PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (**http://bmjopen.bmj.com/site/about/resources/checklist.pdf**) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Prevalence and correlates of smoking and nicotine dependence: Results of a nationwide cross-sectional survey among Singapore residents
AUTHORS	Shahwan, Shazana; Abdin, Edimansyah; Shafie, Saleha; Chang, Sherilyn; Sambasivam, Rajeswari; Zhang, Yun-jue; Vaingankar, Janhavi; Teo, Yik Ying; Heng, Derrick; Chong, Siow Ann; Subramaniam, M

VERSION 1 – REVIEW

REVIEWER	Nain-Feng Chu NDMC-TSGH, Taiwan
REVIEW RETURNED	0-Jun-2019

GENERAL COMMENTS	This is an interested manuscript to evaluate the prevalence of cigarette smoking and nicotine dependence among Singapore population. The authors found that the prevalence of cigarette smoking between 2010 and 2016 is similar. Furthermore, the male, ethnic minority, divorced and lower education level are associated with higher risk of cigarette smoking. However, there are some points should be addressed in this manuscript. 1. The method is straight forward and similar to 2010 for easy comparison – but there is no much comparison between 2010 and 2016 for the results, such as the risk of smoking or nicotine dependence. 2. For elder population and ethnic minority (Malay and Indian) over-sampling was noted – please explain the possible reasons. 3. Table 1: it would be more informative if the subjects can be divided into smoking, ex-smoking and non-smoking. 4. Table 2: please with age-adjusting and gender-specification. 5. Tables 3and 4: please using different models after adjusting for potential confounders (such as age) and with gender-specification. 6. Table 5: I would like to see the gender-specification (for better understanding the difference between genders).

REVIEWER	Cecilie Svanes, professor MD PhD University of Bergen , Norway and Haukeland University Hospital, Norway
REVIEW RETURNED	25-Jul-2019

GENERAL COMMENTS	This is an important, useful and interesting analysis of prevalence of smoking and nicotine dependence in Singapore in 2016, and results are compared with results from a precisely similar survey undertaken in 2010. The overall prevalence is low, and not declining much over these years. The risk groups identified in the 2016 study

	are quite similar to those identified in the 2010 survey, and the authors adequately addresses this in the discussion. The study population needs to be more clearly described, i.e. how was this a household survey – are full households investigated or single individuals from selected households? The response rates should be described properly in the methods part (not just briefly mentioned in the discussion). The importance of response rate and potential selection should be discussed in some more detail. Evidently you have been able to include disadvantaged groups, but still the most disadvantaged are excluded – thus the groups who most likely smoke the most are not included? Please discuss in more detail. Why do you use nicotine dependence as an outcome? Why do you not use i.e. number of cigarettes daily? Please explain in the manuscript. The statistical methods must be more clearly described. I.e. is the household structure accounted for in the analyses? The tables should be self-explanatory – for this, the legends/footnotes should give type of study, numbers included, type of analyses that give the OR, p-values etc I suggest to list the variables in the same sequence in the abstract results and conclusion, throughout the manuscript text, and in the tables. The format of the references is a mix of numbers and author/year, please fix. The age group 18+ should be described in the abstract-methods. That the study do not include adolescents should be listed as a limitation – as bullet point and in the discussion, unless this age group never smokes in Singapore. In that case, that should be mentioned in the manuscript discussion. The abstract should also briefly state that e-cigarettes were not investigated. Please include in the introduction the reference Marcon A et al PlosOne 2018 “Trends in smoking initiation in Europe over 40 years”, I think this is highly relevant, showing increasing trends in smoking debut among the very youngest. Discussion pg 10 “It was also noteworthy that the trend of smokers being in the youngest age group was not seen in this 2016 dataset…” I cannot see that this is true – the outcomes were highest in the youngest age group and significantly lower in the oldest age groups, and the estimate were below 1.0 also in the next youngest age group – suggesting an age effect. Please explain or change this. The relation of smoking with chronic illnesses beyond mental health should be totally deleted from the article – i.e. the causal effects of smoking on respiratory and cardiovascular health are well known from studies that are much better designed for this purpose. The negative findings of the present analysis give no new knowledge, but may be a consequence of a variety of methodological challenges as suggested by the authors, and should be deleted from the paper.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name: Nain-Feng Chu
Institution and Country: NDMC-TSGH, Taiwan
Please state any competing interests or state 'None declared': none

