Reviewer's report

Title: Evaluation of a bespoke training to increase uptake by midwifery teams of NICE Guidance for membrane sweeping to reduce induction of labour: a stepped wedge cluster randomised design

Version: 0 Date: 14 Dec 2016

Reviewer: Erik Cobo

Reviewer’s report:

I read the trial paper "Evaluation of a bespoke training to increase uptake by midwifery teams of NICE Guidance for membrane sweeping to reduce induction of labour: a stepped wedge cluster randomised design", and I think the design is very interesting but, unfortunately, I don't feel comfortable enough with your statistical results to recommend publication in Trials.

Major consideration.

Your adjusted analysis provides extreme values too far from direct unadjusted estimates. For your main outcome, you report proportions of 0.444 and 0.468, leading to a ratio of 0.95, far from you adjusted estimate of 0.90. Due to non-collapsibility, generalized linear models provide values for the adjusted estimators of the unit-level effect that are higher to the unadjusted population averaged estimation. I am not an expert in this random Poisson modeling, but my 'feeling' is that the differences between both estimates are too big, in special for, P11 L13, the average number of sweeps per woman, where an unadjusted ratio of 0.95 (0.660 over 0.701) becomes a significant adjusted ratio of 0.71. How can we merge adjusted unit effects of 0.71 to become a pooled unadjusted population effect of 0.95? If I interpret correctly, your results table 2 provides different non-significant estimates for this variable (unadjusted 0.627/0.603=0.96 vs adjusted 0.83). On this table, emergency CS shows an almost complete tie (13.2 vs 13.1) but an adjusted ratio of 0.89.

If those results are correct, I wonder if such modeling can provide useful and interpretable results. [In that case, I would suggest a more technical publication directed to one statistical journal.]

I'm more in favor of some programming or copying error (despite the replication with different software by different authors). In any case, I'll be happy to listen an explication for those discrepancies. In that case, please be also prepare to help readers to interpret such results.

Other considerations
Please, discuss the discordance between the levels of instrumental delivery (the true desired outcome) and sweep, both major in the treated arm. Is this results against the NICE background to recommend sweep?

Please, extend your Consort table to include the items from the extension to cluster designs. And adapt your paper accordingly. For example, adapt table 1 to include the cluster data, adapting it to the temporal, paired nature of the SWD.

Please, report proportions following Consort advice. (i.e., X out of Y, Z%).

Please, define what you exactly mean by pseudo-anonymized.

Please, clarify in sample size and analysis if your alpha level is two-sided.

**Level of interest**
Please indicate how interesting you found the manuscript:

- An article of importance in its field

**Quality of written English**
Please indicate the quality of language in the manuscript:

- Acceptable

**Quality of figures**
All images and figures within the manuscript should be genuine i.e. without evidence of manipulation. No specific feature within an image may be enhanced, obscured, moved, removed, or introduced. If you have concerns about the veracity of the figures you should choose the first option below.

**Statistical review**
Is it essential that this manuscript is seen by an expert statistician? If so, please give your reasons in your report.

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