Reviewer’s report

Title: A meta-regression analysis of 41 Australian problem gambling prevalence estimates and their relationship to total spending on electronic gaming machines

Version: 0 Date: 19 Feb 2017

Reviewer: Matthew Browne

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Minor comments:

pg 5 line 84 - 'presumably…' I think these is a somewhat limited and unfair characterisation of the rationale for population-level adaption. I think it's fair to acknowledge that cultural and social adaption to dangerous products or environmental features run far deeper that simply destigmatisation and improved treatment. E.g. knowledge regarding harmful experiences can result in media and informal discourse that leads to new cultural norms regarding acceptable / unacceptable behaviour.

pg 6 last para, page 7 first para - these paragraphs begin with run-on phrases - which doesn't scan well.

line 128 - Although I can well imagine why EGM losses are the most reliable surrogate, this is an unclear justification.

line 132 - explain this better also - give some idea in what year was gambling made more available / legal in each jurisdiction, the years in which surveys were done, to give some idea of the expected adaptation effect.

line 133 - 138 - these repeated justifications for employing Australian states, especially the last sentence, could be phrases a little more elegantly.

line 139 - when, how many? Does this mean that data from all states were incorporated into the meta-analysis? If this information is provided later on, then this sentence is superfluous

line 167 - carry on initial phrase of para

line 181 - 'association' yes it's not causal, yet regression involves on variable being treated as the 'response' so this should be explicitly mentioned.

line 183 - be more specific - do you mean heterogeneity in effects calculated within each study?

line 184 - why logistic? mention you response variable so it's clear why.
General comment - Usually GLME models can be written out very concisely and explicitly in a simple formula, that explains the fixed and random effects very clearly. If that's possible in this meta application, perhaps do that? Based on what's written currently in this section, I have no real idea of the model that's being applied, or the data that it's being applied to.

191 - 'spuriously'

205 - Very neat. Good to see implementation of the two extremes and the 'Bayesian middle ground' within the same framework.

205-206 - I'm confused by this 4.0x inflation so other readers probably will be too

211 What does tau denote?

237 - The reader needs heterogeneity explained to them in this context. Define I^2

247 - When you fix the estimates using a highly constrained prior, then the data at hand is not contributing to parameter value - in this case, what does the 'significance test' comparison with zero actually mean?

Main comments and advice (re-iterating some the above):

1) Please provide the reader with a clear specification your model and the parameter labels (e.g. tau, I^2). Although I comprehend and appreciate the general Bayesean strategy, due to the 'broad brushstrokes' explanation

2) Because of the above, I'm not 100% clear on the way on the data analysis. The data table implies that raw prevalences and expenditure were analyses. However, the discussion of heterogeneity (of effects one assume), implies that effect coefficients from each study were the basis of analysis. Please confirm the 'meta data' that was incorporated into your analysis. I'm not 100% clear as to whether it was simple PG prevalence and expenditure, or whether the relationship between these two variables was reported by each study, and your meta analysis analysed these coefficients.

2) The wide uncertainty range is understandable, given the necessarily relatively moderate number of studies, and the wide heterogeneity in effects. E.g. EGM loss effect estimate of 1.35 [1.04, 1.74] means we are only moderately confident that the relationship is non-zero - that spend and problem are unrelated. This is acknowledged, but should be more forth-rightly explained as a limitation in the discussion. In short, this meta analysis suggests that the relationship between loss and PG prevalence, if it exists, may be either very small, or reasonably large. So unfortunately the results speak to the unsatisfactory state of evidence at the moment regarding this relationship.

One reason that we might explain this heterogeneity is due to restriction of range of the true effect - i.e. the true variation in PG prevalence in Australian states is probably quite small. The
variation in PG prevalence observed in this dataset may be dominated by differences in sampling, which would show up as noise.

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Some consideration of the reasons why results are so heterogenous would strengthen the paper.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

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?
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