Reviewer's report

Title: Smoking, Drinking and Body Weight after Re-employment: Does Unemployment Experience and Compensation Make a Difference?

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Reviewer: Sijmen A. Reijneveld

Reviewer's report:

This paper examines the impact of unemployment on several health related behaviors. Its introduction provides a sound basis for the aim as defined, but I have severe doubts regarding its methodology.

My main doubt concerns the Methods part. The authors indicate that they use self-reported outcomes, but they provide very little information on the way in which this data have been obtained (paper-and-pencil, internet-based, etc.?). Next, any change, albeit very minor, is assumed to be a relevant change. This implies that regarding weight a difference in reported weight of any size already leads to a ‘change’ (and to a rather high percentage of decreasing weights of about 30%). This approach may increase the prevue of the study as stated by the authors, but it leads to rather irrelevant changes being taken into account.

Moreover, regarding the other outcomes, it remains unclear how these were asked for. For instance, use of alcohol varies often across the week, being higher in the weekend than on other days. How was this accounted for? The authors indicate the use of some categories (5+ being the highest) but further details lack.

In the analyses, the authors put a large number of variables jointly in the models (Tables 2 and further). They do not provide a reason for that. Apparently, some of these additional variables will be associated with the occurrence of a given type of unemployment (and thus may represent over-adjustment).

Moreover, the authors put in their models a number of continuous level products. The use of a logistic model implies the assumption of an exponential association in that case. I am not convinced that this is a right (it has to be shown). In addition, a Box-Cox transformation should be explained.

Regarding the results, the tables contain some redundant information (too many p-values and decimals for the OR’s, -2 log likelihood being uninformative) and need a restyling and check (exp (B), should be OR, missing Ref in Table 5, categories being combined more clearly, etc.). Table 1 should also contain information on variation for the means (i.e. SDs).

Regarding the discussion, my major point is that its discusses results that may be of rather irrelevant size. A more relevant change should be defined a priori. The discussion further lacks somewhat thoroughness. E.g. at page 13, 2nd paragraph, the authors fail to discuss relevant aspects of their study such as selection bias and information bias. They then introduce a weighted analysis
without clarifying which problem that solves. In my opinion, anyhow, this should not be placed in a strengths and limitations section.

The next paragraph states that results are consistent with a part of the literature but not with the remainder. Which literature is that (references)? What does it state? And what is the (in)consistency?

Also, the discussion fully focuses on the US system without any reference to other countries with rather different compensation schemes, such as the European welfare states. How do these results compare with them?

Finally, the references need some redoing, e.g. refs. 42, 43, 46 and 48 (but this is a minor point indeed).

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

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

Declaration of competing interests:

I declare that I have no competing interests