Author's response to reviews

Title: Barriers To Women's Participation In Inter-Conceptional Care: A Cross-sectional Analysis

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Author's response to reviews: see over
We wish to thank the reviewers for their very insightful comments. Attached is the revised version. Specific responses to reviewers’ comments are below:

1) I think the additions made to the paper were very useful. However, I am not sure I was clear about my point regarding intervention intensity (and smoking). My point is that intervention intensity—defined as the total number of sessions prescribed to a client—should be controlled for in the modelling. This could be represented by one continuous variable (i.e., number of appointments assigned). I think this is important because, after addressing “all known barriers” to care, it may be that the only thing that matters for care uptake is the risk profile of the client, as reflected by the total number of sessions prescribed. Based on the definition of intervention intensity provided above, I had also wondered if smoking was a proxy for intervention intensity b/c smokers had more risk factors and, thus, were assigned more sessions. It follows that smokers maybe be less likely to attend all the sessions because they have more sessions to begin with. One way to test this possibility is to control for intervention intensity as described above.

Authors’ response:
We did not find that number of “required” visits correlates with intensity. For example, we found the following mean number of scheduled appointments by participation rate (not in tables):

<table>
<thead>
<tr>
<th>Participation Rate</th>
<th>All</th>
<th>Non smokers</th>
<th>Smokers</th>
</tr>
</thead>
<tbody>
<tr>
<td>Attended No visits</td>
<td>2.27</td>
<td>2.18</td>
<td>2.72</td>
</tr>
<tr>
<td>Attended some visits</td>
<td>8.95</td>
<td>7.23</td>
<td>10.53</td>
</tr>
<tr>
<td>Attended most visits</td>
<td>9.77</td>
<td>9.96</td>
<td>9.45</td>
</tr>
<tr>
<td>Attended all visits</td>
<td>2.58</td>
<td>2.53</td>
<td>2.77</td>
</tr>
</tbody>
</table>

First, there is not a linear relationship between scheduled visits and participation. The highest number of scheduled appointments appears in the group that attended Most of the scheduled visits and the women who attended no visits have the lowest mean number of scheduled appointments. Second, while there are differences in the mean number of scheduled visits between smokers and non-smokers in the “Some visits” category, it is not significantly different in the other participation categories. We think that the major difference between smokers and non smokers is in their demographic characteristics which describe a social context that creates additional vulnerabilities which are not measured by this model. In fact, in another qualitative analysis that we are submitting for publication, we assessed for factors that may influence participation and identify several social contextual issues that likely predict care participation and which are not measured.
in this model. In fact, this was our initial hypothesis—that we were not able to measure the factors that really mattered, particularly for African American women.

Thirdly, intensity is not defined as number of scheduled visits, but as invasiveness of the intervention. For example, a woman who was scheduled for a first appointment for smoking may have missed and rescheduled this same appointment 3 times. It appears as if she was required to make more visits than another woman, but only because she missed the prior appointments. There may reasonably be another subject who was a smoker, who has only 1 scheduled appointment and because she attended, does not require more. In other words, the more missed appointments, the more a subject would have scheduled.

2) I am also not sure the authors have adequately addressed the issue of the selection of categories for the outcome variable. I realize they choose the categories "for conceptual understanding", but did the authors test alternative versions of the categories (e.g., by combining the middle two categories)? Due to the fact that only one variable was found to be significant (smoking) in the entire model, the authors could consider using fewer categories for the sake of parsimony. At a very minimal, the authors should acknowledge that their (seemingly arbitrary) categorization of a count-based outcome may be a limitation to their study.

Author response:

We think that any categorization would be “arbitrary”. We chose the one that made some conceptual sense for interpretation. Aggregating the 2 middle categories did not make conceptual sense to us because there is too a large difference between attending 1% visit vs. 99% of visits, all of which would be grouped in the same category. We did test aggregation of the highest 2 categories. This makes some conceptual sense. However, we did not find any differences in the significance of smoking and the model was essentially unchanged. No other individual variable was significant. We will acknowledge that the categorization may pose some limitations.

3) It is still unclear which version of the Andersen model is being used: the original (i.e., Andersen's behavioral model of health service use) or the revised model (i.e., Gelberg-Andersen Behavioral Model for Vulnerable Populations to Health Services)? Both are cited. Also, the model used should to be labelled consistently throughout.

Author response:

We used the Gelberg-Andersen Behavioral Model for Vulnerable Populations to Health Services. We shorten the name throughout the text to “Andersen Model”. We will correct the references in the text to the model to make it consistent and add an abbreviation in the abbreviation key.

4) At the end of the dependent variable section the authors say that " these values
were recoded into 4 categories for descriptive analyses”. In fact, the values were recoded for all the analyses. Some justification for recoding should be provided.

Author response:
The values were aggregated for analysis into the 4 stated categories. We modified the test to indicate that all analyses used this categorization.

5) The authors report that "Only Enabling factors as a group (income, social support, neighborhood safety and competing needs) offered significant predictive power (p=.058). (Table 3)"; however, the p-value is only of borderline significance - this fact should be emphasized. Indeed, the p-value for predisposing factors is also of marginal significance (p=.08).

Author response:
We edited to make note of this

6) Editing issues
Author response:

The cited editing issues have been addressed.