Reviewer's report

Title: An investigation of the smoking behaviours of parents before, during and after the birth of their children in Taiwan

Version: 3 Date: 2 October 2007

Reviewer: Gerald Hoff

Reviewer's report:

General

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

1. Table 1 is totally irrelevant and should be dropped. The authors stated from where the data was derived. In the text the general categories of information could be stated such as number of children, marital status, etc, but using only those categories with direct bearing on the question the authors are attempting to answer.

2. Table 2 could be condensed by combining at least the information on the 4th, 5th, and 6th child as the numbers drop off considerably. I would suggest that a summary statement be added to the bottom of the table as it would facilitate some the comments found in the text portion of the manuscript which talk about overall results. On page 7 of the text there is no mention of statistical significance of the pregnancy-smoking variances for the mothers. Using the “N” for each child and the smoking status during pregnancy to back-calculate numbers, there appears to be no significant differences across 1st, 2nd, and 4th + children, and only a minimal significance with the 3rd child. Yet there is no information in the text or table that informs the reader of this.

Also, in Table 2, I do not understand how the overall smoking prevalence for the fathers can be lower than any of the other values for fathers. It should be somewhere between the lowest and highest values given, like the mothers overall rate. It really would enhance the table to show the actual numbers in addition to the percentages.

3. Table 3 illustrates my concern about the manner in which the author’s are using the terms “first” and “second” pregnancy. Are they discussing a woman’s true first and second pregnancies, or do they mean two consecutive pregnancies during the study period. If the reader is to interpret “first child” (table 2) = “first pregnancy” (table 3), then there is problem with the “N” value. From table 2, back-calculation yields 116 women who smoked during that pregnancy and 146 who smoked prior to that pregnancy. Therefore, you have only 30 women who exhibited pregnancy-smoking cessation during their first pregnancy. Yet, table 3
shows an “N” value of 129 not smoking during the first pregnancy. So it would appear that the authors are discussing consecutive pregnancies. If that is correct, then the use of “first” and “second” pregnancies in tables 3 & 4 and the corresponding text should be replaced with some other terminology.

The precision of the terminology is a concern because other publications have looked at changes in pregnancy-smoking behavior between a woman’s actual “first” and “second” births. For example, Hoff GL et al (Am J Health Behav 2007;31(6):583-590) found that the prevalence of pregnancy-smoking increased significantly between 1st and 2nd pregnancies despite a 25% cessation rate during the initial pregnancy. Therefore, the present authors need to be clear when talking about consecutive pregnancies are not restricted to the actual first and second births.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

1. Under Statistical Analyses, the last line states that the definition of socioeconomic indicator is the appendix. Yet, there is no appendix. Therefore, it is not clear to me how fathers and mothers can be in different social classes if married (page 7 first full sentence) or why different reference groups are used in Tables 5 & 6.

Discretionary Revisions (which the author can choose to ignore)

1. Tables 3 & 4 should be constructed as matched value contingency tables. That would still result in two separate tables (cessation & relapse) but would consolidate the data on each topic.

2. Tables 5 & 6 should be reduced to a single table and omit all the information related to residential region and residential district since it not discussed. Again, I would like to see actual numbers included in the data.

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

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.
Declaration of competing interests:

I declare that I have no competing interests.