Author's response to reviews

Title: Utilization of antenatal ultrasound scan and implications for caesarean section: a cross-sectional study in rural Eastern China

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Author's response to reviews: see over
Dear Editor,

We send the revised article “Utilization of antenatal ultrasound scan and implications for caesarean section: a cross-sectional study in rural Eastern China”. We have revised the manuscript according to the second round comments sent by the reviewers on 27 Sep 2011.

We are grateful to the reviewers for the very useful and constructive comments. We hope that we have addressed the concerns raised and that we are getting closer to an acceptable manuscript.

Below are the comments (in italics) made by the reviewers and our responses to these comments. All the authors have contributed to the revised manuscript.

Reviewer: Xing Lin Feng

Reviewer's report:

Re-Review report for manuscript entitled “Utilization of antenatal ultrasound scan and implications for the caesarean section: a cross-sectional study in rural Eastern China” submitted to BMC Health Services Research

Thank you very much to give me an opportunity to re-review the revised paper. I have major compulsory revision comments for the authors as follows.

1. Since the main objective of this research is to demonstrate the positive association between uptake of antenatal ultrasound scan and utilization of Caesarean section. Structural equation models (SEM) are not appropriate. The usual approach in the literature is to use reduced form regression (which is referred as “traditional regression model” in the authors’ response to the first reviewer). By such an approach, the analyzer can step-wisely insert independent variables to check whether the association between the dependent variable (CS here) and the main explanatory variable (number of ultrasound scans here) is confounded. I do agree that the strength of SEM comparing to is to show the interplay of different determinants of the dependent variable. But the prerequisite condition for such an approach is that the causal relationship is TRUE. The validity of SEM is not guaranteed if the causal relationships assumed by the SEM are spurious, which is definitely the case in this manuscript as demonstrated in my 3rd comment and the authors’ response in the first review. Therefore I strongly suggest the authors to revise their statistical analysis. Since the key explanatory variable is number of ultrasound scans, its distribution merits carefully analysis. The normal approach in the public health literature is to stratify the variable into several groups and report the distribution. Then both the crude and adjusted association between such stratified variables and the dependent variable (CS) should be reported. Normally for statistical reviewers, the step-wise findings are indispensable.
Re: Many thanks for the reviewer’s suggestion. We have now adopted multivariate logistic
regression models, by using the stepwise method Forward: LR instead of structural equation model when exploring the association between antenatal ultrasound scans and CS. Uptake of CS is regarded as dependent variables and antenatal ultrasound scan is introduced as the main independent variable. Other variables were stratified into two groups: socio-demographic variables (including maternal and husband education level, maternal age and family annual income) and clinical variables (including antenatal care, pregnant complications, previous adverse pregnant outcomes and primiparity). The two groups of variables were inserted into the model respectively and simultaneously. Crude and adjusted odds ratio of antenatal ultrasound scans were observed to understand the independent association between ultrasound scan and CS. Methods, Results and Discussion section have been revised accordingly (P6, P7, P10 and Table 3).

2. The tables merit improvements in several ways.
(1) Table 1, normally in the public health literature, a stratification of each explanatory variable is performed. The standard deviation is not required but p values for the associations are normally presented.
Re: The table has been revised based on reviewer’s suggestion. P17.

(2) Table 2 can be replaced by reporting the associations between CS and various explanatory variables.
Re: The table has been revised based on reviewer’s suggestion. Univariate logistic regression analysis of variables associated with CS was conducted. Uptake of CS was regarded as dependent variable, and other variables as well as antenatal ultrasound scans were introduced into the model as independent variable one by one. Crude ORs, OR 95% CI and p values for each association were presented. P18.

(3) Table 3 can be replaced by a stepwise regression table.
Re: The table has been revised based on reviewer’s suggestion. Multivariate logistic regression models were adopted to explore the association between antenatal ultrasound scans and uptake of CS. Variables with p values over 0.1 in univariate logistic regression model were excluded from the multivariate logistic regression analysis. Except for antenatal ultrasound scans, the remaining variables were stratified into two groups: socio-demographic variables (including maternal and husband education level, maternal age and family annual income) and clinical variables (including antenatal care, pregnant complications, previous adverse pregnant outcomes and primiparity). In the original model, ultrasound scan was inserted as the only independent variable. Socio-demographic variables were then added into the first model and clinical variables were added into the second model. In the last model, both socio-demographic and clinical variables were simultaneously introduced into the model as independent variables. Crude OR and adjusted ORs of antenatal ultrasound scans were observed. The multivariate logistic regression analysis was performed with the method Forward: LR and the statistical significance was set at the alpha = 0.05 level. P19.
Reviewer: Tine Gammeltoft

