Reviewer's report

Title: Mental health affects future employment as job loss affects mental health: Findings from a longitudinal population study

Version: 2 Date: 17 March 2013

Reviewer: Simon Overland

Reviewer's report:

I appreciate the authors’ responses to the comments from both reviewers, and the corresponding changes made to the manuscript. The authors’ responses regarding choice of statistical model are to the point and reassuring. I agree that retaining their initial model is the best choice given their nominal measurement of employment status and that they did not expect a systematic growth. And, many of my other concerns, such as loss of statistical power through missing responses, are now eliminated with the authors using the recent version of the Mplus software where this is handled.

I think the text revisions have strengthened the paper and added clarity.

Minor essential revisions:

There is one important topic where I remain a bit confused however. In the manuscript, the authors say the variable on labour force participation was used as two (separate?) dummies: one on unemployed vs employed, and another on “not in labour force” (NILF) vs employed (pg 8). In the reponse letter, the authors describe it as a nominal variable, and reading about it there I understand it as a nominal variable with three categories (and not two sets of dichotomous variables). And, in the prose, the emphasis is on unemployment and not so much the NILF-category (for example the abstract only mentions unemployment, although the NILF was a much larger category than unemployment at baseline: 17% vs 4.2%). In Figure 1, I am not sure I fully understand how NILF was modelled, was it as a covariate along with PF-10 and marital status? If so, my understanding is that the authors want to model unemployment and control for periods where the individual was not in the labour force (as in not searching for work). As the main aim is to examine how unemployment relates to mental health over time, it strengthens the study to focus on involuntary unemployment, and adjusting for NILF would be one way of doing this. If this is a correct understanding of how the authors wanted to model the relationship, maybe they could make this even clearer by for example describing the strategy in the aims, or be specific about it when describing the analyses (pg9)?

To sum up: I think the authors could further increase precision regarding this key variable, how it was used in the modelling, and also in text making sure that the aspect of NILF (which I agree with the authors often is neglected) is reflected. Unless I have completely missed something, it is not mentioned in the abstract or the discussion as of now, and it is not made clear how NILF was used and what
the authors want to achieve by including it in the model (beyond describing the
collection of the variable pg 8). Adding detail on this would strengthen the
study.

Discretionary revisions:
A minor detail, on pg 9 when describing “wave 1 variables” that were covariates, I
would have added (age and sex) just to make things clearer.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

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

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