Author’s response to reviews

Title: Smoking, mental illness and socioeconomic disadvantage: Analysis of the Australian National Survey of Mental Health and Wellbeing

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Author’s response to reviews: see over
We would like to thank each of the three reviewers for their thoughtful consideration of our manuscript, and the helpful and useful feedback they provided. We have revised the manuscript based on this valuable feedback and feel that the paper is significantly improved as a result. We have detailed below the changes we have made to the paper.

Reviewer 1

Footnote c on page 45 should not say smoking cessation as this table is about smoking.

This error has been corrected.

The use of the word “inform” is inappropriate. How about “inform us about” or “shed light upon”.

We have changed this to “shed light upon” as suggested.

There are no statistical analyses described for the data in tables 1 and 2, and yet the authors make many statements about differences in the text. Please address this concern.

We have added test statistics for tests of association between variables presented in tables 1 and 2. As the survey data were collected using a complex survey design, we have used the R ao-Scott Chi-squared test of association. We now provide test statistics and associated p-values for each comparison to Tables 1 and 2.

Reviewer 2

To my knowledge, the independent association between smoking and mental illness is very well established and has been increasingly explored from neurobiological, psychosocial and genetic perspectives. I have problems to understand the rationale presented for the analyses at hand, seeing as I am not sure the work adds much to existing knowledge (but is presented as such).

We acknowledge that for some people working with the mentally ill, the association between smoking and mental illness seems obvious. However, apart from schizophrenia, it is far from established in the published literature. Indeed de Leon and Diaz, who have published the most extensive recent meta-analysis of smoking in people with schizophrenia, recently published their hypothesis that there is no independent association between smoking and mental illness for other mental disorders and thus people with other disorders could be used as a control group in future studies aimed to identify the genetic underpinnings of the association between smoking and schizophrenia (Human Genetics, 2011). We have amended the introduction of our paper to make clear that the focus of our paper is on people with mental illness living in the general community, not people with severe and chronic mental illness living in institutional settings.

Further, while some UK leaders in Tobacco Control have been pro-active in advocating for programmes based on the association between smoking and mental illness, as noted in our paper it appears to be accepted by leaders in tobacco control in both Australia and the US that there is no
need to include any focus on common mental disorders such as anxiety and depression in any tobacco control initiatives as current programmes based on other indicators of disadvantage are all that are required. As this remains a key tenet of tobacco control, we believe that the topic is an important area of research. We are unaware of any other published work that independently considers the effects of common mental disorders and socio-economic factors.

Ziedonis et al, in particular have published several papers/reviews to explore the usually bidirectional causal links between smoking and various mental illnesses. Looking at the reference list of this paper, it strikes me that many of the authors and seminal pieces of work in the area of smoking/mental illness appear to be absent.

We have previously published a review of the mechanisms underpinning the association between mental illness and smoking. For the sake of brevity we cited our previous review rather than include a detailed discussion of this work in the present paper. We recognise that this was the wrong approach, and we have revised the paper to include discussion in both the introduction and discussion of this literature directly.

We note that while Ziedonis et al have described associations between mental illnesses and smoking, apart from the case of schizophrenia, neither their work nor any other we have seen address whether these associations are causal. To our knowledge, existing work suggests the possibility of associations in both directions being possibly causal or the possibility that the association between smoking and mental illness could be attributed to a common association with some other prior genetic or environmental factor and that the exact mechanisms remain unclear.

Despite the lack of knowledge of causal pathways, we believe that, as has been the case with other factors associated with smoking, knowledge of strong associations is useful in designing interventions.

The authors don’t justify the exclusion of the more severe mental illnesses (schizophrenia, bipolar disorder, major depression) and categorise the common disorders wrongly: ‘anxiety, affective disorders, and substance abuse disorders’ - anxiety is an affective disorder itself.

The NSMHWB along with all other World Mental Health Surveys, was unable to assess low prevalence mental disorders such as schizophrenia and organic psychoses. However, affective psychoses (ie bipolar disorder) and major depression are included in the survey. We have amended the introduction of our paper to make clear that our focus is people living in the general community, not those living in institutional settings. The limitations section has been amended to note these exclusions and to comment on the possible impact on the results.

