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 considering 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 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.

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.

This is a very interesting study pay attention to MMORG. It had provide many interested results. However, there are several major compulsory revisions should be clarify.

1. The validity of the diagnostic tools were not tested according to any diagnosis based on directed interviewing. Further, how to determine the cut off point of the three diagnosis should be discussed. The low cut/off point may result in higher rate of addiction in this study.

The cutoff point of the screening scales for Internet Addiction were those defined by their authors. We did not allow ourselves to change the cut off points to keep the same assessment level as previous studies. For DAS criteria, we respected the same cut off that the DSM IV TR for substance dependence from which it was adapted. The lack of validated diagnostic tool for both video game addiction and Internet addiction when we designed our study (2008 summer) was problematic but also an opportunity to test existing even not validated tools in a self-reported design. Validating those tools with directed interviewing is a further objective. In our article we preferred talking about screening than diagnostic criteria.

The rate of gaming addiction in our study is higher than other studies (i.e. 15% with the ProblemVideogamePlaying (PVP) (Tejeiro et al., 2002); 12% with CIM 10 criteria for dependence (Grusser et al. 2007), and an average of 40% responders considered themselves addicted (Smahel et al., 2008) in other research studies). These differences could be explained by:

- the targeted population of adult online gamers(and not general population or student sample) recruited in-game (guild forums);
- the focus on the most popular MMORPG with more and more attractive add-ons that may have more addictive effects;
- and by the anonymity of data that may encourage honest responses.

For Internet addiction we found higher rates than literature. The higher rates for Internet addiction in our study could be explained by the sample characteristics: online gamers are more likely to overuse Internet vector for gaming but also for other Internet activities. For ISS and for GIAD (respectively 32.5% and 44.3% of dependence), the higher rate of GIAD could be explained by the fact that these tools both didn't measure the same dimensions. In fact ISS focused on addiction with the
control loss, and the persistence of the behavior despite adverse consequences on important life fields; whereas GIAD evaluated in addition tolerance and withdrawal symptoms, matching with dependence.

In spite of a high rate of addiction observed in our study, we observed similarities with other studies regarding age, sex and educational level (Peter et al., 2008; Smahel et al., 2008; Yee, 2009).

This information has been explained in more detail in the Methods and Discussion sections.


2. The diagnosis was just based on self reported yes/no response online. It is difficult to test the validity of response.

In fact we did not choose to interview participants as several studies have done because it was a further objective if the tools were considered as good candidates when tested in a large as representative as possible sample of the adult online gamers population. Nevertheless, our online questionnaire took 45 minutes to complete (a high amount of time for gamers). The first part presented here consisted of 63 questions and was composed of 3 different addiction screening instruments consisting of different and special items. And we observed that whatever the scale used, the same trend was observed (32.5% for ISS, 44.3% for GIAD and 27.5% for DAS). This and precautions that we used in our statistical treatment of incoherent responses are explained in more detail in the Methods (study design) and Discussion sections.

3. How to assess variables discussion in "distribution among DSM-IV TR dependence criteria" should be provided in methods. Further, their psychometric validity should be also provided. Lastly, the term of "DSM-IV TR dependence criteria" was not appropriate. There is no criteria for Internet addiction in DSM-IV.

The questionnaire including items from different scales has been added in an additional file. Regarding the term "DSM-IV-TR dependence criteria" We meant, but not clearly formulated in the previous manuscript “DSM-IV-TR substance dependence criteria” and not “DSM-IV-TR Internet addiction criteria”. In fact, this term was not appropriate because no criteria for Internet addiction was in DSM-IV. In our study, we observed with the adapted scale a potential risk for MMORPG addiction and not for Internet and we emphasized that the DAS was not a diagnosis scale but a first-line instrument for screening gamers who could be at-risk to gaming addiction. This has been explained in more detail in the discussion section.
Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable
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.

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General Comments

The manuscript lacks sufficient detail in the Methods and Results sections. The DAS questionnaire and 76 item questionnaire, both of which the authors devised, are not included anywhere. Details of and justifications for statistical tests are not given. I am concerned that statistical tests might not have been done properly, given that no mention was made of correction for multiple comparisons. In general, it would be impossible to replicate the authors' study from the information given. Many results are not described in sufficient detail for me to understand what is being measured. For example, Table 4 lists such things as "Less go out". I assume this was based on one or more questions in the 76 item questionnaire, but those details are not presented. Given the shortcomings with the descriptions of the methods and results, I am unable to evaluate the work in terms of its scientific merit.

