Reviewer's report

Title: Massively Multiplayer Online Roleplaying games: Comparing characteristics of addicted vs non-addicted gamers in a French adult population

Version: 2 Date: 2 February 2011

Reviewer: Dave Hayes

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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?)
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.

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.
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.

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)?

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.

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?

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’.

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.

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?

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

Comment 8
The acronym DAS should be defined earlier in the background section (i.e. when it is first discussed).

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.”

Comment 10
Grammatical errors should be eliminated.

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

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

**Declaration of competing interests:**

I declare that I have no competing interests