

## PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form ([see an example](#)) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below. Some articles will have been accepted based in part or entirely on reviews undertaken for other BMJ Group journals. These will be reproduced where possible.

### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Impacts of early smoking initiation: long-term trends of lung cancer mortality and smoking initiation from repeated cross-sectional surveys in Great Britain
<b>AUTHORS</b>	Funatogawa, Ikuko; Funatogawa, Takashi; Yano, Eiji

### VERSION 1 - REVIEW

<b>REVIEWER</b>	Guignard Romain, in charge of research and studies at the Scientific Affairs department, French Institute for Health Promotion and Health Education (INPES), France
<b>REVIEW RETURNED</b>	19-Jul-2012

<b>THE STUDY</b>	<p>In my view, the large number of indicators and surveys makes global understanding difficult. In particular, the "smoking initiation part" is not easy to read. For example, when the authors write "we estimated cumulative initiation by taking an average of the data from the birth cohorts within two years", is it a sort of smoothing? This should be specified and an example could be proposed.</p> <p>Moreover, prevalences for the 1905, 1925 and 1945 birth cohorts, obtained thanks to retrospective data with the 1984-85 Health and Lifestyle study in Great Britain and the General Household Survey in 1998-2002, are questionable because of possible memory bias, and above all because the history of consumption is not known: people could have stopped smoking in their lifetime and been back smoking later.</p> <p>The first paragraph of Methods should be detailed and would be better placed after the description of indicators.</p>
<b>RESULTS &amp; CONCLUSIONS</b>	<p>In my opinion, the conclusion would be clearer if the authors focused on a limited number of indicators and/or surveys.</p>
<b>GENERAL COMMENTS</b>	<p>This project of paper is very interesting by trying to explain the impact of smoking initiation on the lung cancer mortality with cross-sectional data.</p> <p>However, some parts are quite complex, due to the large number of indicators and surveys used.</p> <p>My main comment is about prevalences for the 1905, 1925 and 1945 birth cohorts, obtained thanks to retrospective data with the 1984-85 Health and Lifestyle study in Great Britain and the General Household Survey in 1998-2002. They are questionable because of possible memory bias, and above all because the history of consumption is not known: people could have stopped smoking in their lifetime and been back smoking later. These data are not useful for the global demonstration and could be removed, which would simplify the understanding of the paper. Besides, as suggested by</p>

	<p>the title, the focus of the paper is the influence of smoking initiation (not prevalence), so that this part of the analysis is not needed.</p> <p>In the discussion, it would be interesting to discuss the decrease of initiation in the 1920's and the increase in the 1930's. Have the authors some elements of possible explanations?</p> <p>The authors should also discuss possible memory biases in the elderly concerning the age of initiation: elder people could indeed over-estimate their age of initiation. Is it possible to check this hypothesis thanks to subsequent waves of surveys on similar birth cohorts? The authors should discuss the possible influence of such differences on their results.</p> <p>Page 11 : "At the mid 1930s cohort, initiation and prevalence increased again". The authors should specify "except prevalence in 50-59 years-old". Is there an explanation that could be discussed?</p>
--	--

<b>REVIEWER</b>	<p>Yoneatsu Osaki          Division of Environmental and Preventive Medicine, Faculty of Medicine, Tottori University</p> <p>No conflicts of interests.</p>
<b>REVIEW RETURNED</b>	31-Jul-2012

<b>GENERAL COMMENTS</b>	<p>This study covers some important content about a historical association between smoking behavior and the lung cancer of the UK. However, some improvement seems to be necessary. Because there are many studies to show trends in smoking rate and lung cancer mortality, it is considered that this study has a little impact on new findings.</p> <ol style="list-style-type: none"> <li>1. Authors should make some additional analyses. In particular, analysis adjusted by lifetime tobacco consumption considering daily tobacco consumption and smoking cessation rate is necessary to analyze whether the earlier initiation of smoking becomes a risk of lung cancer mortality. Since lifetime tobacco consumption increases with earlier initiation by longer smoking history, authors must pay some attention to observe an association between smoking and lung cancer. The person who quit smoking also has a risk of lung cancer according to the lifetime tobacco consumption before cessation.</li> <li>2. In the case of cross-sectional study, recall-bias is more likely to include in reports, in particular, the effect is more serious in this illegal minor smoking. When a social norm about the minor smoking changes with the calendar year, the analysis considering the change of the effect is required. Consideration on these problems must include in discussion.</li> <li>3. Authors should show not only a figure of results, but also make a statistical analysis on the association between smoking rate and lung cancer mortality considering the lag time.</li> <li>4. In figure 3, the change with the point of inflection is found in age-specific smoking initiation rate and lung cancer mortality in a cohort for 1920 through 1940 of the women.</li> </ol>
-------------------------	--

