**Author's response to reviews**

**Title:** Massively Multiplayer Online RolePlaying Games: Comparing characteristics of addict vs non-addict online recruited gamers in a French adult population

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**Author's response to reviews:** see over
Dear Editor,

Thank you very much for reconsidering our manuscript entitled « Massively Multiplayer Online Role Playing Games: Comparing characteristics of addict vs non-addict online recruited gamers in a French adult population ». In reply to the new comments raised by the three reviewers, we have modified the manuscript (added elements highlighted in yellow and deleted elements crossed off). Please find below the responses and modifications (point by point) to the reviewers’ comments.

We clarified statistical analysis - particularly with respect to stating degrees of freedom and accounting for adjustments related to multiple comparisons.

Reviewer: Chih-Hung Ko

Dear reviewer, thank you very much for considering our manuscript entitled « Massively Multiplayer Online Roleplaying games: Comparing characteristics of addict vs non-addict online recruited gamers in a French adult population ». Thank you for your comments. Please find below the responses and modifications (point by point) to your comments.

Reviewer’s report:
The article had provide the important information for Internet addiction. Further, the author had developed a screen tool for internet addiction.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:
I declare that I have no competing interests
Reviewer: Matthew Brown

Dear reviewer, thank you very much for considering our manuscript entitled « Massively Multiplayer Online Roleplaying Games: Comparing characteristics of addict vs non-addict online recruited gamers in a French adult population ». Thank you for your comments. Please find below the responses and modifications (point by point) to your comments.

General comments:
The revised manuscript is an improvement over the original. It is now clear what raw questionnaire data the authors collected and how they classified participants based on that data. Unfortunately, it is my assessment that the statistical analysis is flawed. There seems to be confusion on the nature and importance of multiple comparisons and correcting for them. I discuss this in one of my comments below. For this reason, I am not convinced by the manuscript in its current form. I would like to note that the results presented in the tables do look promising, and they might in fact survive multiple comparisons correction. I encourage the authors to look into this issue in more detail. with kind regards
Matthew Brown

The statistical analysis was reconsidered particularly for multivariate statistical tests.

Based on the instructions from BioMed Central, my individual comments are labelled as:
(DR) Discretionary Revisions (which are recommendations for improvement but which the author can choose to ignore)
(MER) Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)
(MCR) Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached)

1. (MCR) Statistical tests are still not described adequately. For example, what are the details of the multivariate statistical model that was used to adjust for age, gender, and education level? For example, was this model applied to all of the various dependent variables (questionnaire items) that you measured simultaneously? Or was it applied to individual dependent variables with addiction risk positive/negative status, age, gender, and education level used as predictors?

Statistical analysis section was re-written, taking your comments into consideration. In fact, all variables were adjusted (step by step) by taking into account usual factors of confusion as gender, age and socio-economic characteristics (references: 13, 14, 16, 32, 33). There is a single explained dependant variable: DAS+; but not the items of the questionnaire. DAS considered all items with a cut-off score: DAS+/DAS-. For the multivariate analysis: all variables were tested one by one independently one of the others.

2. (MCR) The issue of multiple comparisons has to be addressed here. In your reply to my original comments, you state "DAS variable was the only one main criteria. Testing the difference between 3 or more groups implies to use a specific statistical strategy including first a “global” test, and after 2-2 tests, and a correction of the p-value threshold. Although numerous statistic tests were conducted, in our point of view, our analysis was not concerned by a multiple comparisons situation." Multiple comparisons are a problem whenever you are doing more than one test. If you do 20 separate T-tests with false positive rate controlled at $p < 0.05$ for the individual tests, across all 20 tests you are controlling the false positive rate at $p < 1$ (because $20 \times 0.05 = 1$). I.e., you are basically certain to observe a
false positive in one of the tests. Doing multiple tests between 3 or more groups is one way to create a multiple comparison situation, as you say. However, one can also do multiple comparisons when measuring multiple dependent variables from two different groups (addiction risk positive and negative groups), as you are doing here. Given that you are performing around two dozen univariate statistical tests, some kind of multiple comparison correction is necessary. One simple though severe correction is the Bonferroni correction, in which you simply multiply each univariate test's p-value by the number of tests you are performing. The univariate tests look promising. Many of the test statistic values (eg: chi-squared values) are quite high. These tests might well survive a Bonferroni correction, not to mention a less conservative multiple comparison correction.

