Author's response to reviews

Title: The influence of in-pregnancy smoking cessation programs on partner quitting and women's social support mobilization. A randomized controlled trial.

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Author's response to reviews: see over
Response to Tim Lancaster
1. Secondary outcome should have been published with the primary outcome. In the sense that is meant by Dr Lancaster, we cannot justify separating the primary and secondary outcomes. However, it is done for a very practical reason. The main outcome paper was published in Tobacco Control, and already the length of the primary outcome paper is somewhat longer than their maximum normal length. There were numerous secondary outcomes, of which these are only part. The reason these outcomes were assessed is because it is possible that the intervention might affect these outcomes. Also, the authors of the relevant Cochrane review suggest that investigators should record these kinds of outcomes. The process of explaining how the trial intervention could affect all the primary and secondary outcomes, how they were measured, and displaying the results and commenting on them requires either a very long paper, which would be unpublishable in the current journals, or several papers (essentially four in total, though one is quite theoretical). We are playing a game, although the rules of the game are set by the journal editors and publishers. My own view (also as an editor) is that this does not constitute what we would deem duplicate publication. This publication certainly references the main outcome paper, and as the others are now accepted, I have ensured that it references the others.

Response to Laurence Moore
1. The outcomes were not specified in the trial protocol
This is true, but the truth is that the trial protocol was written by someone who was not a researcher at the time. I can assure the editor and Laurence that we did specify well before the data analysis that we were going to examine this outcome, inspired by the Cochrane review which suggested that we should.

2. The trial was not designed primarily to affect social functioning and partner quitting. This is true, but Lumley (the author of the Cochrane review) and Oliver, a commentator, make the point that smoking cessation interventions in pregnancy in particular could (and in their view are likely to) have inadvertent social effects and that smoking cessation trials in pregnancy should seek to measure these. Our trial is the first to do so, or at least report such data. Additionally, we describe how the TTM intervention might have had an effect on social functioning, in particular.

3. The trial had limited power to detect such effects
I agree whole heartedly with respect to partner quitting, which was a rare outcome. I have added such comments into the Discussion. However, the power to detect changes in ISSB scores was good. If you look at the width of the confidence intervals in relation to the SD, you will see that the power to detect changes in social support captured by the ISSB is very good.

4. The main weakness (that it is a post hoc analysis) should be more clearly recognised.
We accept the thrust of Laurence’s point, but do not regard this as a post hoc analysis-it was pre-planned even if not written in the protocol. Nevertheless, we have
emphasised this more in the summary of the results at the end of the Discussion and elsewhere.

5. Make more of the recommendation to involve partners in pregnancy smoking cessation interventions. This has been emphasised more clearly, including in the Abstract and in the Conclusion at the end of the paper.

Thank you for spotting these. I have amended them.

7. Discretionary amendments
All suggested changes have been made.