	<p>Whereas, in the men, the change with the point of inflection is found in smoking rate, but the change is not found, and the discussion about this difference and its possible reason is necessary.</p> <p>5. I recommend to make an additional analysis for COPD to arrive at a new finding using a similar method.</p>
--	--

### **VERSION 1 – AUTHOR RESPONSE**

Responses to the comments:

Reviewer: Guignard Romain, in charge of research and studies at the Scientific Affairs department, French Institute for Health Promotion and Health Education (INPES), France

We greatly appreciate the helpful and valuable comments. Our responses to the comments are given as follows and we attached the text file indicating the changes.

Comment 1: In my view, the large number of indicators and surveys makes global understanding difficult. In particular, the "smoking initiation part" is not easy to read. For example, when the authors write "we estimated cumulative initiation by taking an average of the data from the birth cohorts within two years", is it a sort of smoothing? This should be specified and an example could be proposed.

Response 1: We described that it is a smoothing method in the text, and we added an example. Following this comment and Comment 3, we removed the prevalence for the 1905, 1925 and 1945 birth cohorts.

Comment 2: The first paragraph of Methods should be detailed and would be better placed after the description of indicators.

Response 2: We moved the first paragraph of Methods after the description of indicators and added details. Because we removed Figure 1 following Comment 3, some sentences of the first paragraph of Methods were removed.

Comment 3: In my opinion, the conclusion would be clearer if the authors focalized on a limited number of indicators and/or surveys. This project of paper is very interesting by trying to explain the impact of smoking initiation on the lung cancer mortality with cross-sectional data. However, some parts are quite complex, due to the large number of indicators and surveys used. My main comment is about prevalences for the 1905, 1925 and 1945 birth cohorts, obtained thanks to retrospective data with the 1984-85 Health and Lifestyle study in Great Britain and the General Household Survey in 1998-2002. They are questionable because of possible memory bias, and above all because the history of consumption is not known: people could have stopped smoking in their lifetime and been back smoking later. These data are not useful for the global demonstration and could be removed, which would simplify the understanding of the paper. Besides, as suggested by the title, the focus of the paper is the influence of smoking initiation (not prevalence), so that this part of the analysis is not needed.

Response 3: Following the comment, we removed the prevalence for the 1905, 1925 and 1945 birth cohorts from the paper. Because the information other than the prevalence was included in Figure 2 and 3, we removed Figure 1.

Comment 4: In the discussion, it would be interesting to discuss the decrease of initiation in the 1920's and the increase in the 1930's. Have the authors some elements of possible explanations?

Response 4: We added discussion about possible explanations including medical papers about adverse health effects, controversy over the adverse health effects, and filtered cigarettes.

Comment 5: The authors should also discuss possible memory biases in the elderly concerning the age of initiation: elder people could indeed over-estimate their age of initiation. Is it possible to check this hypothesis thanks to subsequent waves of surveys on similar birth cohorts? The authors should discuss the possible influence of such differences on their results.

Response 5: We added discussion about possible memory biases in the elderly. There was no birth cohort they have been surveyed more than twice both at elderly and younger ages, because the age category 35-59 years-old was too wide to define birth cohorts.

Comment 6: Page 11 : "At the mid 1930s cohort, initiation and prevalence increased again". The authors should specify "except prevalence in 50-59 years-old". Is there an explanation that could be discussed?

Response 6: We described the decreased prevalence in 50-59 years-old in the text. We discussed a possible explanation including decreasing age of quitting in later cohorts.

