Author's response to reviews

Title: Problematic computer gaming, console-gaming, and internet use among adolescents: new measurement tool and association with time use

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Author's response to reviews:

Reviewer 1

Comment 1a: Could the authors explain the distinction between computer versus console gaming, as a clinical concept? This distinction seems somewhat unnecessary. However, if required, why not portable gaming or smartphone gaming also?

Response: We have rewritten the section about measurement in order to clarify the distinction between computer and console gaming. We agree with the reviewer that the distinction is unclear (which we also mention in the new text) but it appeared during our pilot interviews with adolescents that they thought this distinction was important.

Unfortunately, the questionnaire did not include items about portable gaming or smartphone gaming and we have explained this issue in the revised text.

Comment 1b: The argument that there is a need for a “short” measure of problem gaming is lacking in support, given that the majority of available measures have fewer than 20 items. I direct the authors to a recent review of 18 instruments summarising the current state of the art. King, D. L., Haagsma, M. C., Delfabbro, P. H., Gradisar, M., & Griffiths, M. D. (2013). Toward a consensus definition of pathological video-gaming: A systematic review of psychometric assessment tools. Clinical Psychology Review, 33, 331-342.

Response: Thank you for directing our attention to the paper by King et al. 2013. We have learned from this paper and rewritten the justification for building a short measure. In the revised text, we have explained that our measure was not intended for clinical use, rather for school surveys about adolescents’ daily life. We have also explained why we think there is a need for a very short measure for use in public health research. In order to clarify this issue we have also
changed the title of the manuscript.

Comment 2a: This paper may offer a guide for further indicators of psychometric quality for a paper on proposing a new measure of problem gaming. This is especially important given that there are already many measures in the literature, and a new instrument should clearly indicate its distinct advantages over the current ones. Also I think this paper would be strengthened by referring to the proposed clinical diagnosis of Internet Use Disorder or Gaming Disorder (see King, D. L., & Delfabbro, P. H. (2013). Issues for DSM-5: Video-gaming disorder? Australian and New Zealand Journal of Psychiatry, 47, 20-22) or a conceptualisation by Porter et al. (2010) or Tao in his Addiction paper.

Response: Thank you for your kind remark about indicators of psychometric quality. We have also enjoyed the papers by King et al. 2013 and by Porter et al. 2010 and used these papers in the rewritten manuscript. In this way, we have been able to strengthen our explanation about why we feel there is a need for a short measurement for use in public health studies as a supplement to the many good measures for clinical research.

Comment 2b: The conceptualisation presented in this paper does not appear to map onto any identifiable clinical disorder, which makes it problematic for use in clinical research.

Response: In the rewritten manuscript, we have been explicit about this issue. Our new measure does not map onto any identifiable clinical disorder and the measure is therefore not useful in clinical research. We have also explained the need for a different approach in studies of everyday behaviour in population-based studies of adolescents.

Comment 3: The research aim – examine the relationship between time use and these perceived problems – is not novel, and has been examined across dozens of studies. I would suggest that a more original research aim be identified.

Response: Thank you for this useful comment. In the revised version, we have attempted to communicate what we really wanted to analyse regarding time use.

Comment 4a: The authors should consult work by Douglas Gentile (particularly, his 2011 longitudinal study) which highlights how time spent gaming and pathological use are not linearly related – therefore, association with time spent is an imperfect measure of criterion validity. Additionally, work by Charlton and Danforth (2007) has shown that healthy obsession with video games often produces high levels of gaming without indicating pathology. In this way, time spent gaming as a validity marker, again, should be regarded with caution.

Response: This was very useful. We have studied the papers by Gentile and the paper by Charlton & Danforth 2007 and included these papers in the rewritten manuscript. In this way, we have been able to improve the justification for the study. We have also tried to explain that we do not consider correlation with time use as a validation issue but rather a research question in its own right.
Comment 4b: I am not 100% sure how this “non-pathological” measure is useful for clinicians (and I confess a degree of confusion given that the measure refers to “self-reported problems”, which sounds to me as though it would fall along a continuum indicating pathology). I think the authors could explain the advantages of this measure in greater detail.

