Author's response to reviews

Title: Alcohol use and misuse: What are the contributions of occupation and work organization conditions?

Authors:

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Author's response to reviews: see over
Dear Dr. da-Silva,

I thank you for this opportunity to revise and resubmit my manuscript to BMC Public Health (MS: 5881125601519526). As requested in your email dated 03-03-2008, I have taken care of the following points raised in your letter.

First, I have addressed reviewers’ comments and changed the manuscript accordingly. Details for each reviewer are provided below. Second, the manuscript has been copyedited by a professional English native reviser. Third, as requested for ethics/consent, I have added in the method section (subheading Data) the reference to the Government of Quebec statistical institute- “Institut de la Statistique du Québec”- which was responsible on this survey. I have also specified that written consent have been obtained prior the person interview. Fourth, I have included a 'Competing interests' section between the Conclusions and Authors' contributions. Last, the manuscript has been revised to conform to the journal style paying particular attention to the reference. BMC Microsoft Word template has been used.

I hope that everything is in accordance with your expectations.

Do not hesitate to contact me if you need any additional information.

Yours truly,

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Reply to reviewers

Reviewer 1: Mitch Earleywine

- “The current draft is very difficult to read, with some expressions that show a lack of understanding of English idiom.”

The manuscript has been copyedited by a professional English native reviser. He is acknowledged in the revised manuscript.

- “With a sample size that exceeds 10,000, it is important to consider statistically significant effects with caution. A large sample can make very small, potentially meaningless effects, reach statistical significance. Over 50 statistical tests appear in the paper, with 95% confidence intervals. With no correction for Type I error, I am not sure which of the statistically significant findings to take seriously. In addition, only 1 odds-ratio exceeds 2.0, with many of the statistically significant ones falling below 1.1. (The large sample makes these statistically significant.) I cannot get particularly excited about statistically significant predictor if it doesn’t increase risk by 10%; most epidemiologists would scoff at anything with an odds-ratio below 2.0.”
I do not see exactly what is going wrong here. First, for each model reported in Table 2, Chi-square statistics tested that the effects of independent variables in the model equal zero taking care of the number of degrees of freedom (number of variables) and a probability of rejecting the null hypothesis. These tests are further corrected to take care of the sample design effect as explained in p.15 of the manuscript. Therefore, if \( p(\chi^2 \text{ df}) < .05 \), there is at least one significant difference under the overall probability of 0.05. Second, all standard errors used for computing 95% confidence intervals were also corrected for the sample design effect. Because of the design effect in QHSS, correction for clustering of observations deflated Chi-square and standard errors by a factor of 45%.

Effects seem not, however, to be small. For occupation, OR range 1.27-2.39, and 1.27-1.78 for harassment. Theses variables thus increase the risk by 27%-139% and are reported in the discussion. For apparently small OR associated with social support outside the workplace, number of minor children, cigarettes and physical activities, I explain in p.13-15 that these variables were continuous variables. Means and standard deviations were reported in Table 1. For these variables, OR expresses level of exponential increases (or decreases) in odds for one unit increases (or decreases) in the values of the variables. For example OR of 1.03 of cigarette smoking means that the odds of high-risk drinking increases by 3% for one cigarette smoked. For 20 cigarettes smoked, the risk increases by 81%.

References:


- “Given the small size of these effects, I do not see that these data add anything to the literature.”

What it is added in this study is more fundamental for occupational alcohol research. As discussed in the manuscript, integrating in the analysis the family situation, the support from the social network outside the workplace, and a diversity of individual characteristics help and then will help in better identifying what is going wrong with occupations and workplaces regarding alcohol related problems. Therefore, the results presented in this manuscript challenge those obtained by previous research and provide a clearer picture of the specific contribution of occupations and work organization conditions.
Reviewer 2: Paul Roman

• “This appears to be a very well informed manuscript that suffers on occasion from inappropriate translation from what I assume is French.”

The manuscript has been copyedited by a professional English native reviser. He is acknowledged in the revised manuscript.

