Reviewer’s report

Title: The effects of a multi-disciplinary, minimal smoking intervention among pregnant women and their partners: A real-life controlled intervention study

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Reviewer: Wolfgang Hannöver

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Reviewer’s report on manuscript no. 2775 624561711231; Oien, Storro, Jenssen & Johnsen: The effects of a multi-disciplinary, minimal smoking intervention among pregnant women and their partners: A real-life controlled intervention study submitted for publication to BMC Public Health as a research article.

The authors report a study from Trondheim, a specially selected region in Norway to lower smoking rates in pregnancy. The manuscript focuses on the effects of a training intervention following the 5 A guidelines that was administered to three groups of health professionals: midwives, public health nurses, general practitioners (GP) called health workers. The intervention consisted of a three hours training and written material supplied four times. The authors investigate the effects of this intervention by comparing two cohorts: the first being assessed between September 2000 and June 2002 (serving as controls) and the second being assessed between June 2002 and December 2004. The investigation addressed all pregnant women who presented with a midwife or a GP for pregnancy care, or with a maternity health center after delivery in control group. In the intervention group, all women were invited to the study without detailed information as to how the women’s data were assessed. The authors found a difference in smoking status between women in the control and intervention group at onset of pregnancy (61% vs. 70%) and different quit rates during pregnancy (OR, 1.4 [1.1 1.8]). They also found that smoking rates of partners differed between intervention and control groups at onset of pregnancy, during pregnancy and six weeks post-partum. The also found differing smoking rates compared with other regions in Norway. They conclude that a minimal intervention delivered repeatedly throughout pregnancy by GPs and midwifes have contributed to a decline in smoking in
pregnancy, mostly as a local campaign.

The manuscript provides a deeper insight into the development of smoking habits in Pregnancy in Norway and presents large datasets, gathered under naturalistic circumstances. The second strength of the manuscript lies in the comparison between Trondheim as a specially selected region with regard to smoking cessation in pregnancy approaches and other regions in Norway of similar size and sociodemographic constitution. The manuscript offers a vast information on smoking habits in pregnancy and will well contribute to knowledge in the field.

However the manuscript in its present form does not speak in favour of its publication as a research article in BMC public health due to conceptual and methodological issues that are addressed in detail hereafter:

1. The question posed by the authors is not well defined. The scope of the paper is to evaluate the effect of the introduction of the 5A into routine health care for pregnant women and new mothers in Trondheim. No clear outcome parameters have been defined, neither in terms of smoking behaviour, habits with regard to indoor-smoking and not with regard to actual counselling behaviour of the trained health workers. For criteria see for example the works of: Hughes et al. 2003 or West et al. 2005 Comparisons with other regions appear in the course of the manuscript.

2. The methods have not been sufficiently described. We do not get a clear flow of participants through the study. No information is given about who addressed the participants in which context. Also we do not know how health workers where trained, and how training was evaluated by the trainees, or how the training was implemented into routine care.

3. The data are not usable to answer the question of effectiveness of the training of health workers according to 5A. In order to estimate an effect, we would need to compare data against a control group. The group used here to contrast effects against is a natural cohort and differences between groups may as well be attributed to societal changes. Also the groups differ with regard to smoking behaviour before treatment was administered. Attrition rates are high in all groups and we do not receive any information on differential attribution at all.

4. The manuscript addresses the question of effectiveness and thus should adhere to standards for the reporting of trials. Yet the authors
adhere to the STROBE, which essentially reflects the scope and ends of the study much better. In essence the manuscript deals with observational data over time with changes in smoking policy and training of health workers at markedly distinct time points. From my point of view, this is the real strength of this paper that becomes overcast by the effort to report effectiveness data.

5. The discussion is soundly formulated but for the data at hand lengthy. The conclusions again pertain to the results of an efficacy trial, which the study in essence is not, and thus do not correspond to the data presented.

6. Judging from the discussion, the authors are well aware of the shortcomings of their approach but do their best to uphold the notion to report efficacy trial results.

7. The research on the cited literature is flawless. The authors take great pains to give a thorough and sound overview over literature. A number of epidemiological studies on smoking in pregnancy are not referred to, e.g. the PRAMS studies, the seminal work by Fingerhut and colleagues or epidemiological data presented by Kahn and Collagues. Also more conceptual papers as e.g. Windsor et al. 1993 or Velicer et al. 1992, or McBride et al 2003 are not cited. Also studies with a similar outlook as from Valanis et al. 2001, Tappin et al. 2005, Lawrence et al. 2003 are not cited. But, as has been said, the authors cover the field from their perspective (which is rooted in allergology as I take it) well.

8. Title and abstract do not correspond to the contents of the paper since they suggest results from an efficacy trial.

9. Not being of native English tongue it is hard for me to estimate the quality of the writing, but would suggest â## as I subject myself to, when submitting manuscripts to international journals â## to have the manuscript proof-read by a native speaker.

In conclusion I congratulate the authors to their study and would very much like to see their wealth of data published. If guidance is wanted, Iâ##d suggest certain points for improvement:

1. Conceptualization of the manuscript as a true observational study â## as it essentially is.

2. Omit the tempting interpretation of results as effects of an
intervention

3. Describe clearly how health workers were trained; give insights into the training sessions and ramifications.

4. Give an account on how the training was evaluated by the trainees.

5. Describe clearly ways of recruitment for health workers as well as for participants. Who approached them, why did they participate, what happened to those not in the study, clear-cut inclusion and exclusion criteria if applicable for both health workers and participants.

I wish the authors my best in rewriting their manuscript and for a successful future submission.

As can be seen I see it necessary to perform a complete revision of the manuscript before acceptance. This may be seen as compulsory revision for this manuscript.

Decision is: unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Quality of written English:
As stated in the review, should be edited by a native speaker.

Statistical review:
No, does not be seen by a statistician

Competing interests:
I declare that I have no competing interests.

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:
I declare that I have no competing interests.