Response: We are fully aware that the new measure is insufficient for clinicians. We have explained this issue in the revised manuscript and we also think that this thoughtful comment has helped us to build a stronger justification for the paper. In the discussion, we are now explicit about the applicability of this new measure (studies of everyday life of adolescents) and also mention that we regard the measure irrelevant for clinical research.

Comment 5: Unfortunately, I have not learnt anything significantly new from this work, however I think the data are valuable due their rigour and quality of measures. I think the authors should include more information about the measure itself (and its psychometric qualities) rather than focussing on broad observations such as prevalence and gender differences, which is less interesting. Consulting a paper published in Psychological Assessment may be helpful in identifying further material for inclusion and/or further analysis.

Response: Again, this was a useful comment which helped us reformulating the justification for the study.

Comment 6: The data are the strong point of this study. Large sample and excellent response rate. I was particularly interested in the papers’ attention to family structure and I think this could form the basis of another, new paper on a different topic related to gaming.

Response: Thank you for your kind remarks about our study. We have included your suggestions for further studies in the rewritten discussion section where we mention some implications for further research.

Comment 7: I think the authors do not assess validity in sufficient detail to draw strong conclusions. Given the measure is not related to any diagnosis, it is difficult to state whether the tests are measuring a particular concept.

Response: We have dealt with this issue, that the measure is not related to any diagnosis, in some detail in the rewritten manuscript. In our view, the new measure is useful for the description of everyday behaviour in adolescence and we have presented this view in the rewritten manuscript.

Comment 8 (Have the authors provided references wherever necessary?): Yes, they refer to many relevant studies in this area. Although, see my comment re: Gentile’s work.

Response: Again, thank you for your useful suggestions about supplementary
references. As you can see in the rewritten manuscript, we have adopted your suggestions.

Comment 9 (appropriateness of methods): Yes. However, the authors should explain in greater detail how they developed the gaming measures. It is great that a pilot study was conducted, but how were the measures themselves created?

Response: In the rewritten manuscript, we have provided a more careful description about the development of the measures.

Comment 11: I think this paper would benefit from a factor analysis to assess dimensionality of the instrument(s).

Response: We have conducted a factor analysis and reported the findings in the rewritten manuscript.

Comment 12 (recommended improvements): Please see my earlier comments re: theoretical model/concepts to give the study grounding, and the need for greater assessment of validity. The study may not have sufficient measures to assess validity. However, as highlighted above, I think the authors have an impressive dataset to evaluate family structure and its relationship to gaming.

Response: Thank you for your recommendations. We think that we have complied with all your suggestions.

Comment 13: Paragraph structure is a bit problematic. Avoid short paragraphs with underdeveloped ideas.

Response: We have revised the entire text and improved the paragraph structure.

Comment 14: The study should adhere to ethical standards of scientific/medical research and the authors should declare that they have received ethics approval and or patient consent for the study, where appropriate.

Response: We have explained in the rewritten manuscript that there is no formal agency for approval of this kind on anonymous surveys in our country. Further, we have explained how we dealt with issues of informed consent, anonymity, voluntary participation, and data protection.

Comment 16: An article whose findings are important to those with closely related research interests

Response: Thank you for this assessment. We think the rewritten manuscript could be useful for colleagues who study everyday life and behaviour among adolescents.

Comment 17: Needs some language corrections before being published
Response: The rewritten manuscript has been checked by a colleague whose mother tongue is English.

