Reviewer's report

Title: Concerned significant others of people with gambling problems in Finland: a cross-sectional population study

Version: 1 Date: 6 January 2014

Reviewer: Francis Markham

Reviewer's report:

- Major Compulsory Revisions
The author must respond to these before a decision on publication can be reached. For example, additional necessary experiments or controls, statistical mistakes, errors in interpretation.

1. There is an error in the interpretation of the multivariate logistic regression analysis. In the discussion, the authors state that: “Furthermore, multivariate analyses revealed an existing gender difference denoting that female CSOs had two significant gambling related variables as underlying factors that may cause distress in their lives: their own gambling problem and past-year gambling. Further studies are needed to understand why female CSOs have more gambling related behaviours than male CSOs.” This is apparently on the basis of Model 2, in which the odds-ratio of being a CSO for past-year gambling was estimated as 1.04 (.74-1.50) for men and 1.51 (1.09-2.08) for women, with only the women’s estimates being statistically different to 1. This does not mean that there are statistically significant differences between this predictor between the two groups, however, as the authors imply. Indeed, the confidence intervals overlap significantly, suggesting the absence of statistically significant differences (for more on this common error, see Gelman, A. & Stern, H., 2006. The Difference Between ‘Significant’ and ‘Not Significant’ is not Itself Statistically Significant. The American Statistician, 60(4), pp.328–331).

2. The same problem applies in the “gender specific differences particularly with risk level of alcohol consumption and smoking.” While the 95% CIs do not overlap for males and females with respect to risky alcohol consumption in Model 2, they do in Model 1. These estimates should be considered to provide only tentative support for the existence of a gender difference here. However, the result for smoking is much stronger, with the analysis providing evidence of a gender difference in the correlates of being a CSO. This is the only clear gender difference I can see in these results, and therefore perhaps warrants more discussion in the section about gender differences: Why are women who smoke more likely to be a CSO than men who smoke?

- Minor Essential Revisions

1. Page 4, paragraph 1. You should mention that this range is cross-national and that these are estimated rates (as opposed to actual prevalence rates of problem
gambling) since survey methods tend to play an important role in determine these estimates.

2. The method by which the results of comparisons are reported throughout the manuscript (and abstract) is not very helpful. For example, the authors write that “Among male CSOs, the percentage of close friends was larger compared with females (p<0.001).” Reporting the p-value here does not convey useful information, except to say that we are very sure that the prevalence of close-friend CSOs differed between genders. What would be more helpful would be to include the point estimates for these two groups, e.g. “Among males, the percentage concerned about close friends (14.4%) was larger compared with females (10.3%).” If the authors feel it is important to provide evidence regarding the likelihood that these point estimates are statistically different in the text, the inclusion of 95% confidence intervals for these proportions would be much more informative.

3. It should be noted in the discussion that, due to the question wording, this should be considered a “lifetime CSO” prevalence rather than a past-year CSO prevalence.

4. It should be noted in the discussion that due to the exploratory aims of this study, a large number of statistical tests were undertaken without corrections being made for multiple comparisons (e.g. Bonferroni etc.). While some might take a dim view of such ‘data dredging’, I think it is appropriate given the limited existing theoretical or empirical knowledge about CSOs, especially in a journal such as BMC PH. Nevertheless, it is important to warn the reader, perhaps in the discussion that it is expected that a number of the findings of significant relationships in this manuscript are incorrect and that future research testing specific hypotheses is required to confirm the existence of these specific relationships.

5. The authors do not specify how the size of the study arrived at? Was a power analysis conducted?

6. Please disclose how missing data was treated in all of the analyses.

7. Given that this is a descriptive study in an emerging area with little consensus about the prevalence of CSOs, the ability to compare this result with others is important. The Norwegian study (which estimated 2.0% as CSOs) is not immediately comparable because it defined CSOs at the family level only. Yet some comparison is possible even from this manuscript using the rates in Table 1, which suggests a family-CSO rate of between 9.8% and 6.9% (depending on how many respondents were CSOs for both family AND friends). Either way, this is a much greater estimate than that found in Norway. The following statement in the discussion is therefore erroneous: “Differences between our results and the results of Wentzel and colleagues (2008) may be explained by the divergent definitions for the CSO: the Norwegian definition was restricted to a family context, whereas our definition also included people outside the family.” Rather, it is more likely that the differences arise because CSOs in the Norwegian study
must have known that they were lied to by their gambling SO and noticed that their SO spent more and more money on gambling.

- Discretionary Revisions
1. Given the nascent nature of research in this field, comparison between studies is important. Yet this study has not used the same question to define CSOs as other studies, making comparison difficult. While breaking comparability may be justified if it is felt that the new measure offers a substantial theoretical or empirical improvement over other studies, no such justification is offered. An explanation of why the very similar Swedish definition was not used would be helpful.

2. It is difficult to understand how concern for significant others constitutes a public health issue as such. The useful introduction to this article makes it clear that SOs of problem gamblers experience a range of impacts, such as lost money, deteriorating social relationships, violence, psychological distress and so on. However, no reason has been given for enumerating the number of CSOs, rather than the number of people experiencing harms from others gambling which would arguably be more informative. A discussion of this decision would be welcome.

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

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