Reviewer’s report

Title: Impact of snus use in teenage boys on tobacco use in young adulthood. A cohort from the HUNT Study Norway.

Version: 0 Date: 10 Jun 2019

Reviewer: Reviewer 2

Reviewer's report:

PEER REVIEWER ASSESSMENTS:

OBJECTIVE - Full research articles: is there a clear objective that addresses a testable research question(s) (brief or other article types: is there a clear objective)?
Yes - there is a clear objective

DESIGN - Is the current approach (including controls and analysis protocols) appropriate for the objective?
No - there are minor issues

EXECUTION - Are the experiments and analyses performed with technical rigor to allow confidence in the results?
No - there are minor issues

STATISTICS - Is the use of statistics in the manuscript appropriate?
No - there are issues with the statistics in the study

INTERPRETATION - Is the current interpretation/discussion of the results reasonable and not overstated?
No - there are minor issues

OVERALL MANUSCRIPT POTENTIAL - Is the current version of this work technically sound? If not, can revisions be made to make the work technically sound?
Probably - with minor revisions

PEER REVIEWER COMMENTS:

GENERAL COMMENTS: Thank you for inviting me to review this longitudinal study assessing transitions in tobacco use among male youth in Norway. I thought it was, in general, very well-written and an insightful contribution to the tobacco control literature. The authors have highlighted their main limitation, that is, the large loss to follow up between waves, but I do not believe this to be a fundamental flaw to the study. I agree with the authors that this limitation is likely to result in a more conservative estimate.

Another major drawback of the paper is that the data are over 10 years old. The authors have not
justified this strongly enough and it could limit the generalisation of the study to 2019. Is it still relevant to today's tobacco control discussions?

That said, the paper stands out from the literature given its measurement of personality traits and school functioning. Some students may simply be "predisposed" to smoking given their risk-taking behaviour baseline, and this study accounts for this somewhat (many other similar studies do not).

The paper can become technically sound with revisions, which I have appended below.

REQUESTED REVISIONS:
TITLE
- Please include the word "cohort" in the title

ABSTRACT
- Is the RR from tobacco to no tobacco necessary, given it is practically the inverse of the RRs presented earlier? Could be removed from the manuscript and main text.

INTRODUCTION
- Could the authors briefly include why snus rates started to increase in Norway from the 1990s? Is it only because of the restriction on tobacco smoking?

METHODS
- I would encourage the authors to include data on females in an appendix - many studies have published tobacco transition data on small sample sizes, and despite the likely wide confidence intervals that will ensue, the data can be used in future meta-analyses (and reduce the likelihood of publication bias)
  - The research approach is to go from ever --> current. However, other studies have gone from current --> current, and from current --> ever (although this last one makes less sense). Can the authors justify only testing ever--> current?
  - Can the authors describe the validity of the responses in both waves. I am certain that there would have been some participants claiming to have been ever smokers in Wave 1 but never smokers in Wave 2. How were these dealt with?
  - What was the incentive to participate in Wave 2, if any?
  - Could missing data be imputed in an additional analysis? Information bias from missing data is problematic in this study, and such an imputation would fit neatly in an appendix.
  - The authors have reported to mean age of participants in the methods - I suggest removing this as it is mentioned in the results (and should only be mentioned in the results)

RESULTS
- Simply speaking, did more participants go from snus-->cigarettes than cigarettes-->snus? This is a key takehome message for policymakers and could also be included in the abstract to spell out the message clearly. My understanding of the data is that 25% of snus users became smokers and 16% of smokers because snus users - indicating a net 9% increase in smoking due to snus.
  - Did the authors consider the number of cigarettes smoked at baseline and at follow up? Both
the participation and intensity of smoking are important in terms of the public health implications of snus use.

DISCUSSION
- I might have missed this but what are the policy implications of this research? Increased tobacco cessation support for snus users, prevention programmes for youth, taxation etc?

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

No

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

No

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

Quality of written English
Please indicate the quality of language in the manuscript:

Needs some language corrections before being published

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