The National Survey of Mental Health and Wellbeing uses the same assessment and classification of mental disorders as all other World Mental Health Surveys. The Composite International Diagnostic Interview identifies mental disorders according to both ICD-10 and DSM-IV criteria. In this paper, we report using the ICD-10 diagnoses, but results using DSM-IV are almost identical. In both ICD-10 and DSM-IV the anxiety disorders and affective disorders included in the NSMHWB and coded in separate sections. We have adopted the same ICD-10 based classification for our study. However
we realise that there are alternative classifications of mental disorders, and a range of different terminologies used. As such we have clearly stated in the methods section the definitions of the terms used in this paper.

*It is well known that the smoking prevalence among homeless people, prisoners etc. is excessive, and that comorbidity with mental illness is high in these populations.*

People living in institutional environments, such as correctional facilities, and currently homeless people were outside the scope of the NSMHWB. This is noted in the limitations section.

We agree that the prevalence of smoking is known to be high in homeless people and prisoners. As far as we are aware, our study is the first to report the independent contribution of mental illness and previous homeless or incarceration status in a population people living in the general community.

*I am in no position to comment on the statistic methods used but find that the methods section is not written in a way to support understanding. I don’t understand the section on weighting.*

The weighting methodology used for the NSMHWB uses standard methods for large-scale surveys of this nature, and is consistent with both other World Mental Health surveys, and with surveys conducted by major statistical organisations such as the Australian Bureau of Statistics and the UK Office of National Statistics. Similar methods have been used in the British child and adult mental health surveys. We have included references to more details on the weighting methodology and techniques used for readers who are unfamiliar with the way data from complex surveys are analysed and who wish to explore the topic further.

*The results section is at times hard to read, as long sequences of similarly structured sentences describing associations found don’t always convey a lot of meaning.*

We have endeavoured to describe the results of the analysis as succinctly as possible, consistent with the general principle of the journal that the results section should only report the facts of the results and discussion of their implications is left for the following section.

*The methods section could probably be restructured, seeing as in some cases, the headings have as many words as the section associated with it.*

We have included a definition of each socio-economic indicator used in the analysis in the methods section. While the mental health and smoking measures used in the survey are consistent with international standards, some of the socio-economic indicators have been created specifically for the Australian environment. We have structured the methods section to have a separate sub-section for the definition of each socio-economic indicator. However, we have ensured that the titles of subsections are no longer than half a line of text in all cases.

*The discussion is comprehensive but extremely long and very much focussed on Australia and Australian examples of research in the area.*
We have expanded the discussion to include more examples from the US, where the policy situation is very similar to Australia.

**Reviewer 3**

1. **Need to clearly define the two outcomes: current smoking and smoking cessation. It is unclear how ‘smoking cessation’ was defined. It is later referred to as ‘time to smoking cessation’ and ‘time to quit smoking’**

   We have amended the methods section to clarify how current smoking and smoking cessation are defined in the analysis.

2. **Terminology in text and tables is not consistent. E.g. ‘bottom quintile’ in text is also referred to as ‘lowest’ in figures and ‘first’ in tables. Similarly, ‘area level disadvantage’ in text appears as ‘relative disadvantage’ in tables.**

   This language has been standardised throughout the text, tables and figures.

3. **Authors refer to tables 1 and 2 when discussing ‘associations’ between mental illness, socioeconomic status and smoking, however associations do not appear to be indicated in tables 1 and 2.**

   We have added the Rao-Scott Chi-squared test of association and associated p-values for each comparison to Tables 1 and 2.

4. **At what significance level were variables excluded from the model?**

   The significance level (p-value) for each variable eliminated from the multivariate models has been included in the text. All eliminated variables had p-values greater than 0.4.

5. **Sentence beginning “Other reports have suggested…” - citations are needed.**

   Citations to five such reports are included in the second sentence of this paragraph, and a quotation from one of these sources has been included.

