Reviewer's report

Title: Non-specific psychological distress, smoking status and smoking cessation: United States National Health Interview Survey 2005

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Reviewer: Marc Kiviniemi

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This paper reports analyses of a large, US-representative population dataset examining the relation of psychological distress and factors related to smoking cessation. The authors examine how psychological distress influences motivation to quit smoking and likelihood of successfully quitting. This research topic is an important one – as the authors point out, psychological distress and other mental health variables are related to smoking rates, making understanding how they influence factors related to cessation very important.

There are many things to like about this paper. The authors do a nice job of setting up the issue, have selected an appropriate high quality data source for addressing the research questions, and have an interesting set of research results to report. However, there are two critical issues, one impacting the ability to fully judge the contribution of the paper, that need to be addressed, as well as some additional steps the authors could take to strengthen the paper.

MAJOR COMPULSORY REVISIONS

1) As written, it is unclear which individuals were included in which analyses. The writing in the methods section and the flowchart in Figure 1 indicate that included participants were: a) individuals who had tried to quit smoking in the recent past but were unsuccessful and b) individuals who had successfully quit in the recent past. On the other hand, the writing in the results section and the sample sizes reported in the various Tables makes it sound as though there were some analyses conducted on the entire NHIS sample and that the two groups described above were only selected out for a small number of overall analyses.

If it is the case that all analyses were conducted only on those groups defined by Figure 1 (tried, not successful and tried, successful), then there is a serious issue with regards to the conclusions about motivation to quit smoking. The question of “who is more motivated to quit smoking” is, I think, most properly a comparison of those who are smoking but express no intentions to quit versus those who express intentions, whether or not those intentions are successful. I am not familiar with the NHIS questionnaire past what the authors describe in their manuscript, so I do not know if there are questions that directly address intentions to quit. If there are, then comparing the proportions of current smoker, no intention to quit to current smoker with intentionuccessful recent quitter is more appropriate. The problem with comparing the two groups defined by figure 1 is that all of the individuals in those groups have already formed a conscious
intention to quit smoking, suggesting that they are all fairly high in motivation to quit.

If it is the case that the motivation to quit analyses included all smokers in NHIS, then the methods section and Figure 1 are incorrect and need to be extensively revised to reflect how the analyses were actually conducted and which participants were included in which analyses.

2) In addition to the main analyses in the NHIS dataset, the authors also report analyses from an Australian national dataset looking at the relation between psychological distress (measured by the K6, the same measure used in NHIS) and specific mental health diagnoses. It is my assessment that these analyses should be removed from the paper (i.e., delete all of the results reported on page 10). There are several reasons for this: 1) their inclusion distracts from the main point the authors are making – the title of the paper, the set up of the research question in the introduction, and the discussion of results are all framed around general, non-specific psychological distress as measured by the K6. There is no need to empirically demonstrate that the K6 is related to more specific mental health diagnoses to draw the key research conclusions the authors wish to draw. The findings about the relation of the K6 to specific diagnoses are interesting, but in my opinion they are a different set of results on a different topic, and thus best left for another manuscript. 2) As the authors point out in the introduction, there is already published evidence for the relation of the K6 to mental health diagnoses. Although I don’t think the authors need to establish the link to draw their conclusions (for the reason stated above), if they feel the link needs to be discussed I think citing the past evidence for the relation is sufficient. 3) Establishing the link between general distress and specific diagnosis in one dataset and then establishing the link between general distress and smoking cessation in another dataset says nothing about the relation between specific diagnosis and smoking cessation. All that we know based on the distress-diagnosis analyses reported here is that a variable that is associated with smoking cessation success is also associated with mental health diagnoses.

MINOR ESSENTIAL REVISIONS

3) The way the results are described in the discussion does not always match what is reported in the results. For example, on p. 14 it states “We found that people with moderate and high levels of non-specific psychological distress were just as likely as anyone else to want to quit smoking…” In fact, the report of the results suggest that those with higher psychological distress were MORE likely to want to quit. The authors should review the discussion to make sure the descriptions are consistent with the results and revise accordingly.

DISCRETIONARY REVISIONS

4) In the introduction the authors discuss nicotine dependence as a mechanism for differences in cessation success as a function of psychological distress. The authors might also want to consider and discuss mood maintenance hypotheses. There is a fairly extensive literature on smoking in the service of mood regulation/alleviation of negative affect. It may be that those in psychological
distress get more benefit from smoking because of the alleviation of feelings of distress.

5) A writing style comment: the authors might consider describing their hypotheses in terms of their actual a priori predictions rather than simply stating the null hypotheses. As a reader, I find this both more informative and easier to follow. I will leave this suggestion to the authors’ discretion, but I think the manuscript would read more smoothly with the hypotheses restated.

ADDITIONAL COMMENT FOR THE AUTHORS

One suggestion for future work on this topic: the authors might want to look at the series of Health Information National Trends (HINTS) studies conducted by the US National Cancer Institute (available at http://hints.cancer.gov). There are an extensive set of questions about smoking beliefs, interest in smoking cessation, etc. in HINTS as well as the K6 measure. I don’t know the NHIS dataset well enough to know whether there are constructs in HINTS that are not available in NHIS, but if the smoking cessation questions in HINTS are more extensive it might be a fruitful dataset for conducting analyses extending the ones reported here.

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

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I declare that I have no competing interests