Reviewer's report

Title: Does a referral from home to hospital affect satisfaction with childbirth? A cross-national comparison

Version: 1 Date: 16 March 2007

Reviewer: Josephine M M Green

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General

Answers to assessment criteria:
1. Is the question posed by the authors new and well defined?
The question is an interesting one. The maternity care experiences of Dutch and Belgian women are surprisingly under-researched.

2. Are the methods appropriate and well described, and are sufficient details provided to replicate the work?
I have some small concerns about the Methods (see below) but the use of self-completion questionnaires with a standardised satisfaction measure is appropriate. The authors may wish to reassure readers with regard to language and translation issues: I presume that the translation was into Dutch for the Dutch women, but what language is spoken in the relevant part of Belgium? If it is not Dutch, what steps were taken to ensure that the two translations of the Satisfaction measure were really equivalent?

3. Are the data sound and well controlled?
I have had some difficulty understanding group membership and exclusions, so it is difficult to answer this question.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
Statistical test results are needed in places (see below)

5. Are the discussion and conclusions well balanced and adequately supported by the data?
Broadly yes (see below)

6. Do the title and abstract accurately convey what has been found?
The ambiguity with regard to the word ‘referral’ makes it impossible to answer this question and suggest that a title that defines this term more specifically might be preferable.

7. Is the writing acceptable?
Generally yes, although the English is not always quite right (e.g. use of the word ‘qualitative’). Also re ‘preference’ – see below. Most of my comments are in fact regarding a need for clarification and it is not clear to what extent this is a linguistic issue.

Referees page-by-page comments comments – most of these are requests for clarification but fall into the category of Major Compulsory Revisions, unless otherwise indicated because the quality of the paper cannot be judged without these clarifications

1. p3. The discrepancy theory of satisfaction is only one theory. It implies that lowering women’s expectations will result in greater satisfaction if expectations are then met, and yet there is little evidence to support this. In most cases, what needs to change are not the expectations but the experiences. (No action necessary unless the authors choose to).

2. p4. ‘Where would you like to give birth?’ – to ask this question in English would be an enquiry about ideal or preference, and would not yield that same answer as ‘where are you expecting to give birth?’ for women who have complications. This is quite critical to interpretation of the study. In the same way on p6 in the Descriptives there are references to what women ‘preferred’ but what a woman prefers will not necessarily be what is planned. I presume that this is just a linguistic issue. This must be clarified.

3. p4. The definition of the 3 groups is critical. Women who planned to give birth at home and did so and women who planned to give birth in hospital and did so are unproblematic, but the third group is less so because the change of plan (the referral) could take place at different stages, specifically antenatally or
intrapartum, which are very different scenarios. Please make it clearer which you mean. Within the antenatal referrals there could logically be those whose referral had already happened when they filled in the 30 week questionnaire and those who were referred subsequently. Since later on we are told that certain women were dropped from the analysis if their actual place of birth differed from that planned, then logically the referral meant here must be ante partum and before completion of the questionnaire (I think!). The Discussion (p8) says that it is a weakness of the study that it cannot distinguish between intrapartum referrals and those in the last 8-10 weeks of pregnancy, but this still doesn’t quite clarify things for me because I do not know whether the classification of women into groups is based only on their postnatal report.

4. p4. ‘Control variables’: Please clarify the definition of ‘medical intervention’ especially in the context of ‘method of delivery’ with/without intervention.

5. p5. Comparability of samples: in Belgium it was necessary to contact many more midwifery practices (21 vs. 6) in order to reach the desired number of home births. This probably introduced more heterogeneity into the Belgian sample. There were also differences in the response procedures for Dutch women having home births (response posted direct to the researchers cf via their caregiver, albeit in a sealed envelope). This could have resulted in the Dutch women having home births feeling more free to be critical. These should be mentioned in the limitations of the study.

6. p5. If women completed the questionnaires anonymously, how were the antenatal and postnatal questionnaires linked? Was there in fact no linkage? Please clarify.

7. p5: the fact that not all questionnaires were distributed does not necessarily mean that the authors estimations are too conservative. It might mean that midwives and doctors were being selective about who they gave them too (which is what usually happens). I would therefore recommend that this possibility be mentioned (optional).

8. p6 (para 1) I think that I must have misunderstood what was said earlier about short stay being coded as hospital etc since I do not understand why women were dropped from the analysis if their actual place of birth was different from that planned. I had thought that that was the point of interest. This needs to be clarified and justified along with point 3 above.

9. p6/ Table 1: Please cite tests of statistical significance for the differences in the descriptives between the Dutch and Belgian samples. Similarly for satisfaction etc later on.

10. p6. “Primary care was attended by 87 (34.3%) Belgian women; 167 (65.7%) preferred secondary care (Table 2). Home birth was preferred by 176 (63.5%) Dutch women and 101 (36.5%) preferred to be taken care of in hospital.” Please be clearer about terminology: is “Primary care was attended by” meant to mean the same as “Home birth was preferred by”?

11. p6/7 (Table 3). I am somewhat confused by the ‘linear regression’ – but that might be my problem rather than the authors. Nonetheless, it might be prudent to have a statistician check that the appropriate type of regression has been used – the parameters cited are those that I would normally associate with a simple linear regression with a continuous variable, rather than a multiple regression with categorical variables. The value of B is not telling me very much, I’m afraid. It would be helpful to present some unadjusted means as well so that we know what we are looking at. Action: clearer presentation of the regression analysis and more information about the type of regression and why.

12. p7 I understand that ‘home’ in the regression model is shorthand for (wanted to give birth at home and did + wanted to give birth at home but was referred) vs. (planned hospital birth) and that ‘referral’ is shorthand for (wanted to give birth at home and did) vs. (planned hospital birth + wanted to give birth at home but was referred). If this is correct then the para starting ‘Regarding place of birth…’ is very misleading because what is being compared is the preferred place of birth not the actual place of birth. This para goes on to make a specific comparison between only two groups (planned hospital vs. referred) and I do not see how that would be from my understanding of the variables described. Please clarify.

13. p9. Discussion. Para beginning “The Reference group theory….” We are not justified in assuming that home is seen as desirable just because it is the norm. As the authors themselves point out in the introduction, Dutch women are not free to choose specialist care. Home therefore is what the system gives them; hospital is something special. Given that there has been so little research asking Dutch women for their feelings about place of birth (in itself quite remarkable), we should take nothing for granted. I do, nonetheless, find the following para convincing. I just do not think that the argument needs to incorporate
this particular assumption. (No action needed unless the authors choose to).

14. p10. Final para. “Two-edged swords” abound. The alternative is mediocrity. I don’t think that this cliché is helpful here. I would support the conclusion though: we need to understand more about referral processes in the Netherlands and how they are experienced by women (and perhaps by midwives). (No action needed unless the authors choose to).

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

as above

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

15. Ref#6 is the same as ref#2

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Discretionary Revisions (which the author can choose to ignore)

as above

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: Needs some language corrections before being published

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:

I declare that I have no competing interests