

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Interactions between microfinance programs and non-economic empowerment of women associated with intimate partner violence in Bangladesh
AUTHORS	Dalal, Assoc. Prof. Koustuv; Dahlstrom, Orjan; Timpka, Toomas

VERSION 1 - REVIEW

REVIEWER	Dr Animesh Biswas, MBBS, MPH, MSc (Sweden), PhD Fellow Team Leader (Reproductive and Child Health) CIPRB
REVIEW RETURNED	13-Apr-2013

GENERAL COMMENTS	The study is well illustrated the association with micro finance and IPV. Bangladesh has experienced limited number of study in IPV area. However, micro finance already shown a remarkable program to empower community by development. However, people are experiencing a numbers of events related to micro finance at household level. Certainly, policy makers and organization deals with micro finance will have scope to use this data in action, specially on social accountability. But it also requires to know whether there is other associated factors which also involve to occur IPV along with micro finance. The study also have scope to suggest to do do longitudinal study on IPV in Bangladesh. Thanks to authors to bring forward messages.
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REVIEWER	Angela Taft PhD Associate Professor/Acting Director Mother and Child Health Research, La Trobe University Melbourne, VIC, Australia
REVIEW RETURNED	19-Apr-2013

GENERAL COMMENTS	<p>this is an important area to be studied with profound implications for women in low income countries who are using micro-financing. Your findings, if accurate are important. I have indicated that the design is adequate, but I your data analytic techniques are not well enough described for readers to be confident of how you derived your results.</p> <p>1. Why was marital status excluded as a socio-demographic variable - some women may have been widowed or separated and it is always important?</p> <p>2. Your reporting of the DHS processes was thorough and the description of key questions eliciting the major exposure and</p>
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	<p>outcome variables. However your description of your analytic methods were not. Were your analyses only limited to contingency tables?? If so, why? Please describe better how you analysed the data. What methods did you use to analyse interactions? How did you generate ORs and CIs and control for the relative effects of SES and your variables of interest?</p> <p>3. 'Patients' may not be equally representative, because less than half respondents responded. Whilst the socio-demographic chosen variables were similar (probably), there may be differences not investigated, such as greater severity or threat of violence which prevented disclosure among non-responders and greater than half did not respond. I am not sufficiently familiar with Bangladeshi household composition to suggest other explanations.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer: Dr Animesh Biswas, MBBS,
MPH, MSc (Sweden), PhD Fellow
Team Leader (Reproductive and Child Health) CIPRB
House- B162, Road-23, New DOHS, Mohakali, Dhaka

The study is well illustrated the association with micro finance and IPV. Bangladesh has experienced limited number of study in IPV area. However, micro finance already shown a remarkable program to empower community by development. However, people are experiencing a numbers of events related to micro finance at household level. Certainly, policy makers and organization deals with micro finance will have scope to use this data in action, specially on social accountability. But it also requires to know whether there is other associated factors which also involve to occur IPV along with micro finance. The study also have scope to suggest to do do longitudinal study on IPV in Bangladesh.

Thanks to authors to bring forward messages.

Authors' response:

Thank you!

Reviewer: Angela Taft PhD
Associate Professor/Acting Director
Mother and Child Health Research, La Trobe University Melbourne, VIC,
Australia

Dear authors - this is an important area to be studied with profound implications for women in low income countries who are using micro-financing. Your findings, if accurate are important. I have indicated that the design is adequate, but I your data analytic techniques are not well enough described for readers to be confident of how you derived your results.

1. Why was marital status excluded as a socio-demographic variable - some women may have been widowed or separated and it is always important?

Authors' response:

We appreciate the comment and have included marital status as a socio-demographic variable and have extended Tables 1-3 accordingly. As a result, we had to use a more conservative limit for significance (due to more comparisons to correct for in the Bonferroni correction), and Tables have also been updated according to this.

2. Your reporting of the DHS processes was thorough and the description of key questions eliciting the major exposure and outcome variables. However your description of your analytic methods were not. Were your analyses only limited to contingency tables?? If so, why? Please describe better how you analysed the data. What methods did you use to analyse interactions? How did you generate ORs and CIs and control for the relative effects of SES and your variables of interest?

Authors' response:

In the first draft of the manuscript we did not focus on interactions between the different socio-demographic variables since we did consider the univariate approach to the analysis more in line with the research questions in this study. We do, however, agree with the reviewer that interactions can be of major interest. In the revised version of the manuscript we have therefore adjusted the research questions to open up also for analysis of interactions. In the methods we have extended the description on how the analyses were made, including the added analysis of interactions (using loglinear analysis). We hope that it will be easy to follow and we do think that the quality of the manuscript has improved by these adjustments.

3. 'Patients' may not be equally representative, because less than half respondents responded. Whilst the socio-demographic chosen variables were similar (probably), there may be differences not investigated, such as greater severity or threat of violence which prevented disclosure among non-responders and greater than half did not respond. I am not sufficiently familiar with Bangladeshi household composition to suggest other explanations.

Authors' response:

We do appreciate the comment and agree with the reviewer. Although the response rate to the IPV questions is low with regard to the general survey participation, it is comparable or only slightly lower than the response rates reported from other IPV surveys at the general population level. An additional remark on the low response rate has been added to the Discussion section. Further, we would like to point out that the important findings from this study are not derived from the analyses of the absolute proportions of women exposed to IPV, but the relative contributions from interactions between different factors associated to IPV. The latter analyses are less susceptible to low participation rates, even though high participation rates always are desirable.