Author's response to reviews

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

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Author's response to reviews: see over
Dear Dr. Todd,

We thank the reviewers for their comments. Our responses to their specific points are given below.

**Reviewer 1 Comments for the Author..**

1) *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.?).*

This has been addressed on page 6 and is highlighted in blue.

   “Approximately 92% of the sample is interviewed by telephone each year; the remaining 8% of the sample is re-interviewed in person[41]. The household head, defined as the husband in a husband-wife pair or the primary wage earner, is most often the respondent.”

2) *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.*

We agree with the reviewer that extent of the weight loss experienced by those who were unemployed without compensation may not be large. However, we feel that our findings that a higher proportion of people who experienced unemployment reported some weight loss in comparison with the proportion of continuously employed who reported weight loss at follow-up are still be of importance. Minor fluctuations in weight are known to be associated with stress and this finding may reflect higher levels of stress exist among those who experience unemployment without compensation, which remains a noteworthy result. To our knowledge, this is the first report to describe the relationship between unemployment compensation and body weight and we hope our findings will lead to further exploration.
In response to this comment, we have added the following comments to the discussion pg.14: “While the association between unemployment compensation and body weight change did not persist when body weight changes were restricted to a change of >5%, we think its useful to report even small changes since minor fluctuations in weight are known to be associated with stress[46] and this finding may suggest higher levels of stress exist among those who experience unemployment without compensation. This is an area that has not been previously explored and we hope that our findings will lead to further exploration of the association between body weight and unemployment compensation”.

3) 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.

We agree that this deserves further clarification. We have added the following to the methods section on page 8: “In the PSID, alcohol consumption is defined as the average number of drinks per day (less than one, 1-2, 3-4, or 5 or more per day) over the past year. Cigarette consumption is defined as the average number of cigarettes smoked per day over the past year (1-100 cigarettes).”

Average alcohol and cigarette consumption will not accurately capture binge drinking or smoking patterns and this is a limitation. We have added the following sentence to the discussion on page 14:

“Because the variables on self-reported smoking and drinking used in this study reflect usual consumption patterns over the year, we were not able to capture binging patterns or other fluctuations in substance use”.

4) 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).

We agree that a large number of possible confounders were included in our models. However, the impact of unemployment compensation on health-related behaviors is a relatively unexplored topic and little is known regarding potential confounders. We chose these variables based on previous literature and their potential for confounding.

We have modified our discussion on the inclusion of potential confounders on page 9 to read as follows: “All of our models adjust for potential confounders of the relationship between the likelihood of a change in smoking, drinking and body weight and unemployment status. These include: age, sex, race, education, health status, income, number of household members and marital status of the respondents. In addition, we created different variables to control for prior smoking, drinking and weight for each of the outcome variables as described above. The income variable used was the total household post-government income created for use in the Cross-National Equivalent File[41]. This represents the combined income after taxes and government transfers.”

5) 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).

We believe that the reviewer is referring to the assumption of a log-linear relationship between continuous variables and the odds ratio in logistic regression. There were three variables in our models that we treated as continuous variables: income, number of household members and, in the case of the models with weight change as the outcome variable, BMI in 1999. We performed a likelihood ratio test comparing the model where the effect for the variable was assumed to be log-linear to the model where the exposure effect was allowed to be non-linear through including the variable as a categorical variable. Specifically, for BMI and income, categories were created by dividing the variable into deciles. For number of household members, quartiles were used. None of
the likelihood ratio tests for these variables for any of the outcomes were significant. 
Thus, in order to increase power, we modeled these variables as continuous.

We have added the following sentence to the methods section of our paper on page 9: 
“Likelihood ratio tests comparing models assuming the exposure showed a log-linear 
effect with a more general model including the exposure as a categorical variable were 
used to determine the most appropriate means for modeling, income, number of 
household members and, BMI in 1999. None of the likelihood ratio test statistics for 
these variables were significant and in order to increase our power to detect an 
association, income, number of household members and BMI in 1999 were modeled as 
continuous variables.”

