Author's response to reviews

Title: Male gender preference, female gender disadvantage as risk factors for psychological morbidity in Pakistani women of childbearing age - a life course perspective

Authors:

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Version: 3 Date: 3 August 2011

Author's response to reviews: see over
Reviewer's report

Title: Male gender preference, female gender disadvantage as risk factors for psychological morbidity in Pakistani women of childbearing age - a life course perspective

Version: 2 Date: 31 May 2011
Reviewer: Robert Meadows
Reviewer's report:
Discretionary Revisions

1) Whilst this is a very well written paper, it is never entirely clear what the research question/hypothesis is/are. In the abstract the hypothesis is stated as "among women or childbearing age, gender disadvantage is an independent risk factor for PM'. Throughout the paper we also get "to test the model that gender disadvantage is a life course phenomenon" & "identify independent associations between socio-demographic, socio-economic and gender disadvantage variables". I would encourage the authors to specify the research question/hypothesis more clearly at the end of the introduction.

2) Related to point 1) - it is never entirely clear why the authors developed a pathway model, then a SEM model, then used ordinal regression. There is usually a close link between the development of the pathway model and the movement to the SEM (latent) model (for example, a little more than simply "next we developed a structural..."). The ordinal model also seems a little 'too much'. I would suggest more information on the development of pathway/SEM and remove the ordinal from the paper.

We accept that the research questions and hypotheses should have been described more clearly, and have now attempted to do so by editing and reordering the final paragraph of the introduction, and by providing additional information, earlier on (i.e. in the introduction rather than the methods section) on the a priori theoretical model of gender disadvantage as an evolving life course phenomenon, which is what we aimed to test using path analysis. In this way, hopefully, the links between the path analysis and the measurement part of the structural equation model are made more evident, and the overall rationale is clearer.

We can see the reviewer’s point regarding the ordinal regression model. However, we do think that this analysis makes an important point, namely that the main risk factors for male gender preference among women seem to be their own experience of gender disadvantage. As far as we are aware this is an original finding. While in principle this could be removed and written up in another paper, we feel that it is quite relevant to the themes of this paper. This apparent trans-generational transmission of gender disadvantage suggests that the cultural phenomenon is to some extent mutually reinforcing and self-propogating, underlining the need for sustained political will and pro- gender equity policies and programmes across government and civil
society. We have therefore tried to integrate this analysis better within the structure of the current paper, by introducing the analysis in the background section, and by underlining the above points in the discussion.

3) I would be interested to know how the areas were allocated to interviewers (one per area? possible interviewer influence? etc)

The four female interviewers all worked across all catchment areas. It is unlikely therefore that interviewer effects could have accounted for observed differences between catchment areas, which are furthermore plausible given the very different character of the high and low SES catchment areas. We cannot exclude interviewer effects completely, we did make every effort in selection of the interviewers (Behavioural Science University graduates), training and quality control monitoring during the project to keep these to a minimum.

4) I very much appreciate the fact that the authors clearly identify the cut-off for a satisfactory TLI etc. However, I would encourage them to reflect a little more on the fact that model(s) run did come out slightly lower. Page 15 (limitations) makes no mention.

We agree that this is an oversight. The model fit is good in terms of RMSEA but slightly sub-optimal for TLI. We have added in the following sentences in the section on the discussion pertaining to study limitations

“The path analysis and, to a lesser extent, the structural equation models assumed, implicitly, a temporal ordering of causal relationships; the overall model fit for the structural equation models was adequate to good in terms of the RMSEA, but slightly sub-optimal (<0.90) in terms of the TLI.

4) is the marital satisfaction figure in table 1 correct? -0.7(8.5)

Yes, this figure is correct

Level of interest: An article of importance in its field
Quality of written English: Acceptable
Statistical review: Yes, and I have assessed the statistics in my report.
Declaration of competing interests

Reviewer's report
Title: Male gender preference, female gender disadvantage as risk factors for psychological morbidity in Pakistani women of childbearing age - a life course perspective
Version: 2 Date: 17 April 2011
Reviewer: Navpreet Dhillon
Reviewer's report:

Major compulsory revisions

- Gender disadvantage questionnaire on pages 7 and 8 should be included as a table with reference to it in the text.

I am afraid that we disagree with this suggestion. We feel that this would interrupt the reader’s flow and use up more space than our current approach, which is to provide the full question in the text.

- Ideally married and unmarried women should be analysed as separate groups as different factors for both these groups limiting factor as affects the results obtained as factors to assess gender disadvantage are different the sample.

We agree in principle with this point, but this was complicated by a) the fact that not all indicators could be used in the sub-sample of single women and b) the number of single women was small in relation to the number of married women.

We did in fact analyse married women (n=304) as a separate group, since two of the key indicators in our ‘evolving life course’ model of gender disadvantage, age at marriage and marital satisfaction could, evidently, only be assessed in married women. We then went on to conduct a sensitivity analysis in which we reassessed our models in the whole sample (n=525), but omitting these two indicators of gender disadvantage. The results, as described in the paper, were reassuringly similar. We did consider the possibility of carrying out a fully stratified analyses as the reviewer suggests, that is running the models in the sub-sample of unmarried women as well as married women, but since there were only 221 single women we felt that the resulting parameter estimates would lack precision, in addition to the main exposure being less well specified, and for these reasons preferred the approach we used in the submitted paper. For the reviewer’s further reassurance, we provide a table below with the standardized coefficients for the most important effects for those who were married and for the whole sample (already provided in Table 2) and now also for those who were single. As can be seen, the parameters for those who are single are very similar to those for the whole sample. We conclude from this that the differences, such as they are, between those who are married and the whole sample arise from the necessarily restricted SEM applied to the whole sample and the subset of single people (omitting age at marriage and marital satisfaction) rather than from any important differences in patterns of association between single and married women. The substantial independent effect of gender disadvantage is apparent in both marital status strata as well as in the whole sample. A note to this effect is now included in the results section.

<table>
<thead>
<tr>
<th>Standardised Coefficients from structural equation model</th>
</tr>
</thead>
<tbody>
<tr>
<td>From to Gender disadvantage Psychological morbidity</td>
</tr>
<tr>
<td>Married 0.61 Single 1.54 Whole sample 1.46</td>
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<tr>
<td></td>
</tr>
<tr>
<td>-----------------------------</td>
</tr>
<tr>
<td>Socioeconomic status</td>
</tr>
<tr>
<td>Negative marital satisfaction</td>
</tr>
<tr>
<td>Autonomy</td>
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<tr>
<td>Life events</td>
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<tr>
<td>Social support</td>
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<tr>
<td>Correlation between</td>
</tr>
<tr>
<td>Gender disadvantage</td>
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<td></td>
</tr>
</tbody>
</table>

-In the conclusion—interventions are not directly related to the results of this study and perhaps would be better to completely omit this section

On reflection, we agree with this suggestion, and have deleted most of this text, restricting ourselves to some more general observations regarding policy level and targeted interventions

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**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

Additional material submitted by the reviewers

Referee 1:
http://www.biomedcentral.com/manuscript/review/attachment/pdf/4933966435418901.pdf

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