Responses to the comments

Reviewer: Yoneatsu Osaki

Division of Environmental and Preventive Medicine, Faculty of Medicine, Tottori University

We greatly appreciate the helpful and valuable comments. Our responses to the comments are given as follows and we attached the text file indicating the changes.

Comment: This study covers some important content about a historical association between smoking behavior and the lung cancer of the UK. However, some improvement seems to be necessary. Because there are many studies to show trends in smoking rate and lung cancer mortality, it is considered that this study has a little impact on new findings.

Response: We added the discussion to make the originality and importance of our study clearer. There are studies for smoking prevalence in several countries, but data for prevalence in the late 19th and the first half of the 20th century are missing or uncertain. This means that parts of smoking prevalence are missing for the cohorts whose lung cancer mortality in later ages has been already observed. In Great Britain, the age specific prevalence before 1948 is unclear. In the surveys from 1948, several age categories for the reported prevalence were too wide to define birth cohorts. In several studies, smoking prevalence and lung cancer mortality were analysed based on statistical models. These models need strong assumptions on these missing data. In this paper, we show the age specific smoking indicators by birth cohort without the strong assumptions, especially focusing on the age of smoking initiation. There are only a few studies to report these statistics. There are fewer studies for smoking initiation, regardless of its importance.

Comment 1: Authors should make some additional analyses. In particularly, analysis adjusted by lifetime tobacco consumption considering daily tobacco consumption and smoking cessation rate is necessary to analyze whether the earlier initiation of smoking becomes a risk of lung cancer mortality. Since lifetime tobacco consumption increase with earlier initiation by longer smoking history, authors must pay some attention to observe an association smoking and lung cancer. The person who quit smoking also has a risk of lung cancer according to the lifetime tobacco consumption before cessation.

Response 1: We added consumption data in a supplemental file. Age specific smoking prevalence

which reflects cessation was provided in Figure 2 and 3 (Figure 1 and 2 in the revised paper). The other reviewer recommended removing the prevalence for the 1905, 1925 and 1945 birth cohorts. Lifetime tobacco consumption is unclear in Great Britain, as described in the response to the comment above. We added the discussion about smoking prevalence and consumption.

Comment 2: In the case of cross-sectional study, recall-bias is more likely to include reports, in particular, the effect is more serious in this illegal minor smoking. When a social norm about the minor smoking changes with the calendar year, the analysis considering the change of the effect is required. Consideration on these problems must include in discussion.

Response 2: We added the discussion about smoking by minors and about social norms, including the prohibition of the sale of tobacco to children under the age of 16 since 1908.

Comment 3: Authors should show not only a figure of results, but also make a statistical analysis on the association between smoking rate and lung cancer mortality considering the lag time.

Response 3: As described in the response to the comment above, statistical models need strong assumptions on the missing data for prevalence and consumption. In this paper, we show the age specific smoking indicators by birth cohort without strong assumptions, especially focusing on the age of smoking initiation. Please see the response to the comment above for more details.

Comment 4: In figure 3, the change with the point of inflection is found in age-specific smoking initiation rate and lung cancer mortality in a cohort for 1920 through 1940 of the women. Whereas, in the men, the change with the point of inflection is found in smoking rate, but the change is not found, and the discussion about this difference and its possible reason is necessary.

Response 4: We added the discussion about the difference and its possible reasons. The differences in prevalence among these cohorts in women were clearer than those in men, and steep decreases and subsequent unclear decreases were seen in male lung cancer too.

Comment 5: I recommend to make an additional analysis for COPD to arrive at a new finding using a similar method.

Response 5: We added COPD mortality from 1979. COPD mortality before 1979 cannot be extracted from the database. The trends are similar to those of lung cancer mortality. We added the discussion about COPD mortality.

#### **VERSION 2 – REVIEW**

<b>REVIEWER</b>	Guignard Romain, in charge of research and studies at the Scientific Affairs department, French Institute for Health Promotion and Health Education (INPES), France  No competing interests.
<b>REVIEW RETURNED</b>	05-Sep-2012

<b>REPORTING &amp; ETHICS</b>	Ethical approval not required
-------------------------------	-------------------------------