The Methods and Results sections have been expanded and more detail added. The questionnaire including items from different scales was added in an additional file. Moreover, the statistical section was re-written according to your comments: the statistical tests have been justified and explained in the statistical analysis in Methods section.

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Specific Comments

Key: As per the instructions from Biomed Central, comments are labelled with one of the following:
* (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)

Note that I have labelled as (MCR) some things which might appear to the authors more appropriately labelled (DR). In these cases, I did not understand what the authors meant and that lack of understanding prevented me from evaluating the manuscript. Hence the label of (MCR).

Abstract

(DR) I found much of the abstract's wording difficult to understand, requiring multiple re-readings and testing different possible interpretations. You need to rewrite and clarify the abstract. This is crucially important for your work to have any impact, as the abstract is the first (and frequently only) part of the paper that others will read.
The abstract was re-written, taking your comments into consideration.

(DR) "The 453 participating adult gamers were typically young adult graduates living alone in urban areas." should be in Methods section of the abstract.

The sentence "The 453 participating adult gamers were typically young adult graduates living alone in urban areas" was the first result of our study so it was included in the Results section and not in Methods section.

(DR) "For all scales, cyberdependence were high (respectively 27.5%, 44.3% and 32.5%)." How was cyberdependence defined? (I later understood what you meant after reading in the Methods section how you derived classifications of dependence. I recommend rephrasing / adding something to clarify that the DAS, GIAD, and ISS scales each included a threshold over which a participant was classified as cyberdependent.)

In the abstract, the positivity in each scale was explained by a score above a specific threshold. In the manuscript, this clarification was added too in the Methods section.

(MCR) "The DAS appeared as a pertinent discriminating instrument as DAS dependent gamers had a 3 times increased tolerance phenomenon," What is a tolerance phenomenon in this context?

"The DAS appeared as a pertinent discriminating instrument as DAS dependent gamers had a 3 times increased tolerance phenomenon,. This sentence was not well formulated and were replaced by "The DAS appeared as a good first-line instrument to estimate gamers with MMORPG addiction probable problem." And "Compared to DAS negative group, DAS positive gamers reported higher rates of self-reported tolerance phenomenon (increased amount of time in online gaming to obtain the desired effect). In the MMORPG context, a tolerance phenomenon was characterized by an increased amount of time in online gaming to obtain the desired effect as can appear in the questionnaire added according to your comments in appendix. This clarification was in Discussion section.

(MER) "declared significantly more social, financial (OR:4.85), couple (OR:4.61), family (OR:4.69) or work difficulties (OR:4.42)" What does OR stand for?

We apologize for forgetting to report that OR was the Odd Ratio acronym. We have corrected this oversight.

(DR) "Furthermore, they were 3 times more irritable, having more daytime sleepiness and more sleep deprivation to play." How was irritability measured?

The sentence has been rephrased because of its confounding nature as "Furthermore, these gamers self-reported higher rates of irritability, daytime sleepiness, sleep deprivation to play, low mood, and emotional changes." Irritability was self-reported by responders and not measured objectively. Exact formulation of the question could be seen in the questionnaire in the appendix file.

(MCR) "The results highlight that the DAS did not overestimate proportion of online video gamers addicted and was associated with several behavioral disorders such as low mood, emotional changes, disturbance of sleep." The
arguments presented do not support this statement. How would you know if the DAS overmeasured or undermeasured the proportion in question, given the lack of an accepted “gold standard” for measuring this proportion?

We agree with your comment. In fact, no gold standard currently exists to estimate the proportion of online video gamers addicted. Here, we used 2 Internet addiction scales, GIAD and ISS, that we compared to a third scale that we have adapted for MMORPG. In this context, we have modified the manuscript: the term “positivity to a scale” has replaced the term “dependence to a scale”. In fact, DAS scale was a first-line instrument to screen for a probable at-risk population to online game addiction but was not a diagnosis scale.

Background

(MCR) What is an addiction? The authors do not actually define addiction, though they discuss many characteristics of and factors associated with addiction.

The background section was re-written taking in consideration your comments on the definition of addiction.