Reviewer's report:

Major compulsory revisions

I have to say that I am not very impressed with the revision of this manuscript. In my comments to the authors I wrote that this paper fails to provide evidence for the claim that there is a causal relationship between ultrasound use and c-sections. The authors reply that the aim of the paper 'is not to clarify the causality between ultrasound scan and CS'. Nevertheless, in the revised version of the paper, causality is still explicitly assumed (e.g., the abstract says that ultrasound scans 'directly caused an increasing likelihood of CS', p 10 says that 'ultrasound scan has a direct effect on having CS'). The authors must make it more clear that what they find are associations, not causal relations. In the discussion they may then discuss how we may account for these associations – but this will basically be guesswork, as this research offers us evidence for the associations only, not how to account for them.

Re: Thanks for the reviewer’s comments. We feel so sorry about the inappropriate expression the reviewer pointed out in the paper. We fully understand that the findings presented in the manuscript are associations, nor causal relations. We have revised these expressions according to reviewer’s comments.

In the discussion section, we have tried to explain the associations through other literatures review and our practical experiences (P9. the second part: Association between frequent antenatal ultrasound scans and high use of CS). Indeed, it’s only guesswork, for we are only able to obtain the association and can’t get the direct reason under the association from the quantitative data.

Minor essential revisions

I also asked the authors about the causes of the rapid increase in rates of CS in China, but I still don’t think that is properly accounted for in the paper.

Re: We have added more information in the third part of discussion section (P10, Other factors related to high CS rate). The causes are more focused on other political, cultural aspects and factors related with quality of maternal health care. It’s difficult for us to answer how much this increase can be attributed to problems identified through ultrasounds because there are few studies with specific objective to examine the attribution of antenatal ultrasound to CS. Only two related articles were found respectively to assess the association between circulor of umbilical cord by ultrasound examination and CS (Ref. No 31, study sample: 200 infants with circulor of umbilical cord identified by antenatal ultrasound scan and diagnosed after delivery) and between ultrasonographic prediction of fetal weight and caesarean section (Ref. No 32, study sample: a total of 196 cases with singleton and vertex presentation, who are grouped according to their ultrasound predictive result, as macrosomic group 46 cases and normal group 150 cases). These two studies are both hospital-based. There is no population-based data to describe the association between antenatal ultrasound scan and CS rate. So we are trying to report this community-based study and the association between community-based high use of antenatal ultrasound scan and CS.

Another question I raised concerned what women themselves said about their motivations for
ultrasound use. The authors discuss this, but they tend to conflate women’s ‘motivations’ and the socio-economic patterning of their ultrasound use (eg on p. 10: ‘women’s reasons for antenatal ultrasound are influenced by their socio-demographic characteristics’). It is important that ‘qualitative’ and ‘quantitative’ insights are distinguished from each other in accounting for women’s use of this technology.

Re: We agree with the reviewer’s comments and have distinguished the qualitative insights from quantitative findings accordingly. In the discussion section (first part: Possible causes of high rate of antenatal ultrasound scans), the first two paragraphs are the views from health providers, the third and forth paragraph are from health user’s viewpoints. In the third paragraph, we have listed some qualitative findings by other researchers about women’s perceptions on antenatal ultrasound scans. In the forth paragraph, although it’s a pity that we have not adopted a qualitative design, we are trying to describe the socio-economic and clinic patterning of women’s ultrasound use. It’s also exactly what we have found from quantitative study.

I also suggested to the authors to focus the discussion section more directly on the findings of this study. They claim to have reorganized this section, but it seems quite similar to the original one.

Re: Based on the 1st and 2nd version of the reviewer’s comments, we have re-organized the discussion section into three parts:

1. Possible causes of high rate of antenatal ultrasound scans;
2. Association between frequent antenatal ultrasound scans and high use of CS;
3. Other factors related to high CS rate

Finally, the discussion of safety issues (in response to the other reviewer) is very long and not very clear.

Re: We have shortened and reorganized this part (the third and forth paragraph of the introduction section).