Author's response to reviews

Title: Women's preferences for inpatient and outpatient priming for labour induction: A discrete choice experiment

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Author's response to reviews: see over
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Prof Marie-Pierre Gagnon  
Section Editor  
BMC Health Services Research  

Dear Professor Gagnon,  

Please find enclosed a response to reviewers comments for our manuscript: Women’s preferences for inpatient and outpatient priming for labour induction: A discrete choice experiment (MS: 2119686693117879). We have addressed the Reviewers’ comments on the following page, and provided a response to each. We have also attached a revised manuscript as requested, with changes to text marked.  

I look forward to your final decision on our manuscript in due course  

Yours Sincerely,  

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Reviewer 1

1. The methods seem sound, although the response rates for the volunteers and the trial participants were very different; while the demographic characteristics of the whole sample are set out in a table, it would be helpful to know how this sample compares with the usual clinic population at the study hospital (eg graduates, household income), and particularly whether there were any differences (demographic or obstetric) between respondents and non-respondents for the trial sample.

We have added some additional explanation in the results section. Unfortunately much of the sociodemographic information requested by the reviewer for the usual clinic population is not routinely collected by hospitals; in addition a direct comparison to all inductions is not appropriate as the vast majority (>80%) are performed for medical reasons other than healthy overdue pregnancies, as was the inclusion criteria in the trial. We have highlighted the similarities of the DCE respondents to the participants of the OPRA study, and this has been added on page 9. We have also added a comment that the demographic profile was similar to all women who were being induced for prolonged pregnancy at the hospitals (page 9).

2. I was surprised that some demographic characteristics which may have an impact on choices were not reported. Did all of the women have access to their own transport? Were they all living with a partner? Outpatient care may be preferred by those with more resources (other findings seem to suggest this is the case). If the authors collected other demographic data it would be good to see it.

Eligibility criteria for the trial also included living within a 45 minute drive of the hospital and having a designated person who would drive them to the hospital. Additional explanation has been added on Page 5

3. I was interested that those in, or following their first pregnancy, and those that had no experience of induction were much more likely to prefer outpatient care. These findings were only discussed briefly… The finding though that those that had no experience of induction were more likely to prefer home care was slightly troubling. The fact that those that know the least about induction were more likely to choose an option that includes poorer access to emergency care may weaken the overall conclusions – this finding certainly needs more discussion.

The role of a discrete choice experiment is to elicit preferences for alternative care options on the basis of trade-offs between factors, precisely because there are multiple factors of value in the delivery of care, and the extent to which they are important to women will vary. We can’t assume that a woman with no experience is going to hold the same valuation as the reviewer’s interpretation. Some women may have a similar response as the reviewer, if for example, experience of induction causes the woman to want closer access to emergency care because
they feel at risk. In this instance, the data suggests that other benefits of outpatient care are more highly valued by those with no experience of induction.

Reviewer 2

Major Compulsory Revisions

1. “A multinomial logit (MNL) model with a panel specification” (p.7): I think further clarification is needed here. While there is a variant of MNL (random parameter or “mixed” MNL) which can address the panel dimension of data, Table 4 and related discussion do not provide information that this variant is what the authors have estimated.

   We have amended this section to be clearer. It now reads as follows, page 7-8: “The data set comprised 25 discrete choice questions of three alternatives for each respondent. A multinomial logit (MNL) model was used to analyse preferences. This modelling approach follows good research practice for DCEs [15,16,18] and is consistent with other discrete choice analyses in antenatal care[19-22].” We have also specified the utility functions to make it clearer. (page 8)

2. “To achieve the most parsimonious model possible, without compromising model fit, each variable that was non-significant was removed and the model re-estimated. Model fit parameters, and Log Likelihood, were assessed after each re-specification.” (pp.7-8) where the model fit parameters seem to be “the likelihood ratio test statistic for the global test of zero model coefficients, the McFadden’s pseudo R-squared and Akaike’s information criterion.” (p.7): This summary of specification search appears incomplete. On one hand, quite a few coefficients in Table 4 are statistically insignificant at any conventional level. On the other hand, it would not be possible to base model selection on all those fit measures simultaneously. For instance, when some variables are dropped, the pseudo R-squared always declines whereas the AIC may go up or go down, regardless of whether dropping them leads to a statistically significant decline in the Log Likelihood.

   All program attributes are included in the model specification. If an attribute does not influence women’s preferences for the model of care, then from a policy perspective, knowing this is also a valuable piece of information. Therefore non-significant attributes were only removed from utility specification when their continued inclusion resulted in a significant decrease in model fit. While the final model was selected on the basis of AIC, the decision to include or exclude a factor was based on significant changes in the Log Likelihood ratios of models with and without the factor.

   Additional explanation has been provided on page 8. “Model fit parameters, and Log Likelihood, were assessed after each re-specification; non-significant attributes were removed from utility specifications when their continued inclusion resulted in a significant change in the Log Likelihood. The final model was selected on the basis of AIC after testing a number of different model specifications.”
3. The authors state that “categorical variables were effects coded.” (p.8) They report both the beta coefficients as well as the exponential functions of those coefficients. The latter transformed coefficients are interpreted as the effects on the odds ratios. The following comments I make may be wrong because I’m not familiar with effects coding. Etc.

