Decision for round #1: *Revision needed*

The two reviewers have been comprehensive (I thank them).
They see plently of merit in this, but there are some suggestions to improve the rigour. Furthermore, they both seem in agreement about your framing and conceptualisation.
I hope you are able to address their points in a revision.

by Andrew Jones, 22 Mar 2024 11:53
Manuscript: https://osf.io/efv56
version: 1

Authors reply ("RE") to the Recommender:

Dear Recommender Dr Jones,

We thank you for considering our stage 1 RR for in-principle acceptance. We also thank the two reviewers for their kind attention in going through the first submitted version of the manuscript. We have carefully considered their suggestions when revising the text.
We welcome further comments if any.

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

Review by Veli-Matti Karhulahti, 24 Jan 2024 17:17. From now on "R#1".

Thank you for inviting me to review this interesting manuscript. For context, I'm somewhat familiar with HDA and find it as one of the most interesting theoretical alternatives in the current field. My topic expertise is on gaming disorder, but I've also worked on related social media questions. In this review, I focus on the study design and its underlying philosophy. I leave details of the statistical analyses to be vetted by statisticians (I have a few remarks but am not fully qualified to propose specific statistics solutions, which should be done by statisticians). I number my comments chronologically, not in order of importance but to make reading easier.

> **RE to R#1:** Dear Dr Karhulahti, thank you for your time in reviewing our manuscript. We appreciate the attention you dedicated to it. Please find below our replies to your thoughtful comments. We believe they helped us to further improve the discussion of some aspects of the study and thus its clarity overall.

R#1:1. I like the intro. It makes the state of the field clear and shows to the main problems that the design will tackle. There are some sentences and framings that are inaccurate, nonetheless. E.g., the first sentence "present study is an attempt to advance the validity of the diagnosis of behavioral addiction" is confusing: currently there is no diagnosis at all for PSMU so it's strange to advance the validity of something that doesn't exist (note that PSMU dominantly derives from *problematic*, not pathodological, social media use). I see what the authors want to say (improving related constructs) but it's important to say things like this correctly. Likewise, on p. 4, it reads the study "uses a related behavioral addiction, PSMU, in a test of validity" but again PSMU is not a behavioral addiction albeit being often studied as such (e.g. Billieux et al. 2015 DOI 10.1007/s40429-015-0054-y "the evidence supporting PMPU [sic] as an addictive behavior is scarce" and thus not included in DSM nor ICD). I would carefully review each sentence to ensure correct framing.

> **RE to R#1:** Thank you for your helpful suggestions. We have now checked and replaced the word *pathological* with *problematic*, except when applying the HDA, that *per* definition refers to disorders. We agree that the study of PSMU is more controversial, PSMU is not classified as a behavioral addition in any major diagnostic manual and of course, is not an instance of GD. Therefore, we clarified the study's logic in the introduction.
> In the new version, the sentence was changed to "The present study is an attempt to advance the debate on the validity of the diagnosis of gaming disorder and other specified disorders due to addictive behaviours …" because findings from recent studies support the view of PSMU as potentially addictive (please refer to the paragraph added to the introduction). We believe that it is still not as widely recognized as an addictive disorder because of concerns about confusing intensive use with addiction, and yet a large literature does consider it an as-yet-unmanualized disorder. Our study would thus constitute a relevant improvement to the current discussion clarifying the differentiation. Accordingly, we improved the description of this study objective.

There is also a consensus about the precise target, i.e., social media, rather than smartphone use or screen time. Accordingly, smartphones are the medium and not the *object* of addiction. This was discussed by Griffiths when differentiating addiction "to" the internet and "on" the internet (https://doi.org/10.3109/16066350009005587). Similarly, Billieux underscored the importance of specific activities for which smartphones are used (https://doi.org/10.2174/157340012803520522). However, the debate is still ongoing because, like for the category of substance and drug use disorders, researchers and clinicians have been suggesting the usefulness of considering "internet-related disorders" as a potential diagnostic category (see for example https://doi.org/10.1556/2006.7.2018.140 and https://doi.org/10.1016/j.addbeh.2022.107451).

R#1: 2. Related to the above, as a larger structural issue, I see it problematic that even though the intro is informative, it focuses narrowly on the BA discourse and mostly on gaming; where the study itself then turns out to operate with PSMU data. There has been a lot of debate on the construct differences/issues regarding use of "social media", "smartphones", "social networking", "screen time" etc. and what the related dynamics of (problem) behavior are (e.g., Mannell https://doi.org/10.1177/2050157918772864 mechanisms of disconnective affordances, Conroy et al. http://dx.doi.org/10.1037/ppm0000425 on smartphone overreliance, Vainio et al. https://doi.org/10.1037/ppm0000508 on perceptions on health changes etc.etc.). In brief, the PSMU literature is rich and not currently well represented by the limited BA lens, which mostly applies to gaming disorder that has a formal diagnostic status (adding two reviews here which are by no means exhaustive).