• “The investigator opens the paper with a quite clear exposition of hypotheses, except for the gender issue, which has been found of quite dramatic significance in the broader literature, and which in this instance seems relegated to an afterthought.

I first discussed the gender issue at p.5 of the original manuscript recognizing the males-females differences in alcohol intake. Furthermore, I wrote at p.10 that the moderating effects of gender need to be evaluated because of the gender differential in drinking and the unequal distribution of occupations and work occupation conditions between males and females. Then, the interaction occupations by gender, and work organization condition by gender were estimated and found non-significant (see p.17).

However, the revised manuscript now integrates more explicitly the role of gender in the discussion. Changes appear at p.21 of the revised manuscript.

• “Given the findings that have emerged in this analysis, I would strongly suggest that the authors distinguish gender in their further analyses. There is no doubt that men harass men, and perhaps that women harass men, but the governing model here is one of men harassing women. As it stands, that is not explicated in the analysis.”

This might be an interesting way to go in further analysis. However, because I failed to find specific gender interactions, a more promising avenue might be to look at gender indirect effect on alcohol intake. This will directed me toward the need to estimate structural equation models with path starting from gender to alcohol and paths from gender to work factors. Finally, because there is no gender interaction in the analysis reported here, there is no need to stratify the sample according to gender. If it uses, it will result in a lost of power and the problem will it be more important because of QHSS design effect.

However, I do not see how it is possible to conclude, based on the data reported here, that the governing model is one of men harassing women. The harassment variable measures only if the individual had been the victim of harassment in the last twelve months. I have no information on the aggressor. So, I’m only looking at the role of being harassed on alcohol intake. The results show a significant association and also show that gender is not moderating this association.

• “The author fails to consider the possibility of self selection into these occupational settings by persons with a propensity to drink more heavily. Unfortunately the cross sectional design does not allow for addressing these concerns.”
This is true. This comment is now integrated in the revised manuscript in the study limits section on p.22. This section has also been improved by adding others limits not discussed in the original manuscript.

- “The author is greatly limited by his/her data, but the intervening variable of union membership is measured so weakly that it raises major concern. It is obviously a protective factor between being able to be an active problem drinker and suffer few consequences. The discussion does not appear to address this”

The only information available in QHSS was a question asking for union membership (yes/non). However, this variable was not significant in the analysis and was not further discussed. What it is more important than unionization is surely colleagues’ social support and this is discussed as study limits (p.22).

- “Given the findings, I would suggest that this ms. might have a huge contribution to the literature on women’s harassment experience in the workplace and drinking behaviour”.

The results show that harassment is related in significance and magnitude to the drinking behaviour of both genders, not only women. It is thus an important workplace risk factor, irrespective of the victim gender.

- “Otherwise it is a reasonable piece, but its cross-sectional design unfortunately would have it join a long line of boring and inconclusive articles that say the workplace affects drinking, but not really saying much that is either new or, in particular, practical in terms of leading to suggestions for organizational change.”

The contribution of this manuscript may be seen as follow. Using a broader conceptual perspective that integrates family situation, social network outside the workplace and a diversity of individual characteristics, this study challenges results of previous studies and argues for expanding future research orientations in alcohol consumption in the workforce in order to avoid erroneous conclusions about the role of occupations and workplaces.

**Reviewer 3: Roland More**

- “…the author could add more caveats to the study limitations section on page 21.”

The revised manuscript now adds more limitations on pp.21-22.

- “However, there are a number of minor essential revisions: substantial editing by a native English speaker would make the currently substandard abstract and entire manuscript body more acceptable for publication in this journal.”

The manuscript has been copyedited by a professional English native reviser. He is acknowledged in the revised manuscript.
• “The formatting of references is inconsistent and should be tightened up if the paper is published. The conclusions require a bit of a leap of generalizing from the specific measures that were available to be analyzed from the QHSS and the broad conclusions. Acknowledging this leap more fully in the study limitations section would forestall major criticism.”

The references have been check in the revised manuscript. The conclusion has been revised to take care of this comment. Changes appear on p.23. For example, words like “demonstrates” are replaced by “suggests”, etc.