(DR) In general, I found the tone of the background a little overstated. Though online gaming creates problems for some individuals, including severe problems in some cases, many others play these games without adverse effects. Parts of the background section seem to imply that online gaming necessarily leads to whichever ill is being discussed, though perhaps this was not your intent?

The background section was re-written, taking your comments into consideration. In fact, online video game including MMORPG was an emerging phenomenon. Not all gamers would be dependent or have problems with excessive use. Only a minority of them is concerned by overuse or addictive use. Nevertheless, faced with this increasing phenomenon, it is firstly important to develop specific tools to screen gamers who could be at risk. Secondly, the ultimate goal was to better characterize this particular population at risk to be able to offer them adequate treatment.

(DR) p5 of pdf: “providing detailed descriptions of personal problems that arise due to playing MMORPG [13]” This sentence implies that these personal problem always ensue from playing MMORPGs. Many players of MMORPGs presumably do not have those problems, even if many do. This sentence should be rephrased to be more accurate.

We have specified in the background section that in the study presented by Hussain et al. (Hussain et al., 2009), with 71 online interviews, a third of them described personal problems that arise due to playing MMORPG.


(MCR) p6 of pdf: “the DSM-IV-TR criteria for substance dependence [21, 22] which we adapted for online video gaming (replacing the substance with online video games, and specifying withdrawal symptoms as agitation or irritability)” Why were agitation and irritability chosen as withdrawal symptoms? Were other withdrawal symptoms. A later part of the manuscript suggests that sadness might be a good candidate.
Were other possible symptoms of withdrawal considered but then excluded? For what reason?

The DSM-IV-TR criteria for substance dependence talk about withdrawal symptoms specific to the substance. In online gaming, data on specific withdrawal symptoms were not provided in literature when we designed our questionnaire. For DAS we chose agitation and irritability reported by patients in our emerging clinical practice on videogame Addiction in Summer 2008 (during the study design). We totally agree with your comments, and three years later, other criteria could be candidates like mood changes.

(MCR) p6 of pdf: "In fact substance dependence is outlined in the DSM-IV-TR and this scale was identified as the strongest scale for addiction [21, 22]." It's not clear to me what would make a scale "strong for addiction". Please clarify this sentence.

We agree with your comment. We have therefore clarified this term in the manuscript. The aim of our study was to show that DAS was a pertinent scale in first-line screening of gamers who could be at risk of developing or presenting problematic overuse or addiction to MMORPG. But DAS was not supposed to be a diagnosis scale. Diagnosis could be only established by a psychiatrist during an interview and not by a self-administered questionnaire.

Methods

(DR) p7 of pdf: "All responders were voluntarily, declared having over 18, consented to participate online and authorized researchers to use their incomplete data when necessary." I suggest rephrasing this to something like: "All responders voluntarily declared that they were age 18 years or older. All participants consented to online study participation and authorized the researchers to use their incomplete data when necessary."

Your comment had been taken into consideration and we rephrased this part of manuscript (line 259 to 261).

(MCR) p7 of pdf: "The online questionnaire consisted of a 76-item self-administered list of questions -taking 45 minutes to complete- assessing social and demographical data, the relationship between gaming and health, gaming and socioprofessional consequences, and clinical standardized scales assessing dependence." Provide these questionnaires in an appendix. Otherwise, the reader cannot tell what you did, nor attempt to replicate it. I was unable to evaluate substantial portions of the manuscript due to the lack of such an appendix entry.

The French questionnaire was translated into English and added in an Appendix file.

(MER) p8 of pdf: "Three instruments were used in the present study." Isn't this incorrect? You mention using another questionnaire (the one with 76 items) on the previous page, in addition to the DAS, GIAD, and ISS scales.

We have clarified the Methods section, in particular the Study design, and we provided the questionnaire for a better understanding. In fact, the questionnaire presented here consisted of 63 items including sociodemographical data, the 3
different scales, each with its own specific items and different questions related to online game habits, game effects, etc.

(MCR) The DSM adapted Scale (DAS) is not provided. Please provide it. I was unable to evaluate much of the Results section without it.

We added the questionnaire including the DAS in an Appendix file.

(MCR) p8 of pdf: There is not enough detail in the statistical analysis subsection of the methods for me to understand how you analyzed your data or for me to try to replicate that analysis.

Taking into consideration your comments, the statistical analysis subsection was rewritten to provide more details.

