Author's response to reviews

Title: The impact of televised tobacco control advertising content on campaign recall: Evidence from the International Tobacco Control (ITC) United Kingdom Survey

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Author's response to reviews: see over
Covering letter and point by point responses to reviewers

We thank the reviewers for their supportive comments and are grateful for the opportunity to respond to these comments:

Reviewer 1

Minor comments:

1.) There is work by researchers (Langleben, Wang) at the U. of Pennsylvania that warrants being cited and discussed. If I remember correctly, they have found contradictory findings based on fMRI studies. While I think the bulk of the research now indicates that graphic/emotional ads are more effective, the Langleben research is worthy of discussing.

Responses: This is a study of the effect of graphic warning labels on smokers urge to smoke in response to smoking cues. It actually finds reduced craving after strong emotional graphic warning labels so rather than being contradictory it actually provides a potential mechanism for why exposure to strong emotional images, in this case on warning labels rather than adverts, might change smoking behaviour. However, we have been careful in our paper not to over-interpret our data as showing that negative emotive adverts change behaviour and therefore suggesting this, or any other mechanism for an effect on behaviour, seems inappropriate to us.

2.) I appreciate that the authors explored non-linearities in GRPs, however, I would suggest trying the square root of GRPs as an alternative to quadratic GRPs because quadratic GRPs can lead to lower levels of awareness as GRPs increase, whereas the square root of GRPs reflect diminishing returns and a possible plateau in awareness at high levels of GRPs which I think makes more intuitive sense.

Response: We appreciate this suggestion and have now explored this alternative non-linear relationship which is also non-significant, and have reported this finding.

3.) Given the variation in the timing and delivery of GRPs for the two styles of ads, it would be good to superimpose the number of observations (with bigger and smaller bubbles representing the number of surveys) from the survey over time on the figures with GRPs to illustrate that there is no bias in the timing of surveys and GRPs. In other words, if the survey is completed at certain times of year that is closer in timing to one style of ad over another, that might boost recall. If the surveys are spread out evenly over time, there should be no issue.

Response: We have amended the plot and legend accordingly and we believe this demonstrates that there is no such bias. In addition, we made two small amendments to the text to clarify this point.

4.) The measure of recall is quite general. The results, while convincing, would be even stronger if the measures of recall were tailored to each of the ads (i.e., one question per ad and then aggregated). That said, the current measure works in that only those that are truly memorable appear to register in this general measure of awareness.

Response: We agree and have described this limitation in the Discussion section.
Reviewer 2:

Minor comments:

1. **Background – first paragraph:** The authors describe that two other studies have examined the impact of campaign content on recall among adults. Perhaps the authors should explain more about these studies and what is missing from them, so it becomes clear why another study is needed.

Response: We have amended the first paragraph of the introduction to draw out the limitations of these studies: “However, the first was based in Australia where the overwhelming majority of televised campaigns contain negative emotive content and graphic images [16] and the other was based on an internet survey rather than a representative sample of smokers [17].

2. **Methods – survey methodology:** Is it correct that respondents who reported to have successfully discontinued smoking in two consecutive surveys are no longer recontacted in ITC UK?

Response: Yes, I can confirm that this is correct.

3. **Methods – campaign exposure:** Campaigns were categorized by two researchers as having either positive or negative emotional content. It would be good to report the inter-coder reliability.

Response: We inserted the following sentence in the Methods section to clarify this point: “While there was complete concordance between the reviewers on theme, emotional content and delivery style, a third researcher resolved disagreement on the informational content of one advertisement.”

4. **Methods – campaign exposure:** It is unclear to me what the authors mean with “these figures included campaigns run by charities which were not part of our original dataset.”

Response: We have now amended these paragraphs to clarify this.

5. **References - #24:** Please check whether reference 24 is correct (title and page numbers).

Responses: Many thanks. This has now been corrected.
Reviewer 3:

Major comments:

1. Can the authors explain why they only included 'recall' as an outcome measure. As discussed above, I acknowledge that recall is important, but the ITC Cohort provides the opportunity to further explore other downstream smoking-related attitudes and quitting behaviours, such as quit attempts close to campaign exposure. I am not persuaded by the argument in the Discussion that the surveying didn’t cover the New Year period when many attempts occur. The previous analyses of the ITC data in Australia referred to in the paper had similar timing of surveys and were able to examine quitting attempts and find important effects of campaigns(1). There are 3,932 observations, so there seems like there should be enough power to examine the effects of campaign exposure (as measured by GRPs &/or ad recall) on firm plans to quit and quit attempts...?