We made adjustments related to multiple comparisons. Bonferroni correction is the most popular of the methodologies controlling the increase of false positive rate in Multiple Comparison Procedure (MCP). It aims to control the probability of committing any type I error in comparisons under simultaneous consideration (the family wise error rate or FWER). But this procedure is also the most conservative correction, drastically reducing the power of the analyses. So, we applied an alternative approach proposed by Benjamini and Hochberg [Benjamini et al.,1995], which is controlling the expected proportion of falsely rejected hypotheses, the False Discovery Rate (FDR).

This correction was applied to groups of simultaneous tests of null hypotheses. Analyses were considered as simultaneous when the independent variables were describing a characteristic of the same family, and were presented in the same tables. (MMORPG addiction's screening scales, baseline demographics, and social impairments…). Corrected p-values [Reiner, 2003] and corrected Confidence Intervals [Benjamini et al., 2005] were calculated in order to control FDR (tables have been revised).


3. (MCR) In addition, your univariate tests are not independent from each other. For example, the same individuals who reported having financial difficulties likely also reported marital problems as well as other problems. Therefore, significance or non-significance on one test will tend to be associated with significance or non-significance in other tests.

We made adjustments related to univariate tests .

4. (DR) The manuscript was difficult to read. Details of statistics (p values, computed statistics values, degrees of freedom, which test was used) are not included in the text of the Results section, though some of these details are included in the Tables. This originally obscured the fact that several of the differences you stated to have observed in your data were not in fact significant. Only careful comparison of the Tables with the Results text revealed this. Having to flip back and forth between the Results text and the Tables was also distracting.

Your comment had been taken into consideration; details of statistics were included in the text. Unfortunately, we mentioned two confusing variables in the text that are not significant (age and gender, table 3).
5. (DR) There are several typographical errors throughout the text.

Your comment had been taken into consideration, and typographical errors have been corrected.

6. (MER) pdf p3 "The 453 participating adult gamers were young adult graduates living alone in urban areas.": The participants were living alone even though some of them reported marital and / or family difficulties?

The 453 participating adult gamers were mainly young adult graduates living alone in urban areas. The sentence was corrected in the text.

7. (MER) pdf p5 "This distinction is sustained by recent neurobiological findings on the different neuronal process involved in dependence or addiction.": Provide reference(s) to support this statement.


8. (MER) pdf p13 Statistical Analysis: Much necessary information is missing here. For example, you mention that some specific univariate tests were used in "Univariate analysis was performed using the two-sample t-test (continuous variables), Pearson’s chi-square test (unmatched categorical variables)." But, you do not specify which data these tests were used on, which comparisons were being made, and so on. I am having to flip back and forth among the Methods, Results, and Tables to try to figure out what comparisons you made. Most readers will not be as patient, which will reduce your paper's impact. I have similar concerns for the other tests you mention here.

Statistical analysis section was re-written, taking your comments into consideration.

9. (DR) pdf p14-16 Results: P-values and other statistical test details are not in the text, though you do reference Tables with this information. You do not even state that a given observation is statistically significant (or not significant). This forces me to break my train of reading to go look up a table just to be sure that a stated difference is actually significant and at what level. This makes your results harder to read.

The results section has been re-written, taking your comments into consideration.

10. (DR) pdf p15 "DAS was statistically associated with GIAD and ISS (all p<10-3)": I originally thought you had done some kind of multivariate test to show an overall statistical association, but later I figured out that you were merely introducing the univariate tests which you discuss next. You might want to clarify this for your readers.

DAS was statistically associated on one hand with GIAD and on the other hand with ISS (respectively p<10-3 and p<10-3).

11. (MER) pdf p15 "77.5% (84+263 on 448) of concordant responses were found for the 355 DAS and ISS scales." What do the (84+263) numbers mean? What does "concordant responses" mean? Does this mean that 77.5% of participants were classified the same with respect to addiction in the DAS and ISS scales (i.e. 77.5% of participants were classified either as not addicted on both the DAS and ISS or as addicted on both the DAS and ISS)?
This section has been rephrased to improve reader’s understanding as you suggested.