(MCR) p8 of pdf: "Two-sample t-tests were used for the comparison of continuous variables" Why were two-sample tests used? I expect that results from the four scales (DAS, GIAD, ISS and custom 76 item scale) would be correlated within subjects. (In fact, if this were not the case, there would be serious construct validity problems with using these scales to investigate online gaming addiction.) Within-subjects effects could potentially make multiple two-sample tests not independent of each other in certain cases. Did you consider a mixed effects ANOVA, which would address this issue?

The aim of our study was to characterize MMORPG gamers. With the lack of validated scale to screen MMORPG overuse, we decided to adapt the DSM-IV-TR substance. We also compare the responses of gamers for DAS versus 2 others Internet addiction scales (ISS and GIAD). The three scales were included in the questionnaire and all participants responded to the question in the same order. For the three instruments, a cut-off defined the negativity or positivity criteria. So the variables were binary qualitative data (the anova test is not required). The 3 scales were supposed to assess nearby but not identical reality, so the responses were statistically associated but did not constitute 3 repetitions of the same information. So, considering the objectives of the study, a mixed model (like a multilevel logistic regression model) didn’t seem to be appropriate.

(MCR) What multiple comparisons correction methods were used?

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.

(MCR) p8 of pdf: "All variables which were significant in univariate analyses (P-value < 0.20)" P < 0.2 is not accepted as a sufficiently rigorous false positive rate. Is this a typographical error?

We had taken into consideration your comment and modified the manuscript content. In the revised manuscript, all variables were significant in univariate analysis with a p-value <0.05.

(MCR) p8 of pdf: "A Logistic regression model was used in multivariate analyses. All variables which were significant in univariate analyses (P-value < 0.20) were tested with a forward stepwise procedure."
Why was logistic regression used (as opposed to linear regression, for example)?

What is the forward stepwise procedure? You must provide sufficient detail / references for the reader to reproduce your methods.

The variable of interest was a binary variable. So, we used a logistic model in the multivariate analysis of the data. We had taken into consideration your comment and detailed statistical analysis in Method section. To assess independent factors associated with the DAS score (recoded as positive or negative), a multivariate analysis was conducted using a logistic regression model. Variables associated with the outcome on univariate analysis at P<0.2 were tested. Age, sex and educational level were considered as potential confounding factors (Hussain et al., 2009; Kim et al., 2008; Peters et al., 2008; Smahel et al., 2008 and Yee, 2006) and were systematically introduced in the logistic model (adjusted results).


Results

(MER) p9-12 of pdf: For each result reported, you must include a p-value, test used, and relevant details of test used such as degrees of freedom. These details should be included in the main text event if they are also reported in the tables, as inclusion in the main text makes things much clearer and lessens the burden on the reader in terms of mapping statements in the text to results reported in tables.

Taken into consideration your comment, we detailed statistical analysis in Method section to a better comprehension. However, for a sake of clarity, we suggest not to duplicate all the significance information in the text and in the tables.

(MER) p10 of pdf: "Positive groups were less likely to be graduates" Graduate of what? Secondary school? University?

Regarding the school grade, the positive groups were mostly below high school diploma (37.2%) or between high school diploma and university degree (29.3%) compared to the general population of our study (respectively 9.6% and 66.7%) (line 371).

(MCR) p10 of pdf: "Regarding the similarities with previous studies [10, 11, 13, 26, 27], age, sex and educational level were chosen as adjusted variables for MMORPG addiction obtained by the DAS scale." What is an "adjusted variable for MMORPG addiction obtained by the DAS scale"?
We had detailed in the statistical analysis in the Method section and the sentence including “the adjusted variable” has been modified: “Variables associated with the outcome on univariate analysis at P<0.2 were tested. Age, sex and educational level were considered as potential confounding factors (Hussain et al., 2009; Kim et al., 2008; Peters et al., 2008; Smahel et al., 2008 and Yee, 2006) and were systematically introduced in the logistic model (adjusted results”).


(MCR) p10-12 of pdf, bottom paragraph: This description is too imprecise and informal.
Eg: “Gamers without other leisure activities” What leisure activities are being considered? What questions did you ask to find out? Was this part of the 76 item questionnaire mentioned above?
Similar criticisms apply to “who went out less”, "saw fewer friends", and "were more dependent", the last of which did not have OR and CI numbers shown for it (p10).
What are OR and CI. Is OR odds ratio? CI is confidence interval, I assume? Acronyms must be spelled out.
Similar criticism apply to the other constructs and data presented in the remainder of the results.

