Author's response to reviews

Title: What is behind smoker support for new smokefree areas? National survey data

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Author's response to reviews: see over
Response to Reviewers Comments on: “What is behind smoker support for new smokefree areas? National survey”

These reviewer comments were most helpful – thank you.

Reviewer: Janne Scheffels

1) General comments: The aim of this study is to describe support for new smokefree areas laws in NZ, and to explore factors that may influence this support. A sample of 1376 adult smokers is surveyed, the sample taken from the NZ Health Survey. This is a study asking an interesting question that has not been explored much until now. Smokefree laws covering cars and various outdoor settings applies to settings that, compared to the areas most often restricted by smokefree laws until now such as offices and public serving places, more are part of the private sphere. When laws restricting what people can do in these places are considered, the issue of public support is crucial. The data and methods used to explore the questions asked appears sound, and the paper presents interesting findings.

Our response: Thank you for these positive comments. No specific response required.

2) However, the paper is not very easy to read. This is in my opinion mainly due to two things. First, the tables are too large, and to difficult to understand intuitively. Five tables of this size is very much, and especially if the tables are not so well edited, as these appear.

Our response: We have now removed the original Table 1 and turned it into a Figure (also suggested by the other reviewer). The former tables 2-4 have all been edited down in various ways to make them shorter and footnotes to the tables have been reduced in size.

3) Second, as the large tables illustrate, the paper could be better focused. It presents a lot of findings, but the framework of presentation is still a bit unclear. I would like to see more about why are the different variables are included, and what the associations tells us.

Our response: As detailed above, the editing down of the tables has meant that the article is more focused on the variables that relate to the hypotheses and other key variables. The Discussion Section is also now expanded to further interpret the results and the implications of the results eg, a new paragraph of generalisability (see below – re the other reviewer) and the new last paragraph of the Discussion Section. There have also been changes to some of the headings in the Results Section – to make the overall framework clearer.

4) About the tables: Table 1 is an overview of the distribution of how the sample has answered on the Smokefree Support Index. This table is not so difficult to
read, but I am not sure how necessary it is to present these data in such detail. Perhaps could this information be given in the text only instead?

Our response: This Table has now been turned into Figure 1 (as suggested by the other reviewer).

5) Table 2 is very detailed, this could also be economized down to a smaller table, showing only main tendencies. Also, I do not like that the tables comes with so extensive notes. I would, in this table and in all the others, shorten the notes considerably. One option could also be to put the information presented instead in the notes in the methods section of the text part of the paper.

Our response: We shortened the Table (now called Table 1) but cutting out some non-significant results. As suggested some of the table footnotes have been moved to the Methods Section and otherwise abbreviated.

6) Table 3 is confusing, as the system of presenting what is reference category is not the same all through it (for the two first variables the reference is presented as 1.00 in the table, for the two next it is only mentioned in parentheses, and finally for the SHS exposure and control behaviors it is a little bit of both). This table also is very large. I would suggest taking out those variables that do not inform the subject very much, and only inform in a note or in the heading that they are controlled for.

Our response: We have now standardised how the reference category is presented in this table (now called Table 2). We have also substantially shortened this table by removing less important variables.

7) Table 4 is a presentation of compared mean scores. This too is very detailed, and has a lot of text. I would cut this down considerably.

Our response: We have also cut down the size of this Table (now called Table 3). Nevertheless, it was not cut as much as the preceding table as some of the information removed in the preceding table still needs to be shown (to inform readers of the variables considered in the logistic regression in the last Table).

8) Page 3, paragraph 2: I think that the importance of this study should be better argued for. In what specific ways can it be useful to know more about smokers’ attitudes towards smokefree areas laws?

Our response: Thank you. The following text has been added:

“Understanding these attitudes is potentially important if such understanding can: (i) inform appropriate public health interventions to accelerate the spread of such new smokefree areas; (ii) improve the design of new smokefree laws so that there is adequate public and smoker acceptability and compliance; and (iii) maximise
the synergies with other tobacco control interventions (eg, mass media campaigns).”

Also, to further boost the Introduction we have added a key reference to a new IARC publication on smokefree areas [reference 1].

9) Page 3, last paragraph: I would prefer that the authors described in the text what they want to do: to examine how the support among smokers is, and explore how this support is associated with specific sociodemographic variables, attitudes and practices. We do not need to know that you are doing ‘the most detailed multivariate analysis’, we want to know what you can find out for us.

