**Author's response to reviews**

**Title:** Associations between e-cigarette access and smoking and drinking behaviours in teenagers

**Authors:**

Karen Hughes (k.e.hughes@ljmu.ac.uk)  
Mark A Bellis (m.a.bellis@ljmu.ac.uk)  
Katherine A Hardcastle (k.a.hardcastle@ljmu.ac.uk)  
Phil McHale (p.mchale@ljmu.ac.uk)  
Andrew Bennett (andrew.bennett@hegroup.org.uk)  
Robin Ireland (robin.ireland@hegroup.org.uk)  
Kate Pike (k.pike@warrington.gov.uk)

**Version:** 3  
**Date:** 4 March 2015

**Author's response to reviews:** see over
Dear Dr Kristjansson

Thank you for the opportunity to revised our article. We are delighted with the positive reviews we received from the referees and are very grateful for their considered comments and suggestions for improving the article. We have detailed below how we have addressed each comment and have marked our changes to the article in red font.

We hope these revisions meet with your approval and please do not hesitate to contact us if you require any further changes.

Yours sincerely

Karen Hughes

**Referee 1**

Overall, I think this is a valuable and timely done study that address an important topic and examine an overly vulnerable age group that is being consistently targeted by the tobacco industry. The authors of this study have done a great job in conducting this research. Here are my comments intended for discretionary revisions:

- You have used the Tobacco Survey that covers schools in North West England. Is this representative for other teens in the rest of the country? How would it compare to South of England. I think the authors should discuss this issue.

We have raised this issue in the limitation section of the discussion:

“Finally, while little data are available on the regional distribution of e-cigarette use among either children or adults in England, prevalence of tobacco smoking tends to be higher in the North than in the South. [32] Thus findings should not be considered representative of all 14-17 year olds in England or the North West region.”

- The authors stated that student for whom no marker of deprivation was available were excluded from analyses (n=1,253 cases). How would this will affect the internal validity of the study? Does those 1,253 participant may have different characteristics than those selected? (The issue of selection bias). I believe this should be discussed as well.

We have also added this issue into the limitations section:

“Students that could not be assigned to a deprivation quintile were excluded from analysis and therefore represent additional potential bias in the final sample.”

- The authors stated that Hierarchical logistic regression was used to account for clustering of students within schools, with the exception of analysis of regular
smokers (Logistic regression was used) However, logistic regression was also used in the model for the association between access to e-cigarettes and alcohol drinking behavior in non-smokers. What was the justification for that? How could dependency between observations from the same school would can affect the result?

We thank the reviewer for picking this up – the analysis does actually use hierarchical logistic regression but this was not identified in the footnote so we have now corrected this.

• One of the interesting finding the this study find is that almost quarter of teenagers that had accessed e-cigarettes had also tried smoking conventional cigarettes but not liked them. The authors provided the explanation that “it is likely that flavorings make e-cigarettes an attractive option to teenagers who would otherwise be put off conventional cigarettes by their taste”. I believe that there is a room for alternative explanation, since we can't establish the temporal relation, it’s also possible that teenagers have tried conventional cigarettes first and probably didn't like the taste, then tried e-cigarettes, but perhaps, once they get hooked on nicotine they will initiate smoking again. Therefore, the reader should know that e-cigarettes would not be considered as a path away from cigarettes.

We agree with the reviewer that this may be a pathway through which e-cigarettes attract young people to tobacco products. However, we have specifically avoided discussion on the gateway potential of e-cigarettes as our study was not intended to answer these questions but rather to examine child e-cigarette access as a public health issue in its own right. While we have not incorporated any further discussion on this issue into the article, however, we feel that our wording in this section (“Although we cannot determine whether this experience occurred before or after accessing e-cigarettes....”) indicates to readers that both pathways are possible.

• What are the possibilities of information bias? Is it possible that the rates of access to e-cigarettes are even higher than what was reported, in case there was a social desirability bias in completing the survey?

Yes, this effect could work in both directions to either increase or reduce reporting of e-cigarette access and we have now included this issue in the limitations section of the discussion.

“Equally, as with all surveys of self-reported social behaviours, students’ may have under or over reported e-cigarette access, smoking and drinking behaviours due to factors including social desirability, poor recall or lack of knowledge.”

Referee 2

This is a very well-written paper assessing e-cigarette use rates and its associations with alcohol use, cigarette smoking and parental smoking among
adolescents using a large cross-sectional survey in North West England. This study is highly significant because it adds to the literature documenting youth e-cigarette use rates globally and it can inform the regulation of e-cigarettes (i.e., policies surrounding decreasing youth e-cigarette access/rates).

Minor essential revisions

1. Can the authors define “ex-smokers?” Was ex-smoking defined as smoking cigarettes previously but have now quit or was it based on the endorsement of lifetime use but denying current use? If it’s the latter the authors may consider renaming ex-smoking to experimental smoking.

Ex-smokers were identified through the response ‘I used to smoke but have given up’ and this is now reported in the methods section:

"Ex-smokers were identified through the option ‘I used to smoke but have given up.’"

2. Please consider including the sample sizes in the title of the tables.

We have now included the sample sizes for the multivariate analyses in the footnotes to the tables. The sample sizes for bivariate analyses are already provided.

3. The analyses section is missing the description of the results presented in tables 2-4

We think this may be a mistake – the findings in tables 2 to 4 are already described in the results section, lines 194 to 222.

4. The authors discuss that the rate of light smokers having tried an e-cigarette is 67%, which is a rate higher than those observed in France (33%) and South Korea (37%). I would like to point out that a survey study conducted in Fall 2013 in the USA also found that close to 60% of those who had tried e-cigarettes have also tried cigarettes (Krishnan-Sarin et al., 2014, NTR). Krishnan-Sarin et al.’s study along with the current study’s findings, conducted around similar times confirms that the co-use of rates may be rising.

We thank the reviewer for highlighting this study and have now included reference to it in our discussion:

“However, findings are consistent with other recent studies reporting high levels of e-cigarette access among tobacco smokers (e.g. USA[20])...”