The Results section was clarified for a better comprehension. Moreover we added a more important description of the course questionnaire, in particular the fact that participants answered yes or no except for one or two open-ended questions. In this latter case, we clarified the question and the choice of answer.
The questionnaire has been translated into English and added in an Appendix file. For the acronyms, OR was Odds Ratio and CI was Confidence Interval. The acronyms were spelled the first time they were used and listed in the List of abbreviations section.

Discussion

As discussed above, the manuscript did not include enough details of methods and results for me to evaluate the results. Therefore, I cannot evaluate most of the discussion, which is based on the results.

We have taken your comments into consideration and have re-written the Abstract, Background, Methods, Results and Discussion sections.

(MCR) p13 of pdf: "We showed that the adapted substance DSM-IV-TR scale (named DAS) appeared the most efficient to evaluate MMORPG dependence.
This conclusion is poorly supported. You state on p9 that "In view to these results and due to the fact that ISS and GIAD scales assessed Internet dependence and
not only MMORPG dependence, we focused the following work on the MMORPG dependence according to DAS scale." That is, only the DAS is designed to measure MMORPG dependence specifically as opposed internet dependence, so you do not have a pure comparison set. In addition, what is the formal meaning of "efficient" in this case?

_Regarding your comment, we clarified the Discussion section. In the present study, we used 3 different scales to screen the population of MMORPG gamers. Two scales, GIAD and ISS, assessed Internet Addiction whereas DAS was an instrument to screen the gamers who could be at risk of online gaming addiction. Our results confirmed the need for a specific tool for each component of the Internet. In fact, according to GIAD the scale for Internet addiction we used in our study and that is similar to DAS, both inspired from DSM IV substance dependence criteria) 44.3% of participants were positive. While according to DAS, 27.5% of responders were positive. These rates tend to suggest that MMORPG as a sub-type of Internet activities needs specific addiction tools. In this study, we focused on MMORPG with a scale, named DAS, for first-line screening of gamers who could have a problematic online gaming use. This scale was a first-line instrument. A diagnosis of addiction could be established only by a psychiatrist during an interview._

**Level of interest:** An article of limited interest  
**Quality of written English:** Needs some language corrections before being Published

_An English proofreader has corrected the manuscript._

**Statistical review:** No, the manuscript does not need to be seen by a statistician.  
**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.

**Summary**

In the article titled “Massively Multiplayer Online Roleplaying games: Comparing characteristics of addicted vs non-addicted gamers in a French adult population”, the authors use an exploratory approach with the aim of determining which of 3 scales (the augmented DSM IV DAS; ISS; GIAD) best detects and describes online gaming dependency.

**Major compulsory revisions**

**Results**

Comment 1
It is unclear to me whether the chosen statistical tests are appropriate because:
1) As I understand it, the McNemar Chi test is used for 2 x 2 tables which included matched-pair subjects (would the Cochran test have been more applicable here?)

   In the submitted manuscript, the MCNemar was cited one time in the material section (and refer to scales comparisons) and one time in the results section. The later citation was a mistake (this test was used in this context) and the manuscript has been modified. We agree with the reviewer to say that the McNemar test was not the right one for scales comparisons. However, the 3 tests were not considered as the same level, and the DAS score was the major result we focused on. So, our question was not whether one (at least) of the test differs from the others (which could offer the opportunity to use the Cochrane test), but rather how much DAS score and Internet dependence were linked. So, Pearson chi square test was employed; text and tables of the manuscript were modified.

2) All events do not seem to be independent and mutually exclusive – i.e. the 3 surveys target the same population with similar questions; non-independence is assumed; also, did they take the tests in the same order? Perhaps some of these issues can be clarified by a more elaborate description of the statistics undertaken.

   We had taken into consideration your comments and re-writting the statistical analysis in the Method section.