**Our response:** We have improved the wording: “In this study we aimed to examine support among smokers for new smokefree areas and to examine how this support is associated with specific sociodemographic variables, attitudes and behaviours.” The bit about the “most detailed…” has been deleted.

10) Page 5: I think that better explanations of measures and analysis is needed here, the readers needs some more instructions to understand the analysis. I would, as mentioned before avoid having so many and so large notes in the table and instead present most of this information here.

**Our response:** We have expanded the Methods Section in various ways eg, around the deprivation and financial stress measures (also suggested by the other reviewer). We have also reduced the extent of the notes under the tables. But as the Methods Section is already reasonably detailed, we also refer readers wanting further detail to an online Methods Report.

11) Page 5, the results section is short and reports the main findings in a focused way. This is easy to read and gives a good overview. However, the headings of the different sections is a bit confusing: I would not use the general term attitudes only here but describe the dependent variable more precisely: ‘Support for smokefree laws by demographic and socio-demographic characteristics’ and so on.

**Our response:** Thank you – the headings have all been modified accordingly.

12) Page 10, the discussion section, last paragraph discusses why European smokers are less supportive towards smokefree areas laws than Maori/Asian smokers. It is suggested that European smokers may be more representative of a ‘hardcore’ smoking minority than the M/A-smokers. For foreign readers, it is not intuitively understandable why it should be so. I suppose this has to do with that it is fewer smokers in the European population, and that the ‘epidemic’ as such is getting closer to the phase where only the more marginalized smokers remain. And that smoking in M/A-groups is more common, as in an earlier phase of diffusion? Both facts about the prevalence
of smoking in the different populations as well as the way of thinking around the ‘hardcore’-hypothesis will have to be presented here if this discussion is to make any sense to the reader. In relation to this, the way of writing in the next paragraph, the first one about limitations of the study, also seems strange given that this paper is to be read by an international audience (‘because smoking in this country..’). In general, the section about limitations of the study from page 10 on discusses potential problems in a balanced way, pointing to possible selection bias and measurement errors. The last paragraph about how unlikely it is that not all confounders have been controlled for is unnecessary, and in my opinion also not very convincing – I would take this out.

**Our response:** We decided to delete the point about a possible “hardcore” smoking minority given that this comment was relatively speculative and this actual concept is also disputed in the literature. We also changed the introductory wording to the “limitations” subsection – and made the discussion more internationally orientated. With regard to the discussion of confounders – we think this is important to mention (given the other reviewer’s comments) and have added to the limitations section to further give an example of how selection bias may impact on the results (see below – response to point 2 from the other reviewer).

13) Level of interest: An article of importance in its field. Quality of written English: Acceptable. Statistical review: No, the manuscript does not need to be seen by a statistician.

**Our response:** No response required.

**Reviewer:** Jonathan Samet

1) Are the overall findings cut-off dependent? Have you considered a more extreme cut-off level to define “strong support” among smokers?

**Our response:** In the Results section we state that “The linear regression and logistic regression analyses for studying “strong support” (versus “weak support”) for new smokefree areas produced very similar results”. Since the linear regression used 13 gradations in response (from 0.0 to 6.0 with 0.5 increments as per the old Table 1 – or now the new Figure 1) we are reasonably confident that the general pattern of results are not particularly dependent on the cut-off level.

2) You have acknowledged potential selection bias in the study, and have presumed that the adjustment for various socio-demographic characteristics would be sufficient. However, given that the response rate was only 56.4% of all eligible smokers, there is potential for residual bias. What is the distribution of socio-demographics between respondents and non-respondents? This comparison must be provided to gauge representativeness and the potential for bias.
Our response: We do not have detailed data on the non-respondents ie, smokers who declined to participate in the NZ Health Survey (NZHS), who declined to participate in further research after the NZHS, and who declined to participate when contacted about the ITC Project survey. Furthermore, our results have been weighted and adjusted for the complex sample design (including to adjust for over-sampling of some ethnic groups) so as to reflect the national population of smokers (based on the NZ Census which asked a question on smoking). But despite this we have acknowledged that residual selection bias is possible and to elaborate on this we had added to the text a worked example of the potential implications of this selection bias on the results.

