Reviewer's report

Title: Having mentors prevents depressive symptoms via decreasing internet gaming: A moderated mediation analysis

Version: 2
Date: 18 February 2014
Reviewer: Christopher Ferguson

Reviewer's report:

Overall I found this an interesting and potentially important piece. Here are some comments for improvement:

Major, Compulsory Revisions:

As one issue, the report has such a large sample size that many very minor effect sizes (for example $r = .04$ for video game use and depression) are reported as "statistically significant" without noting that these effects are so small (almost zero) that they have little practical relevance. This is a big risk of studies with large sample sizes...you can get a lot of spurious effects that pop up as statistically significant, despite being very small. I'd advocate selecting a minimal effect size, typically either $r = .1$ or $r = .2$ as minimal for practical significance and I would treat all values below that as non-significant. This would help with overreporting of spurious effects. Also the authors should, in their discussion, be careful to highlight...and I mean repetitively...that the effects they are seeing are very, very small, and should not be overinterpreted.

Have the authors examined for any curvilinear effects? See Allahverdipour et al., 2010 as an example of a study showing curvilinear rather than linear correlations between gaming and mental health.

The Gentile et al., study reported by the authors is not a very good one and has come under criticism for the non-valid measures used in the study and for their eagerness to overinterpret very weak effect sizes. I would suggest dropping that study due to it's flawed nature, or spending some time being very honest about it's significant flaws (and I mean several sentences, not just a brief note).

I wanted to see more detail on the measures, particularly worry and mentors, including coefficient alphas for the current sample.

In the multivariate analyses, I think you are reporting unstandardized coefficients but calling them "beta"...beta, however, is usually reserved for standardized regression coefficients. I'd actually rather you report the standardized regression coefficients, not unstandardized (standardized are more useful). If these ARE standardized coefficients, I wonder if there may be a problem...they appear to be much larger than the bivariate $r$ which is the opposite of what you'd expect and is typically a red flag. Have you run multicollinearity diagnostics?
Minor Essential Revisions:

When talking about links between pathological gaming and mental health outcomes I might suggest a brief discussion of some of the conclusions of the Ferguson, Coulson and Barnett (2011) meta-analysis on this issue in Journal of Psychiatry Research.

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