6. **In the introduction, more evidence to support the hypothesis would be beneficial, particularly in paragraph 2 and 5. Are the authors suggesting that the association between smoking and mental illness is wholly explained by socioeconomic status?**

   As noted in response to reviewer 2, we have previously published a review of the mechanisms underpinning the association between mental illness and smoking. For the sake of brevity we cited our previous review rather than include a detailed discussion of this work in the present paper. We recognise that this was the wrong approach, and we have revised the paper to include discussion in both the introduction and discussion of this literature directly.
We are not suggesting that the association between smoking and mental illness is wholly explained by socioeconomic status. This was the null hypothesis that we set out to test in the paper. The results of the paper are inconsistent with this hypothesis. Hence we conclude that the association between smoking and mental illness is strong and independent of associations with measures of socioeconomic status.

7. In the methods section, page 13, paragraph 2, might be useful to state the use of Univariate and Multivariate regression models, and significance levels for dropping variables from the model.

The significance level (p-value) for each variable eliminated from the multivariate models has been included in the text.

8. In the results, paragraph 1, the authors describe prevalence of mental illness, but not prevalence of socio-economic factors. Should equal weight be given to both?

While mental illness prevalence can be defined by whether or not an individual meets diagnostic criteria, many of the socio-economic factors are measured on a relative scale so that an overall prevalence has less meaning. For instance, the prevalence of being in the lowest quintile of disadvantage is by definition 20%, but there is no consensus definition on whether this cut-off point should be the definition of presence or absence of socio-economic disadvantage. The prevalence of each level of each socio-economic indicator used in this analysis is shown in Table 1. As 12 separate indicators of socio-economic disadvantage have been included in this analysis, we feel repeating these figures in Table 1 would be repetitive. We have described the relative prevalence of selected socio-economic measures in paragraph 2.

As mental illness has the strongest relationship to smoking status of all high prevalence indicators used in the analysis, and whose impact is only paralleled by rare socio-economic factors with prevalence one-fifth or less, we feel it is appropriate to discuss the mental illness results first in paragraph one and the socio-economic factors in paragraph two.

9. In the discussion, details regarding the National Preventative Health Taskforce are mostly the same as the introduction. Might be more telling to include outcomes, or indicators of success.

As yet there have been no reports of measured outcomes from the National Preventative Health Taskforce, or any indicators of success of the National Preventative Health Agency’s initiatives. Nor have we been able to find any empirical evidence of success of any similar initiatives in other countries. This is noted in the discussion. We have removed the repetitive mention in the discussion.

10. In the discussion, page 23, paragraph 1, details regarding population-health based models for disadvantaged groups - might be useful to include some evidence of successful models, e.g. effect of price on low SES smokers.

We have been unable to find any empirical evidence of population health models being effective in low SES smokers. We have included a paragraph in the discussion regarding the effect of price on low SES smokers. While there are a couple of published papers that claim price controls are effective
in this group, other papers claim the reverse. Unfortunately all such analyses are based on economic modelling of indirect measures, so there is no clear evidence to resolve this question. This is noted in the discussion.

11. In the limitations section, it might be important to note that the NSMHWB only focused on ‘select’ mental health disorders, and did not include schizophrenia / other psychosis, a group with very high smoking rates and low SES.

The NSMHWB, along with all other World Mental Health Surveys, was unable to assess low prevalence mental disorders such as schizophrenia and organic psychoses. However, affective psychoses are included in the survey.

The limitations section has been amended to note these exclusions and the possible impact on the results.

12. Overall, the authors conclude that the ‘association between mental illness and smoking is not explained by the association between mental illness and socioeconomic status.’ If so perhaps the authors should include some discussion about what the association between mental illness and smoking is accounted for?

As noted previously, we have previously published a review of the mechanisms underpinning the association between mental illness and smoking. For the sake of brevity we cited our previous review rather than include a detailed discussion of this work in the present paper. We recognise that this was the wrong approach, and we have revised the paper to include discussion in both the introduction and discussion of this literature directly.

13. The authors pay great attention to defining socioeconomic status indicators in the methods section, but do not discuss these particular indicators in any great detail in the discussion, despite strong associations (e.g renters vs. home owners). Some discussion of these factors would tie the introduction, methods and discussion sections together more clearly.

We have added an additional paragraph to the discussion (paragraph 3) to specifically address this issue.