**The calculation and interpretation of odds ratios as it is currently expressed is correct. Odds ratios are relatively scaled OR, and are interpreted, as explained in the text, as the relative odds of preferring outpatient or inpatient priming compared to the fixed basic care option with a particular level of an attribute instead of the base level of that attribute.**

4. The comment 3 also applies to Table 5. The footnoted formulas are exactly the same as what they would have been with the use of dummy coding. With the use of effects coding, the numerators may need to be suitably revised to capture the willingness to trade off the frequency and duration of travel for a change in a particular attribute from the base level to another target level.

**As with the interpretation of odds ratios above, the calculation of these trade-offs is correct as specified.**

5. The authors’ DCE is an unlabelled choice experiment wherein the only information that differentiates the 3 presented alternatives is their program characteristics. I have 3 related questions about the model specification. First, why have the alternative-specific constants (ASCs) been included? I’m thinking that except when it is assumed that the patient has realised option A always mimics outpatient priming and option B always mimics enhanced inpatient priming, it is difficult to associate the ASCs with average unexplained tendencies to choose those priming services over basic inpatient priming.

**As explained in the methods section page 6, this is exactly the way the DCE has been designed and presented. Option A always represents an outpatient option, Option B always represents an enhanced inpatient option and option C always represents a fixed basic inpatient care scenario. Therefore the inclusion of ASCs in the utility functions is appropriate, and represents the underlying preference for a particular model of care, regardless of attribute levels.**

Second, why have all coefficients been specified as alternative-specific coefficients? I find it difficult to see why the patient’s tastes for generic attributes like travel time and familiarity with the midwife would change depending on whether the option is labelled A or B.

**As explained above, the utility functions have been specified as alternative specific functions for enhanced inpatient or outpatient care relative to basic inpatient care – the reviewer appears to have made an assumption that the relative importance of various attributes in preferences for inpatient or outpatient care are the same. However, we can’t assume that they will be the same. The DCE is modelled in such a way that we can assess whether the importance and tradeoffs that women are**
prepared to make between attributes are the same for outpatient and enhanced inpatient care. In this instance, the data suggests that these attributes are not necessarily valued the same when considering preferences for outpatient and enhanced inpatient care.

Third, why only the ASCs are assumed to vary with patient characteristics? Given the DCE design, I find it more natural to think that the patient’s choice behaviour is driven by her preferences for the underlying program characteristics, instead of the alphabetical labels. I’m thinking that a more natural way to enter the patient characteristics into the utility function is to let the coefficients, each of which represents the patient’s taste for variation in a particular program characteristic, change with the patient’s observed characteristics.

Interactions between attributes and sociodemographic characteristics were examined before deciding on a final model specification, and there were no significant interactions. However, some sociodemographic characteristics contributed to the underlying preferences of women for outpatient or enhanced inpatient care, over basic care.

6. “Our results suggest that outpatient priming was preferred over either enhanced inpatient priming or basic care” seems unwarranted as an unqualified conclusion. Given the results in Table 4, whether outpatient priming is the most likely choice will depend on the characteristics of this option and enhanced priming, as well as patient characteristics.

We have modified this sentence on page 14 as follows: “Our results suggest that for the typical patient in our study, outpatient priming was slightly preferred over either enhanced inpatient priming or basic care. In our context, while the clinical outcomes[23] and the costs[33] were very similar between inpatient and outpatient priming, preferences varied according to the characteristics of the services on offer and the sociodemographic background of the woman, suggesting that a one size fits all approach to priming may not be appropriate.”

Minor essential revisions
7. In Table 2, a circle needs to be added to Choose Option A

Option A has a box with a tick in it displayed, but it appears this did not display correctly in the reviewers PDF file. A different symbol has now been used

8. In Table 4, the lower and upper bounds of CI are always displayed as positive numbers even when in fact they are negative.

The confidence intervals presented in table 4 are for the odds ratios (as they are in the column adjacent to OR) therefore will always be positive
Discretionary Revisions

9. I’m thinking that “overall value of alternative priming options” can be easier to interpret if the index numbers are transformed into choice probabilities. **We have elected to keep these as ‘utility’ values**

10. Throughout this paper, the results are often summarised as: (a) the patient is more likely to choose Option A (or B) over option C when a particular program characteristic of A (or B) is present, and (b) the patient with a specific personal characteristic is more likely to choose A (or B) over C. I’m thinking that the issue of “more likely” than what or whom can be better clarified. In the present context, “more likely” may mean two different things: (i) A (or B) is less likely to be chosen than C without the relevant program or patient characteristic which increases the odds in favour of A (or B) (ii) A (or B) is already more likely to be chosen than C and the program or patient characteristic in question makes it even more likely to be chosen than C.

**We feel we have clarified the language in the paper and trust this will address this issue. The DCE approach is all based on understanding of what is more likely to be chosen than not, so we are quite comfortable with using this language.**

**Additional clarifications made**

We have added an additional detail in the methods section to provide some clarification around the methods of DCEs, page 6: “The choices selected by an individual result from then weighing up the difference in attribute levels presented of each choice”.

We have also added the following sentence on page 6 under the heading attributes and levels: “The alternatives are described by this set of attributes, some of which have generic or common levels across outpatient and enhanced inpatient options (travel time and how well the midwife is known) and other which have alternative specific attribute levels (all other attributes) that feature key differences across the alternatives’