Decision for round #2: *Revision needed*

Thanks to the reviewers for their comments. As I could not get a follow-up from one of the reviewers I invited a further reviewer to take a look. I think their comments are largely points for clarity which could be addressed and then we can hopefully recommend the report.
Many thanks.

by Andrew Jones, 16 Aug 2024 19:55
Manuscript: https://osf.io/j8dcu
version: 3

Authors reply ("RE") to the Recommender:

Dear Recommender Dr Jones,

Thank you for your consideration of our stage 1 RR for in-principle acceptance.
We thank the two reviewers for their kind attention in going through the second submitted version of the manuscript (labeled as version 3 on OSF).
We have carefully considered their suggestions for revising the text, which we believe helped us to further improve content clarity. We welcome further comments if any.

Kind regards,
Simone Amendola (on behalf of all authors)

Review by Gemma Lucy Smart, 28 Jun 2024 16:50. From now on "R#1".

R#1: Thanks once again for the opportunity to review this paper and the tracked changes. I appreciate the rigor with which the authors have responded to my comments and I am happy with the changes that they have made.

    RE to R#1: Dear Dr Smart, thank you for your positive feedback and for your time in reviewing the new version of the manuscript.

R#1: In response to their request for a couple of references from game studies, I have some references that *may* be of interest - although some come from within psychology: Griffiths & Nuyens, 2017; Karlsen, 2013; and from game studies I think this paper discussing the structural elements of games is important for comparing to social media: Costello and Edmonds, 2007. Many thanks again.

    RE to R#1: We appreciate these useful references and agree on the importance of considering understudied aspects of technology use (like structural characteristics) and the adoption of social and cultural perspectives for analysis in future research. We now acknowledge this in the "limitations" section of the manuscript as follows:

    "*In our analysis, we focus on addictive disorders of social media use at the individual level of analysis. However, social media use is inevitably related to the context in which it occurs and can also be analyzed in terms of social and cultural perspectives (Karlsen, 2016) that may render these activities unproblematic and pleasurable (Costello & Edmonds, 2007). Moreover, the structural characteristics of social media (e.g, like-button, read-receipt functions, endless scrolling, personalization of content, push notifications, time restrictions of content) may influence users' behaviors independently of pathology/nonpathology, prolonging time spent using them (Flayelle et al., 2023; Montag et al., 2019; Montag & Elhai, 2023). The effects of design elements have been more investigated for gaming (Flayelle et al., 2023; Griffiths & Nuyens, 2017) whereas research is in its infancy for social media use (Alutaybi et al., 2019; Montag & Elhai, 2023; Purohit et al., 2020). As in gaming research, future research on social media use adopting a wider perspective incorporating both social and structural mechanisms will fill a relevant gap in the literature (Karlsen, 2016).*".*

    Kind regards,
    The authors

Review by Josip Razum, 16 Aug 2024 13:56. From now on "R#2".

R#2: Thank you for the opportunity to review this interesting report. I'd like to thank the previous reviewers in round 1 on their mindful and comprehensive suggestions, which I think have improved the manuscript. However, I think there is still room for further improvement. I'll number my comments chronologically to make reading easier.

> **RE to R#2:** Dear Dr Razum, thank you for your time in reviewing the previous version of our manuscript. We appreciate your positive feedback overall and considered your valuable comments and suggestions in making changes to the main text. Please refer to our replies below. Best regards

R#2: 1. On page 5: "Other longitudinal studies present a confusing picture in which PSMU correlates with such conditions as anxiety, insomnia, and depression, but at an individual level is not necessarily causally related to such conditions (Chang et al., 2022; Lin et al., 2021)."
I'm not sure that this sentence accurately reflects the findings of the study by Chang et al. (2022). For example, Chang et al. (2022) found a reciprocal relationship between anxiety and future problematic social media use at the within person level. Please revise this sentence accordingly. I would not say that these studies can lead us to conclude that PSMU "at an individual level is not necessarily causally related to such conditions".