Please leave your comments for the authors below

This is an interested manuscript to evaluate the prevalence of cigarette smoking and nicotine dependence among Singapore population. The authors found that the prevalence of cigarette smoking between 2010 and 2016 is similar. Furthermore, the male, ethnic minority, divorced and lower education level are associated with higher risk of cigarette smoking. However, there are some points should be addressed in this manuscript.

1. The method is straight forward and similar to 2010 for easy comparison – but there is no much comparison between 2010 and 2016 for the results, such as the risk of smoking or nicotine dependence.
We have added 2010 and 2016 comparisons by subgroups (age and gender) in Supplementary Table 1 and added the following line in the Discussion to summarise the similarities with the 2010 study:
"The correlates of smoking and nicotine dependence identified in the current study (i.e. the association between smoking and younger age, males, ethnic minority, lower/vocational education) are similar to the findings of the 2010 study, representing the stability and persistence of these factors."

2. For elder population and ethnic minority (Malay and Indian) over-sampling was noted – please explain the possible reasons.
Residents aged 65 and above, Malays and Indians were over sampled to ensure that an adequate sample size would be achieved to improve the reliability of estimates for the subgroup analysis.

3. Table 1: it would be more informative if the subjects can be divided into smoking, ex-smoking and non-smoking.
We have included a breakdown of the sociodemographic profile of the sample by smokers, ex-smokers and non-smokers in Table 1.

4. Table 2: please with age-adjusting and gender-specification.
Please refer to Supplementary Table 1.

5. Tables 3and 4: please using different models after adjusting for potential confounders (such as age) and with gender-specification.
The analyses presented in Tables 3 and 4 were controlled for potential confounders including age, ethnicity, marital status, education, employment and household income.
As recommended, we ran separate models for males and females. The results are largely consistent with the overall sample model. We have discussed these in the results section and the information is presented in Supplementary Tables 2a and b and Supplementary Tables 3a and b.

6. Table 5: I would like to see the gender-specification (for better understanding the difference between genders).
We thank the reviewers for the suggestion and found that female smokers who were nicotine dependent were more likely to have OCD, presented in Table 5. We have added the following in the Discussion section:

“A gender difference was found in the association between OCD and nicotine dependence where 26% of females with nicotine dependence had OCD compared to 2.5% for males. We identified three other studies that have similarly found an association between smoking and OCD only in females [30][31][32]. However, our finding differs from the vast majority of clinical studies that have shown that patients with OCD are less likely to smoke compared to the general population (e.g. [33][34]). As suggested by Wu and colleagues, this may have to do with differences between clinical and community samples and further research is needed to shed light on the association between OCD and nicotine dependence in males and females.”

Reviewer: 2
Reviewer Name: Cecilie Svanes, professor MD PhD
Institution and Country: University of Bergen, Norway and Haukeland University Hospital, Norway
Please state any competing interests or state ‘None declared’: None declared

Please leave your comments for the authors below

This is an important, useful and interesting analysis of prevalence of smoking and nicotine dependence in Singapore in 2016, and results are compared with results from a precisely similar survey undertaken in 2010. The overall prevalence is low, and not declining much over these years. The risk groups identified in the 2016 study are quite similar to those identified in the 2010 survey, and the authors adequately addresses this in the discussion.