6) In addition, a Box-Cox transformation should be explained.

To approximate a normal distribution of income, reduce skewness and stabilize the 
residual variance, we tested different ways of transforming this variable. A Box-Cox 
transformation proved to be the best technique to normalize the variable. We have 
indicated in a footnote to the tables that the purpose of the transformation was to 
normalize the variable.

7) 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.).

These suggested edits have been performed in Tables 2-4.

8) Table 1 should also contain information on variation for the means (i.e. SDs).

We agree that some measure of variation would be helpful. We have included both the 
median and standard deviation for key variables in Table 1.
9) Regarding the discussion, my major point is that it discusses results that may be of rather irrelevant size. A more relevant change should be defined a priori.

We agree that it would be ideal to explore the magnitude of changes in smoking, drinking and body weight. Even though this was not possible in our limited sample size, we feel that our results contribute to the field because this topic has not been explored. We hope that our findings will spur further analyses using larger sample sizes that will allow for more precise estimates of the magnitude of the changes.

10) 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.

In order to address sources of bias in our study we have added the following to the discussion section on page 14.

“Our results could be influenced by information bias, particularly related to the use of self-reported smoking, drinking and body weight. The impact of possible information bias on our results is difficult to predict but seems unlikely to fully explain our findings. Because the variables on self-reported smoking and drinking used in this study reflect usual consumption patterns over the past year, we were not able to capture binging patterns or other fluctuations in substance use. The PSID is an established survey with a high participation rate which decreases the likelihood of selection bias. We attempted to control for the impact of a wide variety of potential confounders in our models. However, as little is known regarding the relationship between unemployment compensation and lifestyle changes, we cannot completely rule out the possibility of residual confounding”

11) 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.
In order to clarify our rationale behind the use of a weighted analysis, we have reformatted our discussion of the weighted analysis on page 15 to the following:

“In order to increase the generalizability of the PSID sample to the general US population, we replicated the analysis weighting the cases by the 2001 longitudinal weight variable created by the PSID[40]. While, the main results remained the same, the significance levels for the explanatory and control variables were higher.”

12) 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?

We have attempted to clarify our meaning by changing the paragraph on page 15 referenced by the reviewer to the following:

“Our finding that job loss increases the long-term risk of increasing alcohol consumption is consistent with previous literature showing an association between unemployment and an increase in alcohol consumption[21,22] but differs from other studies that report no such association[23-25].”

13) 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?

We have added the following passage on page 17 to address this comment:

“This study focuses on the U.S. and it will be interesting to replicate it in different countries with different compensation schemes. Previous work looking at the impact of unemployment benefits on health status has shown remarkable similarities among the U.S and European countries with different compensation schemes such as those found in Germany and the U.K[35].”
Finally, the references need some redoing, e.g. refs. 42, 43, 46 and 48 (but this is a minor point indeed)

This has been fixed.

**Reviewer 2 Comments for the Author..**

1) *I think the body weight data included is difficult to interpret and could probably be excluded.*

We agree the reviewer that the clinical relevance of this finding is difficult to interpret but this is an unexplored area and we hope that our findings with spur further investigation. Please see our response to comment #2 by reviewer #1.

2) *I think the alcohol consumption variable should consider if consumption is at harmful levels.*

Please see our response to comment #9 by reviewer #1.

3) *There is also likely to be a lot of covariation between increased smoking and increased drinking. It might be worth considering where people have an increase in one or the other or an increase in both. This may help clarify the findings.*

This suggestion is interesting and would be useful in clarifying the findings. However, as our sample size in the unemployed group is already small, we would be extremely underpowered to perform subgroup analyses such as this. We have added the following comment to the discussion on page 13: “As drinking and smoking patterns show some degree of correlation, investigation into the impact of unemployment on increases in both smoking and alcohol consumption would be of use. However in this study, very few unemployed individuals increased both their smoking and alcohol consumption and such an analysis would be underpowered.”