Bayer,J.B.,Triệu,P.,&Ellison,N.B.(2020).Social media elements, ecologies, and effects. Annual Review of Psychology, 71(1), 471–497. https:// doi.org/10.1146/annurev-psych-010419-050944

Orben, A. (2020). Teenagers, screens and social media: a narrative review of reviews and key studies. Social psychiatry and psychiatric epidemiology, 55(4), 407-414. DOI 10.1007/s00127-019-01825-4

RE to R#1: Thank you for this valuable comment and scientific references. In line with the suggestions, we expanded the introduction to better describe one of the study's main topics, i.e., social media use and PSMU. With the new paragraph included in the introduction, we clarified that we focus on only one of the potential uses of and conditions related to social media and networking, that is, its addictive potential, and not on the broader concepts of screen time and smartphone use. In support of the adopted psychopathological perspective, we included references to recent research discussing the addictive potential of social media. We especially appreciated some of the suggested references that were incorporated because they were beneficial for our discussion. Others, despite being valuable contributions, were not included because focused on smartphone (problematic) use in general and not on social media specifically.

R#1: 3. Following from the above, I am concerned about how HDA matches PSMU. Indeed, because PSMU is not an addiction but (by definition) a spectrum of problematic social media use patterns, it naturally involves diverse types of problems. Example: on p. 7 it is stated: “harmful consequences in the absence of a dysfunction do not qualify as a disorder. For example, obesity or postural problems may be consequences of inactivity or sedentary behaviors due to high amount of time spent gaming/using social media in absence of a dysfunction.” This could be seen as a straw man when applied to PSMU because PSMU is *not* a disorder, as noted earlier. The fact that PSMU may involve obesity or postural problems is not inconsistent with the idea of PSMU (as a *non-disorder*). Only if the addiction/disorder framing is applied to social media, the debate becomes relevant. Therefore, I would encourage the authors to carefully revisit the underlying philosophy on the constructs of the study, which currently seems to stand on the auxiliary hypothesis that PSMU is addiction/disorder and there is a need for a better disorder framing.

> **RE to R#1:** Thank you for your careful review of our manuscript. We agree that PSMU may refer to a spectrum of problematic social media use patterns with some of them being problematic/harmful whereas others being problematic/harmful non-disorders. We added this aspect in the new paragraph about social media where we discussed its addictive potential and scholars’ perspectives on this. Given this view, PSMU is a particularly good domain in which to explore whether an HDA approach can usefully clarify disordered and non-disordered variants.

R#1: 4. Moving to the next item on my list, I strongly recommend removing the hypothesis and doing this as a non-confirmatory study. Testing a hypotheses by the strict PCI RR guidelines would require major revisions and updates on the currently brief formulation, including assessment of practically meaningful SESOIs, null testing, effect size justifications, etc. Because HDA is being tested, the design should also be crafted in such way that clear criteria for falsifying HDA are outlined, preferably by equivalence testing (see author guidelines). In the current framework, to be honest, I do not see hypothesis testing feasible (also, as the data are already available, confidence level will be 1 anyway so the benefit of testing confirmatory hypotheses is very minor). If the authors really wish to do this, I’m ready re-review the improved hypotheses for the next version.

> **RE to R#1:** Thank you for this wise suggestion. Despite we can confirm that the dataset and variables of interest have not yet been explored, we understand the Reviewer’s perspective. We thus reframed the new version of our study as exploratory, i.e., an exploratory study of the HDA's usefulness in separating things in a way that reflects disorder judgments. This was done under the PCI-RR guidelines: “*The inclusion of hypotheses is not required – a Stage 1 RR can instead propose estimation or measurement of phenomena without expecting a specific observation or relationship between variables.*” (under “3.1 Stage 1 and Stage 2 criteria”).

R#1: 5. A few notes on methods. Regarding the heterogenous composite index, I don't see good evidence/reasoning why it would produce (more) informative results. The authors themselves too address the issue (p. 11) but nonetheless decide to do it. What is the (good) reason for not modeling all these variables separately?