Elsewhere [39], we have described the implications of potential selection bias among survey participants, towards smokers who are more positively inclined to tobacco control measures (ie, smokers who support smokefree policies may be more likely to take part in the NZHS and then in the ITC survey). We estimated that such selection bias would have to be reasonably large to overturn key findings. “For example, there was an observed 31.9% support for smoking in playgrounds among the estimated third of all smokers first approached for interview in the NZHS that actually participated in the ITC study (i.e., a third ≈ 32.6% = 67.9% [NZHS response rate] × 85.2% [NZHS consent to ITC follow up] × 56.4% [successful ITC Project survey re-contact rate]). This would have to be offset by an unobserved 58.8% support for smoking in playgrounds among the two-thirds of eligible NZHS survey smokers not included in the ITC Project study for the “true” support to be 50% [ie, majority support of]. Whilst not impossible, it seems unlikely that this unobserved support might be 58.8% among non-participants compared to 31.9% among participants.”

3) You should comment on the generalizability of these findings to other countries.

Our response: Thanks for this suggestion – we have added the following to the end of the first subsection of the Discussion Section.

“Our main results may be reasonably generalisable to other developed countries which, like New Zealand, are at the tail end of the tobacco epidemic and already have in place relatively advanced smokefree environment policies. But generalisability may be much less for countries where adult smoking prevalence is high (25%+), where indoor smokefree policies are minimal or not enforced, and where there are widespread attitudes involving disrespect for the law and government authority (eg, see work by Lazuras et al [43]). Nevertheless, we note that considerable attitude support for smokefree policies can occur in less-developed country settings [44, 45].”

4) Abstract-Results: Why are the reported aORs selected from different models? Report from the same model for consistency.
Our response: We have modified the abstract to only show the aORs from the full-adjusted model. We have also amended the relevant part of the results section to maintain this consistent approach.

5) Page 3, last paragraph: “We also aimed to undertake the most detailed multivariate analyses to date…” A useless claim, should delete sentence.

Our response: This has been deleted, thanks.

6) Page 9, Discussion, paragraph 1: “…many individuals supported some, but opposed other, types of smokefree area, i.e., fairly nuanced situation-specific attitudes.” It would be good to present the distribution of the responses to the 6 questions.

Our response: We have the specific results to the questions published in a much shorter and simpler descriptive paper [39]. Nevertheless, we added to the end of the first paragraph of the Discussion Section the following:

“For example, only 3.0% thought smoking should be allowed in cars with pre-school children in them, while 82.6% thought that it should be allowed in some of the outdoor seating areas of pubs (with these and other setting-specific results published elsewhere [39]).”

7) Page 11, Research and policy implications: “Our results suggest that improving knowledge of the SHS hazard might be a mechanism to raise smoker support…” Your data show that the majority of smokers are already knowledgeable of the harms of SHS (from Table 3, ranging from 77% to 80% for ‘Beliefs around SHS hazards’ even among weak supporters). Therefore your suggestion is not very convincing.

Our response: Our wording has been changed (start of the last paragraph of the Discussion Section) to be more cautious and to relate the findings with the “health belief model” (see below). We also amended the Conclusion Section and the Conclusion part of the Abstract accordingly.

“For example, only 3.0% thought smoking should be allowed in cars with pre-school children in them, while 82.6% thought that it should be allowed in some of the outdoor seating areas of pubs (with these and other setting-specific results published elsewhere [39]).”

8) Table 1 could be better presented as a cumulative frequency plot.

Our response: We have turned this Table into Figure 1 – thank you.
9) Table 2: “Small area deprivation level was based on a New Zealand specific small area deprivation index.” An international audience might not be familiar with this index, so further clarification or reference would be helpful.

Our response: We have added extra detail to the Methods Section (fifth paragraph) – see below. In the subsequent sentences we have also added further details on the financial stress measures and an online reference that describes how these measures relate to each other.

“In particular, deprivation level was based on a New Zealand-specific deprivation index for small areas (NZDep2006) [34].”

10) Table 4: There is no indication of adjustment for age, sex and study design here. Are there adjustments included as in Tables 2 and 3?

Our response: We have now made it clearer that these results were not-adjusted (adding the word “crude” to the table header).

11) Level of interest: An article whose findings are important to those with closely related research interests. Quality of written English: Acceptable. Statistical review: No, the manuscript does not need to be seen by a statistician.

Our response: No response required.

Final comment

We thank the reviewers for their helpful comments that have helped us to improve the manuscript. We also thank the journal for the opportunity to re-submit the manuscript.