Author's response to reviews

Title: Impact of smoking and smoking cessation on overweight and obesity: Scotland-wide, cross-sectional study on 40,036 participants.

Authors:

Daniel F Mackay (daniel.mackay@glasgow.ac.uk)
Laurence Gruer (laurence.gruer@nhs.net)
Linsay Gray (l.gray@sphsu.mrc.ac.uk)
Jill P Pell (jill.pell@glasgow.ac.uk)

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Author's response to reviews:

Thank you for your comments. We have revised the manuscript accordingly. A point by point reply is attached below:

Reviewer #1

Major Compulsory Revisions

1. I can see what the authors are aiming to achieve here with their method of logistic regression to determine the odds ratio of being overweight/obese or obese only according to smoking status. However because BMI categories are not a binary variable, this presents major problems which casts doubt on their methodology.

Prior to submitting the paper we analysed BMI as a continuous variable and also as an ordinal variable using ordinal logistic regression analyses. The decision to use categorical rather than continuous data was taken because the conclusions were similar but the BMI classifications of normal, overweight and obese had clinical and lay meaning and therefore the results are more easily comprehensible to a wide audience. The decision to present the results using binary rather than ordinal logistic regression was again taken because of the difficulty of non-statisticians being able to understand the methodology being used. For your information we have uploaded, as a supplemental file, the equivalent of table 3 using ordinal logistic regression analysis rather than two binary logistic regression analyses. As you can see, it makes no difference to the results.

a. By using logistic regression the authors have been forced to make an odd classification of 'overweight' which also includes all obese individuals, they then have done a separate analysis for those who are just obese.

The comparisons of obese versus normal/overweight and overweight/obese versus normal are exactly the same as would have been made had ordinal logistic regression been used. See above plus supplemental table. However, we
agree with the referee that the labeling in the original manuscript might be unclear. This has been amended to clarify the groupings.

b. They have excluded all individuals with a BMI of less than 18 making a very large assumption that all these individuals would either have an eating disorder or be cachetic! This is not true, some healthy 16 year old males have a BMI range which begins below 17 (See paediatric BMI charts). In addition the whole analysis may be biased through this sweeping exclusion criteria. The sample is no longer representative of the population. Much better to include all BMI categories and adjust for the variables of mental health and illness that the authors make mention of More appropriate methodology would be to carry out one main analysis using ordinal regression to obtain odds ratios for being in increasingly higher BMI categories according to smoking status or use linear regression with BMI, uncategorised, as a continuous variable to obtain mean BMI according to smoking status. Interactions with age etc could then be investigated.

We did not state that “all” individuals with a BMI <18 had an eating disorder or were cachectic. Rather we stated that some would be. This is a very heterogeneous group and the results are difficult to interpret. Nonetheless, the study is representative of the general population with a BMI >18. Adjusting for mental health and illness would be flawed as there are no robust data available to us that on whether a low BMI was due to psychological or physical ill-health. We do agree with the referee’s point that it would be better to apply a different cut-off value for 16/17 year olds. In the revised manuscript, we have used Cole et al’s paper of 2000 (suggested by the other referee) to modify the cut-off values used in this age-group.

2. The authors interpret their findings as a message for young people, however the age inclusion criteria of >16 years needs to be justified, many smokers will include those younger than this.

We agree that some minors (<16 years of age) smoke, with or without parental consent. However, 16 year old people are able to leave school, gain employment, live independent lives and generally take decisions over how to live their lives. The age-group 16-24 years of age is an age-range commonly reported in tobacco control papers and this age-group is recognized by the tobacco control community as a problematic sub-group of the population in that many interventions that have been effective at reducing smoking prevalence in older age-groups have failed to reduce prevalence in this age group. This point is also contained in the conclusion.

3. The background does not mention key influential reviews in this field e.g. Klesges et al or explain how this study differs from the numerous cross sectional studies that have already been done. No mention is made of several prospective cohort studies that have already examined the association of age with weight gain in smokers and quitting smokers. The authors need to better review existing literature and explain how this study adds to it both in the background and the discussion.
As requested, we have added the Klesges review although it is now quite out of date. It is impossible to include reference to all previous studies on this topic within the allowable number of references. Therefore we did not cite of the evidence that an association exist but, rather, focused on the main issues still to be explored: whether the association is really causal or due to confounder/reverse causation, biological plausibility and possible underlying mechanisms and apparent differences within the population et age and sex.