> **RE to R#2:** Thank you for your attention in going through the manuscript and alerting us to the need for a correction. You are quite right that Chang et al. did find a significant but weak effect of PSMU on subsequent anxiety (stand. estimate = 0.017). Moreover, PSMU seems to influence subsequent depressive symptoms (see Figure 2 in Chang et al.) although the effects found were not stable, i.e., they were significant for one point only (from PSMU-t2 to depression-t3, but not from PSMU-t1 to depression-t2). There are issues with Change et al.'s sample retention and imputation methods that suggest additional caution in considering these weak results. We have corrected the sentence in question to now read:
> "*Other longitudinal studies present a confusing picture in which PSMU correlates with such conditions as anxiety, insomnia, and depression, but at an individual level it is at most weakly and inconsistently causally related to the subsequent presence of such conditions* (Chang et al., 2022; Lin et al., 2021)."

R#2: 2. On page 5: "This suggests that, while time spent playing video games seems to play a major role in determining diagnosis under current approaches, it may not be an effective indicator for validly differentiating high versus pathological involvement, indicating a challenge to current approaches."
Altough these studies show that time spent playing video games does correlate with gaming disorder symptoms whereas not neccesarily being problematic, I would not say that it plays a major role in determining the Gaming Disorder diagnosis. None of the Gaming Disorder criteria, neither in DSM-5 nor in ICD-11, directly involve time spent playing video games. In DSM-5 it is mentioned within the

diagnostic features of GD that problematic gamers typically spend upwards of 30 hours per week playing video games, but this is merely a description, not a criterion. Please revise this sentence accordingly.

> **RE to R#2:** Thank you for pointing out the misleading wording. We have rephrased the sentence as follows:
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> "*This suggests that, while time spent playing video games is associated with GD diagnosis under current approaches, it may not be an effective indicator for validly differentiating high versus pathological involvement.*"

R#2: 3. (on page 6) Besides citing the two older studies by Charlton and Danforth, you could also mention systematic reviews that studied the tolerance and withdrawal criteria of Gaming Disorder across a broad range of studies.

A systematic review of the psychometric validity and the appropriateness of tolerance as a criterion for Gaming Disorder (Razum et al., 2023) has found that tolerance lacks relevance in measuring Gaming Disorder. Another review by Kaptsis et al. (2016) has found a paucity of studies investigating GD withdrawal, and in studies they reviewed "Five (50%) of the qualitative studies and six (86%) of the treatment studies reported no withdrawal symptoms in their samples."

Kaptsis, D., King, D. L., Delfabbro, P. H., & Gradisar, M. (2016). Withdrawal symptoms in internet gaming disorder: A systematic review. Clinical Psychology Review, 43, 58-66.

Razum, J., Baumgartner, B., & Glavak-Tkalić, R. (2023). Psychometric validity and the appropriateness of tolerance as a criterion for internet gaming disorder: A systematic review. Clinical psychology review, 101, 102256.

> **RE to R#2:** We appreciate the above suggestion and expanded the discussion to include more recent research and syntheses, as indicated. We report information from the two recommended studies as well as another recent review on withdrawal symptoms (Starzec et al., 2024). Regarding the quoted text from Kaptsis et al 2016, we encourage also to consider the possible explanations provided by the authors for their findings (p.64): "This might indicate: (1) withdrawal was not properly assessed in these studies and therefore not identified (2) withdrawal symptoms might have occurred across a range of presentations of problem gaming but were mild and not clinically meaningful; (3) that severely problematic gamers modify their environment to ensure it is maximally conducive to continuous, uninterrupted gaming, thereby reducing or eliminating the likelihood of withdrawal, or (4) withdrawal may occur in only a specific 'addictive' subtype of problem gaming, and not across all subtypes of problem gaming.". In our opinion, the above mainly indicates a lack of information availability rather than a lack of or, conversely, robust evidence on the validity of withdrawal for the conceptualization of GD. Importantly, it should be noted that these reviews did not provide an analysis of the original studies' quality (despite some discussion was provided in Starzec et al's study) that could have improved the discussion about results interpretation and generalizability. Experimental studies including abstinence were a minority despite withdrawal symptoms needing, by definition, an abstinence period to be studied comprehensively. Such studies should also provide information about non-participation and drop-out, because potentially related to symptom severity. Thus, the high risk of bias should be

carefully taken into account. We believe that the evidence is thus not definitive to inform on the status of GD criteria. In light of the above, the following information was added to the text:

"The primary grounds for criterion selection in the present study are conceptual, in terms of face validity in satisfying the dysfunction and harm criteria of the HDA. However, previous reviews offer useful input to these judgments. A systematic review of the psychometric validity and usefulness of the tolerance criterion for Gaming Disorder (Razum et al., 2023) found that tolerance lacks relevance in measuring Gaming Disorder. Withdrawal is perhaps one of the more controversial but least studied criteria. A review by Kaptsis et al. (2016) found that that many of the reviewed studies reported no withdrawal symptoms in their samples, but overall "the available evidence on Internet gaming withdrawal is very underdeveloped" (p. 58). Starzec et al. (2024) observed that most of the studies of GD withdrawal that they reviewed had no control for abstinence in evaluating withdrawal, raising questions about the validity of responses. In Castro-Calvo et al.'s (2021) Delphi study of expert appraisals of criteria for GD, withdrawal was among the intermediate group with 31% endorsement for diagnostic validity, meeting the study's criteria neither for inclusion (>80% endorsement) nor exclusion (<20% endorsement). It is difficult to draw any conclusions from this weak result for two reasons. First, other criteria that are widely seen as indicative of addictive dysfunction—for example, craving—also fell into this intermediate category, perhaps because they are less prevalent and thus seen as less "important" (which is how the study's question was worded). Second, as noted, recent reviews indicate that withdrawal in GD has not been extensively studied in methodologically adequate ways and so it is not yet a salient, well-defined, and well-supported marker for many in the field.".

R#2: 4. On page 7: "The ICD-11 definition of GD benefited from these suggestions and incorporated changes clarifying that the main symptom of GD is not excessive involvement itself but rather impaired control over gaming, with other classic symptoms of dependence included as possible additional clinical features."

In the ICD-11 definition of Gaming Disorder it is stated: "Gaming disorder is characterised by a pattern of persistent or recurrent gaming behaviour ('digital gaming' or 'video-gaming'), which may be online (i.e., over the internet) or offline, manifested by: 1. impaired control over gaming (e.g., onset, frequency, intensity, duration, termination, context); 2. increasing priority given to gaming to the extent that gaming takes precedence over other life interests and daily activities; and 3. continuation or escalation of gaming despite the occurrence of negative consequences."

Therefore, Gaming Disorder is defined by these three symptoms: impaired control, increasing priority, and continuation or escalation of gaming, with a requirement that the gaming behavior results in marked distress or significant impairment. Nowhere is it stated that impaired control is the main or the only symptom. Please amend this.

> **RE to R#2:** Thank you for catching this misstatement; indeed, the various ICD-11 criteria are coequal. We revised this statement to read:
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> *"The ICD-11 definition of GD benefited from these suggestions and incorporated a criterion that requires not just excessive involvement but impaired control over gaming, reflecting a dysfunction, in addition to the other three criteria of increasing priority of gaming over other*

activities, continuation of gaming despite negative consequences, and significant distress or impairment resulting from gaming. These last three criteria reflect negative consequences or harms resulting from impaired control over gaming according to the HDA. ".

R#2: 5. On page 7: "However, despite providing some suggestions to differentiate GD from normal gaming behavior, the definition of the ICD-11 does not propose specific and effective indicators for discriminating between normal-range (e.g., functional, high-involvement gaming) and disordered pathological gaming."

This claim needs more elaboration. In my view, the ICD-11 criteria do provide a clear threshold between highly involved and disordered gaming: if the person meets all the criteria, they have Gaming Disorder. If they do not meet the criteria, they do not have the disorder no matter how much they play. ICD-11 also describes "boundary with normality", where they state that some gamers play a lot for different reasons, that they may play more in particular contexts such as holidays etc. There is also a category of "hazardous gaming", which involves increased risk while not meeting the full GD criteria - this category could perhaps be better defined.