1. The study population needs to be more clearly described, i.e. how was this a household survey – are full households investigated or single individuals from selected households?
We would like to thank the reviewer for the suggestion. We have clarified it further in our revised manuscript. ‘A total of 6126 individuals aged 18 years and above who were randomly selected from an administrative database were included in the study’.
2. The response rates should be described properly in the methods part (not just briefly mentioned in the discussion). The importance of response rate and potential selection should be discussed in some more detail. Evidently you have been able to include disadvantaged groups, but still the most disadvantaged are excluded – thus the groups who most likely smoke the most are not included? Please discuss in more detail.

We have included a description of the response rate. In it, we have described that the sample was randomly selected. We noted some systematic differences and highlighted that it is possible that the true prevalence was obscured. We have applied non-response weights to tackle this issue.

3. Why do you use nicotine dependence as an outcome? Why do you not use i.e. number of cigarettes daily? Please explain in the manuscript.

Nicotine dependence as measured by the Fagerstrom Test for Nicotine dependence was chosen as an outcome to enable direct comparisons with the 2010 study (Picco, 2012). We stated in the Measures that: “We categorised those with scores 5 and above as dependence (on the FTND) as defined by previous studies [16], including our previous study [13] to ensure consistency for comparison.” The number of cigarettes daily was an item on the FTND.

4. The statistical methods must be more clearly described. I.e. is the household structure accounted for in the analyses?

Data on household structure was not collated and not accounted for in the analysis.

5. The tables should be self-explanatory – for this, the legends/footnotes should give type of study, numbers included, type of analyses that give the OR, p-values etc I suggest to list the variables in the same sequence in the abstract results and conclusion, throughout the manuscript text, and in the tables.

We have included more details to the tables for better understanding for the reader and have revised the order of the variables according to sequence. However, in several instances, we have retained the original order for better flow in describing and discussing the results.

6. The format of the references is a mix of numbers and author/year, please fix.

We apologise for the inconsistency and have fixed the references so that it is ordered numerically in the main body of the manuscript.

7. The age group 18+ should be described in the abstract-methods. That the study do not include adolescents should be listed as a limitation – as bullet point and in the discussion, unless this age group never smokes in Singapore. In that case, that should be mentioned in the manuscript discussion.
We have included this information in the sections as suggested.
8. The abstract should also briefly state that e-cigarettes were not investigated.
We have included the following line in the abstract, “However, the study did not investigate the use of e-cigarettes.” We would however like to point out that e-cigarettes are banned in Singapore. Thus, we believe that their use would be low in the population.

9. Please include in the introduction the reference Marcon A et al PlosOne 2018 “Trends in smoking initiation in Europe over 40 years”, I think this is highly relevant, showing increasing trends in smoking debut among the very youngest.

We have included this pertinent reference in the Introduction of our revised manuscript: “An upward trend in smoking debut in early adolescence was found in a European study (Marcon et al, 2018).”
10. Discussion pg 10 “It was also noteworthy that the trend of smokers being in the youngest age group was not seen in this 2016 dataset…” I cannot see that this is true – the outcomes were highest in the youngest age group and significantly lower in the oldest age groups, and the estimate were below 1.0 also in the next youngest age group – suggesting an age effect.
Please explain or change this.

Thank you for pointing this error. We have made the revision as follows:
“An age effect was observed with the prevalence of smoking being higher in the younger age groups in spite of the combined efforts of raising the minimum age for smoking, increasing cigarette prices and smoking prevention and cessation programmes in institutes of higher learning in the recent years.”

The relation of smoking with chronic illnesses beyond mental health should be totally deleted from the article – i.e. the causal effects of smoking on respiratory and cardiovascular health are well known from studies that are much better designed for this purpose. The negative findings of the present analysis give no new knowledge, but may be a consequence of a variety of methodological challenges as suggested by the authors, and should be deleted from the paper.

We have deleted this whole paragraph as suggested.

VERSION 2 – REVIEW

REVIEWER	Cecilie Svanes University of Bergen Norway
REVIEW RETURNED	09-Sep-2019

GENERAL COMMENTS	my comments have generally been met