   In the Methods section, the questionnaire was explained in more detail: the questionnaire always followed the same chronology and participants had to answer the current question to be able to pass to the next one. Moreover participants answered yes or no except when explained otherwise. Finally, the French questionnaire was translated into English and added in an Appendix file.
Comment 2
Use of the unqualified term ‘addicted’ in many passages seems misleading; it is unclear whether the term ‘addicted’ and ‘dependent’ are used here interchangeably. For instance, in the results section, the sentence “Moreover, those who felt a sense of power (OR:3.21, 95%CI:1.62-6.36) or of group belonging (OR:1.63, 95%CI:1.06-2.50) were more addicted to MMORPG games” seems problematic given that there are many activities (e.g. skiing or reading) in which group belonging (e.g. in athletic groups or book clubs) at high levels might be considered as successful activities. Thus, it would have been interesting to see a non-online-game comparison group whose activities are largely not considered (socially) to lead to addiction/dependence.
For a related issue, see also comment 4 below.

Taking into consideration your comments on the term “addicted” and “dependence”, the manuscript was re-written to explain the differences between these 2 terms in Background section and to define that the phenomenon observed in our study is addiction and not dependence (in Discussion section).
In the Results section, concerning the sentence “Moreover, those who felt a sense of power (OR:3.21, 95%CI:1.62-6.36) or of belonging to a group (OR:1.63, 95%CI:1.06-2.50) were more addicted to MMORPG games”. This sentence has been changed (line 405 to 409). Participants were asked if the game provided them a feeling of belonging to a group. In this question, group belonging was linked to the game and not to aspects of daily life such as reading or skiing.

Discussion
Comment 3
Given that the authors show that the 3 tests are invariably linked, it seems unreasonable to claim in the discussion that “We showed that the adapted substance DSM-IV-TR scale (named DAS) appeared the most efficient to evaluate MMORPG dependence.” Specifically, I didn’t see any clear evidence that it was the most efficient of the tests.

We have taken your comment into account and modified the manuscript. Here, we used 2 Internet addiction scales, GIAD and ISS, that we compared to a third scale that we have adapted for MMORPG, DAS. In this context, we have modified the manuscript: the term “positivity to a scale” substituted the term “dependence to a scale”. DAS scale was a first-line instrument to screen for a probable at-risk population to online gaming addiction but was not a diagnosis scale.

Moreover, the inherent differences in the tests (as pointed out by the authors) raise the issue of why these 3 particular tests were compared and not, for example, Young’s Internet Addiction Test (e.g. Khazaal et al 2008)?

We chose to compare 2 Internet Addiction scales (GIAD and ISS) to DAS because for the 3 scales, certain dimensions measured were similar such as greater consumption than expected, control loss and socio-professional and recreational adverse impact. The reasons of chosing every tool is better explicated in Methods section. Moreover, the Young’s Addiction Test was validated (Khazaal et al., 2008) in French once the design of our study decided and legal authorizations obtained (summer 2008).
Comment 4
The idea that the selected tests do “not measure the same types of addiction…” further emphasizes the important question of whether it is even appropriate or useful to speak of internet ‘addiction/dependence’ in this sense. This is something that the authors should discuss briefly.

*In agreement with your comment, we decided to re-write all different parts of the manuscript. In fact in background section, we focused on the differences between the terms “dependency” and “addiction”. Moreover our results tend to suggest that DAS was an interesting first-line instrument to screen gamers who could have a risk for online gaming addiction, but does not diagnose online gaming addiction. In fact this latter could be only be established by a psychiatrist during an interview.*

Along these lines, it is curious that the authors point out repeatedly that ‘dependent’ gamers are more likely to be younger. Given that these types of games (and the internet) have now been in existence for a couple of decades, and given that those with substance abuse/addiction can battle it their entire lives, it is unclear why young dependent gamers are no longer dependent at later ages (if this is, in fact, the case). How do the authors explain this?

*In the discussion section, we added a part about the number of years that gamers played online video games. We observed that a young age was a stronger variable associated with DAS positivity compared to the number of years played. In fact, for both groups, we observed the same number of years playing online video games [8.54 years (Standard Deviation (SD): 6.66; 95%CI: 7.81-9.26 for the DAS negative group versus 8.41 years (SD: 5.93; 95%CI: 7.35-9.46) for the DAS positive group]. Longitudinal studies are in fact needed to assess the gaming activity evolution.*

Moreover, the correlative links between traits/characteristics and ‘dependence’ seem suspect in many instances, given the potentially circular nature of the concepts involved. For instance, dependence is defined in terms of social norms and expectations, so it is of no surprise that those gamers likely to be found ‘more dependent’ are also those who show more anomalies in these social norms (e.g. staying up later/getting fewer hours of sleep; more irritated; go out less etc.). In other words, the language of the manuscript often seems to imply that the presence of ‘dependence’ can be used to support the presence of associated ‘addiction’ traits, although some of these traits are explicitly or implicitly used to initially define ‘dependence’.