In any case, the authors need to provide more arguments for their claim that: "the definition of the ICD-11 does not propose specific and effective indicators for discriminating between normal-range (e.g., functional, high-involvement gaming) and disordered pathological gaming.".

https://icd.who.int/browse/2024-01/mms/en#1448597234

> **RE to R#2:** Thank you, we agree that the comments on ICD-11 were not well-stated and require revision. They now read as follows:
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> *"The perceived validity of the ICD-11 criteria by experts is high, with all four reaching a consensus for "inclusion" as diagnostically valid in Castro-Calvo et al.'s (2012) Delphi study, whereas only four out of nine DSM-5-TR criteria reached an inclusion consensus. Moreover, ICD-11 adds useful indicators of the "boundary with normality (threshold)," emphasizing that sheer excessive use without other indicators of disorder does not qualify for diagnosis. However, questions remain about the source of the perceived validity of the ICD-11 criteria and how the precision and conceptual validity of the criteria might best be increased in the future. The present study is a first step toward clarifying whether an explicit HDA approach can provide a path to increased clarity, specificity, and validity.".*

R#2: 6. On page 10: In my view, the authors need to provide a stronger argumentation for why they plan to use withdrawal symptoms as an indicator of dysfunction, as this criterion has been previously criticized as not valid for assessing gaming disorder. In the study by Castro Calvo et al. (2021), a low percentage of included experts thought that withdrawal symptoms have diagnostic validity, clinical utility or prognostic value in assessing Gaming Disorder.

Perhaps problematic social media use may be different, but then the authors need to cite relevant work and/or their rationale. The authors need to provide clear arguments for why they included withdrawal symptoms as an indicator of dysfunction.

Castro-Calvo, J., King, D. L., Stein, D. J., Brand, M., Carmi, L., Chamberlain, S. R., ... & Billieux, J. (2021). Expert appraisal of criteria for assessing gaming disorder: An international Delphi study. Addiction, 116(9), 2463-2475.

> **RE to R#2:** Thank you for your suggestion. Please note that, as we state, our decisions in categorizing the items of the Social Media Disorder Scale were not guided by previous empirical research on the validity of criteria for assessing GD because the point of the present study is to try out a new conceptualization that has been illuminating in some adjacent areas of research and that might reveal how previous formulations have miscategorized cases. The decisions were based on previous studies' consensus judgments and our judgment of each item according to the HDA's conceptualization.
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> Regarding recent research on withdrawal symptoms in GD, including the Castro-Calvo et al. study, please see our reply to your comment n.3 above.
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> We expanded our statement on the rationale for including withdrawal as follows:
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> "*The withdrawal item content was judged as indicating dysfunction because it has been judged a consensus HDA dysfunction indicator in previous studies in adjacent areas of research (see, e.g., Wakefield & Schmitz, 2014, 2015), and because symptoms following pausing of gaming suggest impaired control or self-regulation under the HDA framework. This is also in line with the recent classification of withdrawal symptoms as aspects of obsessive passion (Infanti et al., 2023).*"

R#2: 7. On page 13: The authors thoroughly stated the study limitations and provided suggestions for which theory-driven aspects of dysfunction and harm could be investigated in future studies; however perhaps they could additionally elaborate what would constitute a "true" non-confirmatory application of HDA to problematic social media use. Would this involve interviews with problematic users where possible indicators of dysfunction and harm would be initially investigated?

> **RE to R#2:** Thank you for this helpful suggestion. We have now included that "*Qualitative study findings may represent a valuable starting point for such an exploratory investigation. As well, as the literature develops, a broader network of probabilistic validators could be tapped to offer a more complex and comprehensive test of validity as in earlier HDA studies of alcohol use disorder (Wakefield & Schmitz, 2015).*"
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> However, this is an aspect that goes beyond the present analysis and could be better elaborated subsequently, when discussing the study findings and limitations.