> **<u>RE to R#1</u>:** Thank you for this comment. We decided to include the summary variables because although the HBSC included many measures of well-being, none of them are pathognomonic for disorder or non-disorder (mentioned under Methods → Measures → Dependent variables). Therefore, we will use the summary variables strategy to provide a "rough sense of global outcome". We believe that it could produce more informative results about general distress.

R#1: 6. Finally, I have a bigger theoretical note. I am saying this because I'm actually a big fan of HDA and would love to see it carefully used in this field. I personally believe it can help explain some of the massive confusion in the current BA literature. In particular, a highly productive observation (which Wakefield made already in the influential 1992 paper) is that "whether a condition is a disorder is not determined by how the diagnosed individual subjectively happens to feel about the condition's effects, but by more 'objective' standards determined by the culture's value system" (cited in the MS p. 7). This is highly important especially for BAs (like gaming) which carry different stigma's in different cultures, and the lost time/productivity is measured against different cultural norms of what kids/adults "should be doing instead". For BAs, this culturally relative definition of harm has already been studied in-depth by many researchers and studies, e.g. Trent Bax's extensive work on internet addiction in China (see e.g. the monograph) and Jeffrey Snodgrass team's numerous papers on "cultural dissonance" with internet gaming/disoder (for a recent excellent investigation of culturally generated 'harm' in India, see https://doi.org/10.1086/717769).

--> in this theoretical context where a disorder is experienced through culture and "not determined by how the diagnosed individual subjectively happens to feel", how does the current study tackle the paradox that its own harm inference derives from dichotomous self-report data (Table 1)? I continue on this below.

> **<u>RE to R#1</u>:** Thank you for this precious and thoughtful comment. We agree on the importance of considering environmental conditions for the emergence and persistence of GD and other potential behavioral addictions. We also agree that many phenomena of excessive/problematic use may be better contextualized as a reaction to stress. This discourse might efficaciously be applied to the analyses of all mental disorders. A variable degree of cultural influences may be invariably present as well as social and family-related characteristics. Therefore, we further highlighted this aspect in the new version of the text before the description of the study objectives. However, this does not provide support for all cases being a cultural idiom of distress or symptoms mimicking/representing an adjustment disorder. Similarly to the HDA, previous analyses of addictive behaviors (Fillmore, 2003; Kahler et al., 1995; Leeman et al., 2012, 2014;

Sripada, 2022) underscored the importance of impaired control as one essential aspect of the disorder. Regarding potential behavioral addictions specifically, we believe that the conceptualization of potential dysfunctions requires additional research efforts. For this reason, we included a study limitations section (mentioned also in the abstract of the new version) and will deepen this argument in the discussion section of the manuscript. Under this, we also included the use of data collected via self-reports.

For the present analysis, we benefit from self-report data, but harm is measured using two items about conflicts and arguments with others and one indicating neglecting other activities like hobbies and sports. Despite being less than ideal, this is somehow in line with "more 'objective' standards determined by the culture's value system", i.e., the actual items we use are arguably indicative in the sampled culture of objective harm. We believe that it needs to be considered that our study is an effort to improve the conceptualization using already available data. Future tailored research should explore the usefulness of other measures possibly taking into account the role of familial/cultural values.

R#1: 7. I see it as a meta-problem for this study that despite it being nicely designed to combat the unproductive "confirmatory appraoch" (confirming 6 criteria), instead of exploring the distinct or unique links of dysfunction and harm related to PSMU, it will carry out "another confirmatory appraoch" (confirming 2 criteria) by testing for HDA1/HDA2 via predefined items (Table 1). The logic is basically similar with the component model; the "components" are just different. To take a non-confirmatory approach (via the HDA framework) one would optimally explore the types and forms of problems that manifest in relation to social media use and see how they map out in HDA. E.g., a couple of years ago (https://www.nature.com/articles/s41599-023-01775-y) we asked gaming treatment-seekers about the types of problems they have, some of which matched ICD criteria but many did not. Only 42% met DSM criteria, yet there were no differences in types of problems between DSM-meeting and non-meeting ones (and all were in self-sought treatment). My impresssion is that the current HBSC dataset is not very suitable for such non-confirmatory approach if the only available BA data are 7 predefined self-report items that derive from the component model. To be clear, I fully support the idea of exploring the relevance of different problem-items, which has previously yielded informative results (e.g., Colder Carras & Kardefelt-Winther https://doi.org/10.1007/s00787-018-1108-1 , Ballou & Zendle https://doi.org/10.1016/j.chb.2021.107140). I believe the currently planned study can produce likewise interesting results and reflect them usefully against the theoretical HDA framework, but one must carefully design the exploration to not to frame results as confirmatory (unless the hypothesis structure is completely rebuilt; that would then need to be reassessed).