Response: We have explored the use of these data for quit outcomes, in a similar manner to that in the Australian paper. However, the UK ITC surveys are conducted largely between October and November, and quit attempts unlike recall, are highly seasonal, as are campaigns. These data did not provide sufficient data over the year to allow us to model these seasonal factors appropriately, and in the UK we have a much better source of this monthly outcome data; the Smoking Toolkit which we are currently using for this analysis. We have amended this section to describe more accurately this issue. We have therefore focussed this paper on recall.

2. In the abstract, introduction and discussion, the authors refer to the Heath & Hyder (2005) paper that indicates recall can underestimate the effectiveness of positive emotive brand campaigns. In contrast, recognition (where the campaign message is described or shown to participants and they are asked whether they remember seeing that ad) has previously been used to examine differences in campaign “recall” between positive (how to quit) and negative (why to quit) campaigns(2, 3). The authors need to clarify that when Heath & Hyder are referring to the problems of using recall to measure the impact of positive emotion campaigns, they are referring to semi-prompted recall. Heath & Hyder actually suggest using prompted recall or recognition as a better way to assess the impact of positive emotion campaigns. The fact that Neiderdeppe and colleagues (2011) and Dunlop and colleagues(3) found higher prompted recall (ie. recognition) for the ‘negative emotion - why to quit style’ campaigns than ‘positive emotion – how to quit’ campaigns indicates that the issues Heath & Hyder raise are not relevant for these 2 studies. This should be considered in the 2nd paragraph of the Introduction and in final paragraph of the Discussion. The authors’ point in this Discussion paragraph about Heath & Hyder’s findings and that the previous Dunlop & Niederdeppe findings may be misleading needs to be re-visited in light of the information outlined above.

Response: Robert Heath is a co-author and has contributed to this aspect of the paper. Heath and Hyder’s work used recognition, crosstabulated against brand attitudes, to show that brand name prompted recall is an unreliable measure of ad effectiveness when applied to positive emotive brand advertising campaigns. They did not carry out this experiment with negative emotive campaigns but suggest that it is unlikely to work as well with negative emotive campaigns as these tend to get higher levels of recall due to attentive defence behaviour. This study should not be interpreted as showing that level of recognition is a better indicator of ad effectiveness than recall; recognition is just a very reliable way of
finding out who has and has not been exposed to an ad because recognition memory is extremely powerful and accurate (see Standing 1973).

We have amended these paragraphs slightly. Specifically in the discussion we now write: ‘Classic marketing theory assumes that high levels of recall improve advertising effectiveness and that campaign recall provides a proxy measure of effective campaign exposure [18, 19]. Heath and Hyder [20] have shown that recall can underestimate the effectiveness of positive emotive brand campaigns. High recall of negative emotive campaigns, seen in this and other studies [10, 14, 15] may also be a misleading indicator of their effectiveness.

3. The authors should also discuss the differences between previous findings about the relatively short duration of campaign effects on smoking prevalence and quitting-related behaviours[1, 4, 5] and the current findings of longer effects on campaign recall. It seems logical that campaigns will be recalled for a longer time after airing, but that campaign effects on quitting behaviours will be tied more closely to recent campaign exposure – do the authors agree?

Response: We agree. We have added the following in discussion: The length of time that campaigns were recalled contrasts with our previous findings which suggest that impacts on quitting behaviour in the UK may be limited to the immediate aftermath of the campaigns [4]. It is possible that campaigns will be recalled for a longer time after airing, but that campaign effects on quitting behaviours will be tied more closely to recent campaign exposure.

4. In the methods/sample characteristics section, can the authors please clarify the composition of the sample. One sentence states that between 1 & 4 observations were available, but in the previous sentence, individuals were included ‘...who had provided data and reported being a smoker in the previous wave of follow-up” – implying that at least 2 waves of data per person were necessary for inclusion.

Response: We have amended this section to explain that each individual had at least 2 waves of data (which include data at wave 3), and between 1 and 4 observations (from waves 4 to 7) available for analysis.

Minor issues not for publication

1. Abstract, Results, 2nd sentence – insert “likelihood of” before “recall”

Response: Done

2. Methods, Campaign Exposure, 1st para, 2nd sentence – insert “TV viewing” before “frequency”

Response: Done

3. Methods, Campaign Exposure, 2nd para, 3rd sentence – remove ‘theory-driven’ – this is already implied by stating that the coding framework was based on PRIME theory.
Response: Done