12. (DR) pdf p15 "Similar results were observed regardless of the scales studied (See appendix tables for results obtained with ISS and GIAD scales).” This sentence is imprecise, and I’m not sure of your point here. Results were similar in what way?

The sentence was re-written following your recommendation.

13. (MER) pdf p15-p16 (line 372-375): In the text, you fail to mention that the differences between the DAS+ and DAS- groups in terms of age and gender composition were not significant. This is only evident through careful cross-referencing with Table 3. You need to include details of p values, computed statistics, statistics tests, and degrees of freedom in the results text. Even if this means presenting that information twice (in the text and in the tables), this is acceptable as the emphasis should be on clarity and ease of reading.

*We agree with your comment. The results section has been re-written to provide more details.*

14. (MER) pdf p17 line 414: There is a crucial word missing in this sentence: "GREATER in-game sense of power".

*Your comment had been taken in consideration and we rephrased this part of the manuscript.*

15. (MER) Tables: The "Crude monovariate statistical test" phrase is unclear. This term is not used in the Methods section. I assume this refers to the chi-squared and t-tests. Using inconsistent terminology places an unnecessary extra burden on the reader.

We have clarified the tables’ content.

16. (MER) Tables: What is the "multivariate adjusted #“?

We have clarified the table 7 content: multivariate analysis; # = odd ratio and p value were adjusted for age, sex and educational level.

17. (MER) Tables: I do not see degrees of freedom stated anywhere for the tests described.

*Degrees of freedom were added, taking your comments into consideration.*

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:
I declare that I have no competing interests.
Reviewer: Dave Hayes

Dear reviewer, thank you very much for considering our manuscript entitled “Massively Multiplayer Online RolePlaying Games: Comparing characteristics of addict vs non-addict online recruited gamers in a French adult population”. Thank you for your comments. Please find below the responses and modifications (point by point) to your comments.

Reviewer’s report:
While the authors have sufficiently answered most of my questions, there are still a number of related concerns. My main concern is that it seems that the results are still overstated, or are unclear, in many instances. In addition, I have the following comments/questions:

- Line 111-12: Are tolerance, withdrawal etc really ‘disorders’?
The word “disorder” has been replaced in the text by “symptoms” that is effectively much pertinent.

- I’m still skeptical about the use of ‘addiction’ or ‘dependence’ for gaming behaviour, though I recognize that there are many shared facets. Despite the additional text in the introduction, I must admit I’m not entirely clear on precisely how the authors are using these terms – though it seems that they are reserving ‘dependence’ for physical dependence and addiction for the broader behavioural aspects. If so, this would be fine for the present study, however, it should be emphasized that a clear characterization of abhorrent gaming behaviour (and it’s connection, if any, to addiction) is still under review and is thus debatable.

We thank you for your appreciation on the clarification on the use of both concepts “dependence” and “addiction”. In this study we followed the recent tendency to use the term “dependence” for “physical dependence” and the term “addiction” for “loss of control, automatism, and the persistence of the behaviour despite the adverse consequences” (Balland and Lüscher, 2009).
We completely agree with you that there is no clear characterization yet of abhorrent gaming behaviour, even if scientific community refers to it in a growing number of publications as a part of behavioural addictions (Grant et al. 2010). Its acceptance as a disorder and then as an addictive disorder is effectively yet to come (APA, 2010).


- Line 539: I suspect that the higher rates for IA reported here (compared to other literature) may be due to Type I errors related to the many limitations of this study – again, I think it would benefit to point this out.
We agree with you in this point.

- The manuscript still needs to be corrected for appropriate English grammar and minor errors (e.g. lines 277-280: “How does playing make you feel?” and grated should be greater and fell should be felt). There are many errors such as these, and they often prevent the reader from having a clear understanding of what the authors mean.

We apologize for the English grammar and the typographical errors, the whole article has been corrected.

- Level of interest: An article whose findings are important to those with closely related research interests

- Quality of written English: Needs some language corrections before being published

Language corrections have been made.

- Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:
'I declare that I have no competing interests'