Reviewer 2:

Comment 1a: While I strongly agree with the authors' opinion that new dimensional assessment tools which do not only focus on disorders do need to be developed in order to stimulate research in the field, I am sorry to say that the measurement tool suggested by the authors does not seem to fill the current gap. The authors did very well in conducting a pilot study via focus groups and consequently dividing their tool into the different groups (online gaming, console-gaming and other Internet use). However, the authors have missed to connect their research and the development of their research tool with the current discussion around the new DSM-5, in which Internet Gaming Disorder has been introduced in the section 3 (disorders that warrant further research before being accepted as official diagnosis) and ICD-12.

Response: This comment has helped us to clarify what this paper really is about: The development of a short instrument for measurement of everyday behaviour among adolescents. We did not intend to develop an instrument for clinical use or an instrument which linked with any particular disorder. We have explained this issue in the rewritten introduction. Further, we have changed the title of the manuscript to show this focus.

Comment 1b: Although the authors did not want to focus on illness only, they still should have considered the theoretical background of criteria for gambling and/or dependency which should be the basis for both categorial and dimensional approaches to the topic. The measure suggested by the authors contains a very limited mixture of criteria that lack a theoretical framework, which is the main limitation of the study as it remains unclear what the authors have really measured and it will lack international comparability.

Response: We have considered the theoretical background for criteria for gambling and/or dependency in the rewritten manuscript. We have also acknowledged that the new measure includes a limited number of criteria and that it is insufficient for use in clinical research. However, we still believe that there is a need for such an instrument in community based studies of everyday behaviour among adolescents. Your comments inspired us to a much more explicit explanation of what we really wanted with the new measure. We hope that the rewritten introduction and discussion is much clearer in this respect.

Comment 2: Additionally, taking time spent with gaming or other activities in the internet as validation criterion is questionable. Research on pathological computer and internet use has consistently revealed moderate correlations between problematic use and time spent with the respective activities. Therefore, time spent online was not included in any of the proposed criteria for potential diagnoses in this field. It would have been far more convincing to compare the new questionnaire with one of the established measurement tools.
Response: We acknowledge that time spent with gaming is a questionable validation criterion and this is now explicitly mentioned in the rewritten manuscript. Further, we have explained why we think it may be appropriate to study the association between screen-time and perceived problems, not as a validation issue but as an issue in its own right.

Comment 3: In their measurement tool, the authors ask to exclude homework from Internet activities. However, when they ask for the time spent in the Internet they explicitly include homework in the respective question. This seems quite inconsistent to me.

Response: As the reviewer mentions, there is an inconsistency in the measurement of time use. In the revised manuscript, we have explained the background for this inconsistency and also acknowledged in the discussion section that this is an information bias.

Comment 4: In general, the article is well-written with some need for language editing. As stated before, the introduction and discussion would benefit from some theoretical framework and some arguments for the author's choice of questions in their measurement tool.

Response: Thank you for these kind words. We have rewritten the introduction completely in order to build a more comprehensive justification for the development of a new measure. Further, we have edited the discussion so it better fits the focus of the manuscript.

Reviewer 3:

Comment 1. The Authors emphasized the need for demedicalization or depathologization of gaming and gaming addiction and their measures. However, the argument of the beneficial nature of computer gaming, console-gaming and internet use. I think important part of the issues including motivations, why people play these type of games. These aspects are missing from the introduction which my be understandable since the Authors did not measure the motives of gaming. Anyway, mentioning these approaches would be relevant in the introduction.

(Discretionary revision)

Response: Thank you for this comment. We agree that these issues were missing in the first version of the manuscript. Based on your comments, we have completely rewritten the introduction and provided a new justification for the manuscript.

Comment 2. The statistical analyses are acceptable and up to date, however I missed the divergent/convergent validity analysis of these three short measures. I mean it can be done multitrait-multimethod matrix or just simply the Authors should provide the correlational matrix of nine items, and also the correlations between the three scores, or using factor analysis of the nine items, which should provide three correlating factors. Minor Essential Revisions)
Response: Thank you for this proposal. We have conducted the recommended correlation analyses and provided information about the inter-correlation of the nine items in the revised manuscript and in a new table.