4. In the background the authors write about weight being 'increased' but this is a cross sectional study where an increase has not been observed. Also they do not explain clearly which categories they are comparing. They write about a 'protective effect' which is ambiguous as they do not explain they are referring to weight.

Increased was used in reference to previous studies not our study. In response to the referee’s point, this section has been reworded.

Minor Essential Revisions

1. In the results section 'smoking dose and duration' should the first table referred to be table 3 and not 2?

Tables 1 and 2 presents the summary statistics by smoking status, dose and duration and then Table 3 presents the results of the model. This order follows the flow of the results text.

2. In the discussion section the authors write about 'dieting', this implies people going on a diet, but I think they mean they had no information on nutritional intake of the population. This is therefore ambiguous and needs clarifying.

3. Table 3 contains * but no footnotes.

There was a typographical error on line 321. This should have read diet not dieting and has been corrected. Unfortunately the table footnotes disappeared when the files were uploaded. They have been re-uploaded.

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

Reviewer #2

In the present manuscript authors provide results on the association between smoking behaviours and overweight and obesity. The study is conducted in Scotland using a large dataset from 6 surveys. Several major (essential) and some minor points should be considered.

No comment required

Major points

1) Authors did not specify whether weight and height were self-reported (limitation) or measured (strength). Please specify this information both in the
Abstract, and in the Methods and strengths/limitation sections.

It already states in the methods section that they were both measured by the interviewers. These were trained staff using standard operating procedures. The text has been amended to clarify this.

2) Why did authors choose to exclude by the analyses individuals with BMI<18 kg/m²? Whereas I understand the decision to exclude by the analyses “individuals with anorexia or illnesses that cause cachexia”, I do not understand the choice of BMI=18 kg/m² as the cut-off, without providing any relevant reference. I strongly suggest authors to exclude either those with BMI<18.5 kg/m² (underweight subjects; such an eligibility criteria should be also stated in the Abstract) or, better, BMI<17 kg/m² (moderate to severe thinness, preferred choice; see the International Classification of adult underweight, overweight and obesity according to BMI, available online at: http://apps.who.int/bmi/index.jsp?introPage=intro_3.html), providing the relevant reference.

Reverse causation occurs in all BMI categories. We cannot identify the individuals who have a low/high BMI because of ill-health and those who have a low/high BMI and therefore suffer ill-health. Attempting to exclude such individuals from on BMI group would be both inaccurate and introduce systematic error. However, on reflection, we agree that a cut-off of 18 is not ideal. Changing it to 18.5 would reduce the numbers in the youngest age-group and therefore reduce statistical power. We have changed it to 17 as suggested.

3) Moreover, it is to me surprising that 9,919 subjects (19.2% of the whole sample) had a BMI<18 km/m², since it is unlikely that in Scotland underweight subjects (BMI <18.5 kg/m²) are more than 5% of the overall population. This should be discussed. It is possible that most of those excluded are adolescents aged 16-17 years who have different international cut-off points for BMI categories (see Cole et al., 2007; PMID: 17591624; see also next point). These subjects are erroneously excluded.

On re-reading this sentence was misleading. Most were excluded because of missing BMI rather than BMI below the cut-off. We have revised this whole paragraph to be much clearer about how many individuals were excluded for specific reasons. The referee is correct that applying the same cut-off (BMI</#18) to all age-groups did result in a higher percentage of 16-17 year old being classified as underweight; 5% compared with 0.2% of those #18 years of age. As suggested by the referee, we have amended the cut-offs for participants aged 16 and 17 years in line with Cole’s paper which effectively means that the cut-off values are 1.0 lower in 16 year olds and 0.5 lower in 17 year olds.

4) Furthermore, adolescents (aged <18 years) should not be given the same cut-off points as adults for BMI categories. Cole and colleagues in 2000 provided cut-off points for overweight/obesity in children and adolescents (Cole et al., 2000; PMID: 10797032). Authors should either consider those cut-off points for 16-17 year old subjects, or (much better) exclude minors aged <18 years from
As stated above, we have now applied Cole et al.’s cut-off values for participants aged 16 and 17 years. We do not want to exclude them from the analyses because we would no longer have sufficient statistical power for the sub-group analyses. Also, as described in the responses to the other referee, 16-24 years is the normal age group used in tobacco control studies so we want to include the whole of this age group.