RE to R#1: Dear R#1, we appreciate your attention and helpful comments. According to this and a previous comment (n. 4), the new version of the study was framed as an exploratory analysis of the HDA usefulness. We understand the concern regarding the risk of proposing a new confirmatory approach based on the presence of dysfunction and harm. For this reason, as mentioned in replying to comment n. 6, we believe that the conceptualization of potential

dysfunctions requires additional research efforts. Accordingly, we included a “study limitations” section and will deepen this argument in the discussion section of the manuscript.
In doing so, we considered all the recommended references. One was included in the text (https://www.nature.com/articles/s41599-023-01775-y) because fits well the HDA approach. On the contrary, we did not include the other two - of which we were aware - because one, despite being an outstanding contribution (https://www.sciencedirect.com/science/article/pii/S0747563221004635?via%3Dihub), focused on distress/harm associated with GD rather than harm as one of its criterion (as we do in our manuscript), and then, we preferred to exclude it to avoid confusion. Regarding the second excluded reference (https://link.springer.com/article/10.1007/s00787-018-1108-1), some concerns are related to the choice of the optimal number of profiles. Indeed, it is not clear from Table 3 of the original article and the authors' discussion, why a 5-class solution was preferred over the 4-class solution (entropy did not change). However, in line with the HDA, the GD class showed both symptoms indicating dysfunction and harm.

R#1: 8. As a sidenote, although I avoid commenting on the statistics, I didn’t notice a supplement for the code (assuming open software like R is used) or other description of the specific tools in the analysis. This should be included as per PCI RR guidelines.

RE to R#1: Thank you for this comment. We have not added a supplement for the code because according to the information included in the PCI-RR guide for authors webpage “authors are required to make […] computer code publicly available (at Stage 2 submission)” (under “2.11 Data and materials transparency”).

R#1: Overall, this will be an interesting study and has the potential to produce informative results especially by exploring PSMU data in the HDA context. My major concerns are a) the mixing of addiction/disorder and problems on the construct level, b) need to remove or completely restructure the confirmatory element, and c) build a stronger bridge between theory and data/methodology to make a convincing exploration on which we can keep constructing robust (also confirmatory) studies later. Naturally, the recommender will assess to what degree these observations align with their/other views.

I always sign all of my reviews so I can be personally contacted in case my feedback feels unclear or unfair. I also add a default statement: some studies I have mentioned include me as an author; it is up the authors to assess whether they are worth citing and in case I will be re-reviewing this MS in the future, any citation or a lack thereof will not affect my assessment in any way.
Veli-Matti Karhulahti

RE to R#1: Dear R#1, thank you for the precious time and attention you dedicated to reviewing our manuscript. We believe the clarity of the manuscript benefited from your suggestions. As detailed in the above replies, we a) clarified the conceptualization of PSMU as a potential behavioral addiction based on recent findings, b) framed the study as exploratory, and c) improved the discussion of the link between theory and dataset used, considering the dataset/study limitations and the inclusion of recommendations for future studies applying the HDA to the concept of behavioral addiction.

We welcome further comments on the new version of our stage 1 RR.

Sincerely,

The Authors

Review by Gemma Lucy Smart, 20 Mar 2024 07:53. From now on "R#2".
Thank you for the opportunity to review this report. I have some suggestions to improve the overall rigor which I'll list below with their line numbers for reference.

> **RE to R#2:** Dear Dr Smart, thank you for your time in revising the manuscript and for your valuable comments to which we reply below point by point.

R#2: Overall I'm a bit sceptical about the use of PSMU as a surrogate for IGD. They are very different uses of technology, and different types of activities. You're going to have to do some heavy conceptual lifting to make any claims that they are the same that anyone in Game Studies for instance would accept. I encourage you to look at the literature on types of Play in Games and tasks in Games critically examine whether the repeated tasks you are looking at are actually the same or similar enough to those of gamers.

> **RE to R#2:** Thank you for this comment. We added a new paragraph describing social media use and PSMU as a potential behavioral addiction (different from GD) according to recent evidence and scholars' opinions. We believe that this clarified that we start with the general challenge to validity of BA and GD criteria and in this paper will exploit a PMSU study to test out the plausibility of an HDA approach. Such an approach should then be recalibrated to GD or other categories and this will be further clarified in the discussion of the study. We are thus not assuming that PSMU is a form of GD but merely exploring in a convenient sample the feasibility of an HDA approach that might then be brought back into other areas of behavioral addiction.

