Author's response to reviews

Title: Before and after study of bar workers' perceptions of the impact of smoke-free workplace legislation in the Republic of Ireland

Authors:

Lisa Pursell (lisa.pursell@nuigalway.ie)
Shane Allwright (slwrght@tcd.ie)
Diarmaid O'Donovan (diarmuid.odonovan@nuigalway.ie)
Gillian Paul (g paul@tcd.ie)
Alan Kelly (akelly@tcd.ie)
Bernie J Mullally (b.mullally@ucc.ie)
Maureen D'Eath (maureen.death@nuigalway.ie)

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Author's response to reviews: see over
Thank you very much for the opportunity to review the peer review process of this manuscript. After reading the manuscript, the reviewers' comments and the authors' responses, it is my opinion that the authors have solved the key problems raised by the reviewers in the first round of reviews. Even, I should say, one of the concerns of "Reviewer 1" (representativity of sample) is not of key-importance for the overall validity of the paper (I agree!!) – anyway, it’s OK to have clarified it in the paper.

Regarding the critics by “Reviewer 3”, I believe that the point has been correctly addressed by the authors in their response and the changes introduced in the paper. More precisely, the last paragraph of the Results section clearly show this is a question of agreement or opinion, and it is extensively elaborated in the Discussion section additionally showing that the belief of bar workers’ is not related to the belief of negative impact on business. Although I understand the concerns of “Reviewer 3” I fully agree with the authors that further economic analysis is out of the scope of this paper and that the results presented are enough clear. Misuse of published results is always a danger, even when results are clearly presented – and not only in this sensitive topic. Although we may think “the opinions of bar workers’ on economic impact are not right” we cannot skip them. Deleting this from the paper would be a clear example of bad practice – and of publication bias.

We are very pleased (and relieved!) that Dr Fernandez’ view coincides with our concerns over maintaining the scientific validity of our findings.

I should say, however, that the results used to clarify this point can be a bit confusing: I should maintain the paragraph added (page 5, 5th paragraph) but would clarify, or clarify the last 2 columns in table 3. The footnotes for the last two columns of T3 have now been changed and the text in paragraphs 5 & 6 p. 5 has been re-worded to refer to changes in odds (also see below).

Moreover, in the Methods section there is nothing about these special analyses that can have in turn a problem. The authors refer in the text and Table 3 to odds ratios (OR) whereas they have explained in the Methods section that a Poisson regression version of GEE was used (which provides RRs). This was an oversight, missed after transcribing methodology from our first paper from this study. The Methods should say:

“To model predictors of support for the legislation and change over time a logistic regression version of generalised estimation equations (GEE) was used to give odds ratios (Stata 9, StataCorp, College Station, TX).”

If these estimates are actually these OR I assume they are derived from a logistic regression model (which also takes into account the pre-post nature of the data)

Are the figures ORs or RRs?

ORs were used throughout.

The authors must be aware that OR are an overestimation of the magnitude of the association when the prevalence of the outcome investigated is greater than 20% --thus explaining the high values for the OR observed. They should clarify this point and use a more proper measure of association for this situation (ie, RR)

All results from Table 3 refer to ORs. We accept that these ORs would give poor estimates of RR
given the high prevalence/proportions that they are based on, but we are not attempting to estimate RR here (apologies for the error in the wording in the Methods which may have implied this). OR is of itself a valid estimate of the odds of association albeit that in this situation ORs will give values that would be considerably larger than the equivalent RRs.

We have now re-worded paras 5 & 6, page 5 to further emphasise that T3 estimates ORs by referring to, for example, “the odds of support for the legislation increased two to three fold” (instead of “support for the legislation increased two to three-fold post-implementation”)

In summary, I believe that the paper is of importance and no more changes regarding the economic topic is needed (I should recommend to quickly address my doubt about the use of OR). It is a very important paper that will have a high impact on policy makers, researchers and advocates of tobacco control.

Declaration of competing interests:
I am a researcher involved in several research projects for tobacco control and prevention. Currently, I am the Principal Investigator of a study on Spanish bar workers to evaluate the impact of the Spanish 28/2005 Law for tobacco control and prevention (including a ban on workplaces) with a similar design as the Irish study (Allwright et al. BMJ and the present paper).