*We had taken into consideration your comment and re-writtten the manuscript, notably the background section to distinguish addiction and dependence, and to clarify that no causality could be assessed between gaming and some adverse consequences on social-life or behavior. A co-occurency is self-reported by the participants between since they started MMORPG gaming, and some adverse effects reported in social field, sleep and feelings.*

Comment 5
The authors should be commended on acknowledging the number of limitations inherent in the study. Nonetheless, the paper would have benefitted greatly from
the consistent use of this type of language much earlier (including increased usage of cautious language such as emphasizing ‘potential’ addiction).

Further in the discussion, describing the DAS as a “short and brief index seemed so to be a solid and robust scale which did not overestimate dependence” seems to be an overstatement as this new DAS scale can hardly be called reliable based on the data presented.

The discussion section has been re-written. In the present study, we observed that DAS appeared a good first-line instrument to screen for MMORPG addiction but not to diagnose it, and diagnosis could be only established by a psychiatrist during an interview. Moreover, our results showed that positivity to DAS was associated with numerous factors which were associated with other addictions such as alcohol addiction.

Comment 6
Regarding the unrepresentative, self-selected, sample – although the authors point this out as a limitation they suggest that this sampling strategy could not be avoided due to the nature of the study (i.e. investigating online gamers). However, it seems that alternative strategies could have been employed. For instance, initial online requests could have been posted for the study which would allow for the prescreening and capturing of individuals without unnecessary biases (e.g. without revealing the full nature of the study). Other strategic approaches do seem possible and this should be noted by the authors.

In addition, perhaps some constraints (e.g. using a program which restricts similar IP addresses from the test) could have been employed to improve the study?

The Methods section, particularly the study design, was explained in more detail. In our study, no other strategic approaches could be used because, according to Ethical French standards, we are under obligation to reveal the full nature of the study. Regarding the constraints that could have been implemented to improve the study, we could not use a program which restricted similar IP addresses from filling in the questionnaire because we had committed ourselves to the Ethical French Committee rules which stipulate that you cannot filter the IP addresses of participants, in order to guarantee them total anonymity. Moreover, the questionnaire took 45 minutes to fill in, assessing social and demographical data, the relationship between gaming and health, gaming and socioprofessional consequences, and clinical criteria screening Internet and online game addiction. An amount of time that gamers could not spend repeatedly without a comprehensive goal or benefit in doing so. Moreover in the Results section (participants’ part), we explained the procedure for quality control of data to eliminate inconsistent data.

Minor essential revisions

Abstract

Comment 7
The first instance of MMORPG should include the definition of the acronym (i.e. include the “massively multiplayer”)

We included the complete definition of the MMORPG acronym in the abstract and in the List of abbreviations section.
Comment 8
The acronym DAS should be defined earlier in the background section (i.e. when it is first discussed).

We included the definition of the DAS acronym in the abstract and in the List of abbreviations section.

Results
Comment 9
“56 subjects had indicated that they did not agree to the data being used if their data were incomplete…” Does this imply that the authors included incomplete surveys? If so, what were those numbers? Though it states that 5 subjects were excluded because their surveys were more than 10% incomplete, it is not clear how many incomplete surveys were included. Moreover, what were the authors’ criteria for removing 2 subjects? Were the survey responses subjected to tests of internal validity? This issue is also emphasized by the sentence in the discussion: “Moreover, data quality control eliminated inconsistent questionnaires.”

To make this clearer, the Results section, particularly the Participants part, was re-written. In fact, no incomplete survey was included in this study. And the reason for excluding certain questionnaires was explained in detail: more than 10% of the data were missing for 5 participants and 2 questionnaires presented inconsistent data when we compared several demographic characteristics such as age/number of children/family status or age/educational level. 453 questionnaires were therefore included in this data analysis.

Comment 10
Grammatical errors should be eliminated.

An English proofreader has corrected the manuscript.

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

An English proofreader has corrected the manuscript.

Statistical review: Yes, and I have assessed the statistics in my report.
Declaration of competing interests: I declare that I have no competing interests