R#2: One of the conceptual issues I have with IGD is that it lumps gamers into a homogenous category when they are doing heterogenous tasks. As you note, the potential for proliferation of behavioural addictions is something to be concerned about. My suggestion here is that we have that potential *within a category* because you're actually looking at people doing different things. It's a fundamental lack of understanding of gaming.

> **RE to R#2:** We agree with the general concern about the presence of some degree of within-category heterogeneity in BA. Different articles have applied person-oriented analyses in an effort to examine the implications of such heterogeneity. However, here we focus on criteria importance and, as such, we do not focus on heterogeneity. It is worth considering that not just behavioral addictions but many diagnostic categories (e.g., major depression, schizophrenia) are generally considered to be heterogeneous with respect to construct validity and to encompass multiple etiological pathways to symptoms, and so until further research distinguishes distinct pathogenic etiologies there is a future challenge of refining categories.

R#2: It may be that you can find some correlates here, and if so great. I think the HDA is a good model to apply to the concept of IGD, but the literature from Game Studies is so routinely ignored in this space that I encourage you to engage with it to improve the conceptual rigor of your surrogate here.

RE to R#2: We appreciate the suggestion, but could not identify a Game Studies literature that could inform a revision of our criteria in the way suggested. A more specific recommendation would be appreciated and would be followed up in a final revision. Despite this, we made substantial changes improving conceptual clarity.

R#2: 189 While I agree as an overall claim about the addition category in the DSM, I suggest the authors revise this section to avoid rhetoric. There are obvious limits to the theoretical framing, even within current DSM approach. Without such limits any *substance* would be a target of addictive pathology, including absurd candidates like water. Where authors have framed the debate as 'any type of behaviour will potentially on the table as a behavioural addiction' that's simply not true. We can have a more nuanced discussion than that. Proliferation is a conceptual issue, yes. But it's not so out of control that *anything* can be framed as addictive.

RE to R#2: Thank you for this helpful suggestion. We agree that this is not something out of control, but a concern raised by some scholars. We modified the sentence to convey a less pronounced proliferation issue, as recommended.

R#2: 224 See Murphy & Smart (2018) for an overview of mechanistic models as theoretical approaches to the problem - I think they it would fit well enough with the HDA approach, mechanistic models, especially the work of Ross e al. (2008) agree with your view here, obviously moreso in the 'dysfunction' part of the equation'.

RE to R#2: We referred to one of the valuable recommended contributions when discussing our approach.

R#2: 283 Addiction does pose a problem here as it's always been framed in such a way that allows for external actors to determine or identify that harm in a way that other disorders may or may not (I.e. the old fashioned criteria about addiction affecting marriage). This does mean that you may have to consider 'harm to others' not just 'harm identified by or harm to' the individual gamer. It depends how you conceptually frame harm in this context.

RE to R#2: We appreciate this valuable comment. We rely on self-reported data that includes "conflicts with others" and "neglecting other activities" among potential harm, with the former being on the borderline between personal and social harm. At the same time, we discussed this among the dataset/study limitations as well as the need for future research uncovering harm due to PSMU. Finally, it is worth mentioning that harm to others remains an area of scholarly dispute about the HDA and future further work on this is crucial.

R#2: 319 Interesting data set. The average age of gamers changes yearly, but it's around 30-35 years old. May be worth keeping in mind, especially as social media use would no doubt be quite different in that age group?

RE to R#2: Thank you for noticing this relevant aspect. We have now added the discussion of participants' age to the study limitations.
Social media/network use is common among young people. According to Statista and We Are Social, around 5 billion people worldwide use social networks as of April 2024. The most used social networks are Facebook (3 billion), Youtube (2.5 billion), Instagram, and Whatsapp (2 billion each). The age group 16-24 years uses a higher mean number (7.5) of social media platforms compared to other age groups. Instagram is used by 25% of people aged 16-24 years who represent the larger percentage of Instagram users overall (32%). Further, we know that 54% of European children aged 9-16 years accessed social media daily and 12% once a week according to EU Kids Online (https://www.lse.ac.uk/media-and-communications/research/research-projects/eu-kids-online/eu-kids-online-2020). The percentage increases to 77% when only adolescents aged 15-16 were considered. It follows that studying PSMU among young people is justified. However, as we added to the limitations section in the new version, adolescents show a greater propensity towards impulsive and risky behavior and are more attracted to novel stimuli. This needs to be considered when studying addictive behaviors in this developmental phase.