Reviewer’s report

Title: The protective role of religiosity against problem gambling: Findings from a five-year prospective study

Version: 0 Date: 30 Jul 2017

Reviewer: Don Ross

Reviewer's report:

In a longitudinal survey of adults from Belleville, Canada, in which regular gamblers were deliberately over-sampled, the authors examined the statistical relationships between (a) baseline frequency of religious attendance, (b) overall self-reported religiosity at baseline, (c) baseline severity of problem / pathological gambling, and (d) change in severity of problem / pathological gambling over five years.

The manuscript is not yet ready for publication but can be made so with some relatively straightforward revisions. Most importantly, analytical and modeling methods are appropriate to the data - with one notable exception - so most suggested revisions mainly involve writing.

I will indicate recommended revisions by walking sequentially through the manuscript. This walk-through will include noting grammatical slips and typos.

p. 4, line 19: the semi-colon after <gambling> should be a comma.

p. 4, line 36: <how one> should be replaced by <which someone>.

p. 4, line 46: delete <their stance on>, which is semantically redundant.

p. 4, line 53: delete <the holy book of Muslims>, which is condescending to the educated reader.

p. 5, line 4: delete <the respective> and change <code> to <codes>.

p. 5, line 21: replace <influenced> by <predicted aspects of>; the current phrasing over-interprets the findings.

p. 8, bottom paragraph: Here we encounter the one aspect of the revisionary work that involves additional analysis. The authors should not throw away the substantial sample of respondents who preferred not to identify their religion. This is interesting behaviour in the study context, so the data are also potentially interesting. One might understand the exclusion if the authors were estimating a binary dummy for {religious, secular}, but they are here doing no such thing. Perhaps the authors exclude 'preferred not to say' on the basis of the following reasoning: this group includes people who are 'really' members of other categories, and therefore its inclusion...
would introduce noise. But such reasoning is inconsistent with the proper scientific understanding of self-reported data. The categories are categories of responses, not categories of observed behaviour. Who knows, for example, how many self-reported Catholics are 'really' atheists? Self-reported data are what they are; we cannot, in this instance, see beneath them. If the authors have a reason for their exclusion I'm not seeing or correctly attributing, then they could alternatively include the data on those who don't reveal their religion by analyzing the other data in a hurdle model, i.e., as conditional on self-identifying religion. This would allow differences in other variables between those who answered and those who declined to be represented in the results.

p. 14, line 14: The authors' reference to "various behavioral addictions" is gratuitously tendentious. It is controversial as to whether there is convincing evidence of genuine behavioral addictions other than gambling. Of course an argument that there is such evidence could be mounted. But this is tangential to the paper and would represented a distracting digression. Thus the authors can simply say <various normatively counter-indicated habits> and then insert <'excessive'> in front of <eating>.

p. 14, lines 31-33: The authors say that "The results of the present study are … in line with previous findings indicating that religious service attendance is a more robust protective factor against gambling than other aspects of religiosity." This over-interprets the authors' data, and perhaps the previous findings also. Regular and frequent religious service attendance plausibly differs from other measures of religiosity in reflecting latent self-discipline; and there are truckloads of data indicating that self-discipline, on various measures, is the most important protective factor of all against (problem) gambling. To conclude that regular and frequent religious service attendance itself is a protective factor thus out-runs the data. The alternative hypothesis in terms of self-discipline is particularly salient in light of the report on lines 43-48 that "only weekly or more frequent past-year religious service attendance influenced the slope of problem gambling severity." Note that the use of <influenced> in this sentence is itself an over-interpretation.

p. 17, lines 9-10: The summary claim that "Overall, the findings from the current study … suggest that different aspects of religiosity, including religious service attendance and personal religiosity play a protective role against problem gambling" is misleading. Where religious service attendance is concerned, it overlooks the point made in my comment immediately above. Furthermore, the claim suggests a much more unequivocal relationship between religiosity and problem gambling than the authors' own data support. After all, they find lower baseline gambling and problem gambling in atheists and agnostics than in Christians; and they find that some aspects of religiosity predict less rather more transition away from more severe levels of problem gambling in some sub-samples (which they interpret on the basis of a plausible speculation on p. 15, lines 53-58). The conclusion should be re-written to reflect the far more nuanced and equivocal relationship between religiosity and gambling problems that the data reported here actually support.
Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

No

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