorder criteria, the introduction already comprehensively discussed the confirmatory approach to behavioral addictions, according to which criteria inevitably overlap, and the focus on impaired control/self-regulation over the behavior (and related negative consequences) according to the HDA perspective. Nevertheless, we added the following sentence in the introduction to clarify this issue:

“Empirical findings on PSMU and GD co-occurrence (Chen et al., 2021; Moreno et al., 2022), symptom similarities (Burén et al., 2021) and correlations (Shmulewitz et al., 2024; Zarate et al., 2022) support their close relationship as impaired forms of engagement with technology-generated stimuli. These findings indicate overlapping diagnostic challenges in distinguishing pathology from normal variation in these two areas. PSMU thus offers an appropriate domain for initial exploration of the potential for HDA diagnosis with an eye to later application to GD.”.

Best regards, the authors

Review by Veli-Matti Karhulahti

(R3)

R3: During Stage 1, I was a reviewer for one round and it turned out that one of the authors was serving as a reviewer for a paper where I am an author. Therefore, I dropped from the process, as cross-reviewing can easily generate COIs. As I received a review invitation for this Stage 2 version, I decided to accept the invitation with the following caveats
- I'm curious about the results so I'll read them anyway, thus I can share some notes while doing that
- These notes should not be considered as a "review" but extra feedback, and proper reviewers should be invited to carefully review the full Stage 2

> **RE to R3:** Dear Dr Karhulahti, we appreciate your interest in the topic of this manuscript and that you took the time to comment on its contents. We considered your suggestions when revising the manuscript (additional details are below).
>
> Best regards, the authors

R3: Here are my notes:
- Based on one initial read, the' reporting seemed solid. In particular, reporting language was approriate as it did not push confirmation or oversell the results; it all seemed very honest and clear.
- In one small part, I'd consider toning down a bit: "These findings suggest that the HDA approach, as it is designed to do, eliminates false positive diagnoses..." --> I would say "might eliminate" to be safe, as there's indeed no clinical evidence (which is correctly mentioned later).

> **RE to R3:** Thank you for this encouraging comment. We changed the verb form to communicate a possibility, as suggested.

R3: - For the most central results regarding overlap, I would help the reader by visualising them at least via one Venn diagram.

> **RE to R3:** We appreciate this helpful suggestion. We followed it, adding a figure including four distinct subfigures/Venn diagrams (directly in the main text for Swiss data and in the supplement for Hungarian data).

R3: - This is the most important point I have: when the paper reports small, medium, and large effects, I would really try to transform those effects into some raw, real-world effects. There are many ways to do this, but any will be better than rules of thumb thresholds. The discussion could be a place to address this in more detail, eg. by comparing obtained effect sizes to those found in similar designs/tests in other mental health research fields. The PCI RR author guidelines have some tentative guidance/links on this topic.

RE to R3: This is a valuable suggestion. Considering the explorative nature of the study, we initially preferred not to stress too much the magnitude of the effects we found. Nevertheless, we also understand that clarifying them in the discussion might be useful. Accordingly, we considered adjusted effects of HDA-based PSMU on psychosomatic symptoms (standardized mean difference of 0.52 for HDA1 and 0.83 for HDA2) and life dissatisfaction (0.33 for HDA1 and 0.54 for HDA2) compared to non-cases. We compared our findings to previous findings on life satisfaction and psychosomatic symptoms in samples of children/adolescents. Please note that differences between scoring methods were not discussed because they were non-significant (HDA vs. GD-based cases) or resulted from entirely new non-preregistered analysis (HDA and SUD vs. SUD-only-based cases). Effect sizes from original studies were converted to Cohen's d using the calculator at https://www.escal.site/ or they were calculated using means and standard deviation using the calculator at https://lbecker.uccs.edu/#means%20and%20standard%20deviations.

The following was then added to the discussion:

"To provide an idea of the putative real-world impact of these differences, we compared our findings - i.e., adjusted effects of HDA-based PSMU compared to non-cases on psychosomatic symptoms (standardized mean difference of 0.52 for HDA1 and 0.83 for HDA2) and life dissatisfaction (0.33 for HDA1 and 0.54 for HDA2) - to previous findings on life satisfaction and psychosomatic symptoms in samples of children/adolescents. The effects of HDA-based PSMU on life satisfaction and psychosomatic symptoms are larger than the effects exerted by physical activity on the same outcomes (Cohen's d between 0.16 and 0.22) (Molcho et al., 2021). The effects we found are also larger than the effects exerted by school pressure on life satisfaction (Cohen's d of 0.18) and bullying on life satisfaction and psychosomatic symptoms (Cohen's d of 0.33 and 0.24, respectively), and at least of equal magnitude to the effect of school pressure on psychosomatic symptoms (Cohen's d of 0.45) (Skoric et al., 2023). In addition, the effects we found are larger than the effect of language/cultural background on life satisfaction (Cohen's d from 0.04 to 0.40) and of similar magnitude to the effect of adult and peer support (Cohen's d from 0.50 to 1.1) (Emerson et al., 2018). These comparisons seem robust as they were replicated in the analysis of a very large database (Ottová-Jordan et al., 2015). It is worth mentioning that if we compare our effect sizes with the average effect reported in the psychological literature (Funder & Ozer, 2019), the effect sizes we found are larger than the median effect size in preregistered studies (r = 0.16 or Cohen's d = 0.32) (Schäfer & Schwarz, 2019)".

R3: - At the very end, the paper concludes by addressing design features. Although I fully agree these are very central to the field, the section appears entirely separate from the HDA topic. If you wish to keep that part, I'd suggest trying to build more explicit bridges (or even hypotheses) between design features and HDA. Veli-Matti Karhulahti

RE to R3: We appreciate your suggestion regarding addressing the connection between design features and the HDA. This part of the text was added in response to one of the reviewers' comments during the review process of the stage 1 manuscript, so we needed to keep it. Design features could promote pathological use in users already presenting

vulnerabilities in emotion and self-regulation (thus acting as a moderator of the relationship between impaired control and addictive disorder). Still, they could alternatively yield nondisordered behavior that looks more like dysfunction. We attempted to reflect this link between design features and HDA assessment indirectly with our comment that structural characteristics could prolong the time spent using social media "independently of pathology/nonpathology." In response to your comment, we made this issue more explicit by adding the following clarification at the end of the relevant sentence:

"These social media mechanics may thus pose additional challenges to the application of the HDA in distinguishing dysfunction from contextually amplified normal variation".

Given the paper's length, we thought it best not to further elaborate on this point.