5) Tables should be self-explicative. Please clarify particularly in the headers/footnotes/title what are the acronyms, what categories represent, which is the reference category for ORs, the model used in table 3 (no footnotes are currently provided in Table 3). Tables 1 and 2 should be clarified, and possibly modified. I strongly suggest to consider for both the Tables the following template:

```
Smoking status N Never smokers Current smokers Ex smokers (total)
Overweight (%) Obesity (%) Overweight (%) Obesity (%) Overweight (%) Obesity (%)
Overall 38,223 63.0 53.8 70.6 25.9 19.8 29.4
Age (years) 16-24 3,653 35.4 37.2 33.8 10.8 12.8 12.1
25-44 Y,YYY XX.X XX.X XX.X XX.X XX.X XX.X
45-64 Y,YYY XX.X XX.X XX.X XX.X XX.X XX.X
# 65 Y,YYY XX.X XX.X XX.X XX.X XX.X XX.X
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As mentioned previously, the footnotes were somehow deleted during uploading of the files. They have been re-uploaded.

6) Figure 1 is not really informative in its present form. The x-axis should be limited to BMI=50 kg/m2. I strongly suggest to consider the modified Figure 1 as a supplement, and to replace it with the modified Table 1.

Figure 1 has been omitted from the revised manuscript.

7) The Discussion section lacks of a discussion comparing the study findings with those from the previously published scientific literature. For example, in a recent cross-sectional study from Italy, male current smokers were more frequently overweight and obese than male never smokers (Gallus et al., 2012; PMID: 22645105). Authors may consider this study and the corresponding discussion.

Reference to this paper has been added to the revised manuscript. It is impossible to include reference to all previous studies on this topic within the allowable number of references. We felt that the main merit of the study was not simply confirming an apparent association which has been shown previously in many studies, only some of which are cite but rather highlighting the observed differences by age.
Minor points

1) Please, specify in the Abstract that the surveys are representative of the Scottish population.

We have amended the text as requested.

2) In the abstract and in the main text, please, always add to the BMI values the unit of measurement (kg/m2).

We have amended the text accordingly.

3) Authors state that the questionnaire includes questions on diet (line 120), but among the study limitations of the study (Discussion section; line 326) you add the lack of information in dieting. Please, double check and clarify.

The information collected on diet was not consistent across the individual surveys and, therefore, could not be used. We have amended the manuscript so that it no longer suggests that we had access to useable data on diet.

4) Methods: please, specify the formula used to derive BMI.

This has been added to the revised manuscript.

5) Methods: in Definitions paragraph, please describe the classification of smoking duration.

This is already contained in the methods definitions section: “For current smokers, the self-reported number of cigarettes smoked each day was categorized into 1-9, 10-19 and #25. For ex-smokers, the time since quitting was categorized into <1, 1 to <2, 2-4, 5-9, 10-19 and #20 years.”

6) The last paragraph of the Results section may be omitted.

We disagree with the referee on this point since there is new information not contained elsewhere in the results. However, if the editors wish to omit this section we agree to this being done.

7) Please, specify in all Tables and Figures that overweight includes obesity or provide the BMI range for overweight and obesity

This has been done.

8) In Table 3 the description of notes * and ** are missing. The model should be described.

The footnotes were lost during uploading. They have been re-uploaded.

9) In Table 3 p-values are redundant, since statistical significance is already stated with the 95% confidence intervals. Thus, they should be deleted. I suggest to include a p-value for the trend in order to assess the existence of a
“dose-response” relationship between the number of cigarette smoked per day and overweight/obesity.

We disagree with the referee that p values are redundant. Confidence intervals are not tests of statistical significance. P values convey different information and are therefore included in addition to the confidence intervals. However, we agree with the referee that the overall p value for trend provides additional, useful information. These have been added to the revised table.

10) Authors should carefully re-read the main text in order to correct a few typos and to clarify some contents. For example:

#Line 118: “An interviewer questionnaire” ???;

# This has been changed to interviewer-administered questionnaire

Line 137, Tables 1-3: “obese” should be replaced with “obesity”;

# Obese is appropriate since the term is largely used as an adjective in the following text.

Line 140: the correct cut-point should be “#20 cigarettes” smoked per day instead of #25;

# This has been amended to 20.

Line 141; Figure 3: Please replace “1 to <2” with “1”

This has been done.