

## PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (<http://bmjopen.bmj.com/site/about/resources/checklist.pdf>) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	A social network analysis of psychological morbidity in an urban slum of Bangladesh: a cross-sectional study based on a community census
<b>AUTHORS</b>	Rabbani, Atonu; Biju, Nabila; Rizwan, Ashfique; Sarker, M

### VERSION 1 – REVIEW

<b>REVIEWER</b>	Professor Richard J Lilford University of Warwick, United Kingdom
<b>REVIEW RETURNED</b>	28-Nov-2017

<b>GENERAL COMMENTS</b>	<p>This is an interesting study based on a slum in Dhaka.</p> <p>Regarding the methods, I felt the sampling frame could have been better described. The census was presumably used as the first stage in a three stage process? How many data collectors were involved and how were they selected? I question controlling for socio-economic characteristics when these are likely to be on the causal pathway linking explanatory and outcome variables. However, the authors do analyse the coefficients on these controls separately.</p> <p>Turning to the results, the differences in score by degree of isolation seems small and this warrants some comment.</p> <p>Scores are not compared to those in other populations in the discussion. I think further comment on reverse causality and the literature on this topic demands more discussion.</p> <p>This is overall on reverse causality and the literature on this topic demands more discussion.</p> <p>A very good study with good statistics.</p>
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<b>REVIEWER</b>	Angel Martinez-Hernaez Rovira i Virgili University (Spain)
<b>REVIEW RETURNED</b>	14-Jan-2018

<b>GENERAL COMMENTS</b>	<p>This paper, entitled “A social network analysis of psychological morbidity in an urban slum of Bangladesh: a cross-sectional study based on a community census”, has as main purpose to test whether social ties play any roles in mitigating depression and anxiety among young men living in a poor urban Community in Dhaka (Bangladesh). The study is well-conducted, the study design is robust, and the text is well-written. The paper is interesting and</p>
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	<p>needs only minor revisions:</p> <p>1) The authors developed a cross-sectional study design based on individual respondents from a census of young men living in an urban slum. The sample is adequate even though the authors should describe more in detail why this census was limited only to young men. I recommend explaining more clearly this point.</p> <p>Discretionary Revisions:</p> <p>2) It is well-known that GHQ-12 was originally developed as a unitary screening measure for psychological problems. GHQ-12 include items such as Loss of sleep over worry, Capable of making decisions, Able to face problems, Feeling unhappy and depressed, among others. At this point, the question is what kind of symptoms are more frequent in Vashantek? A table with an item per item analysis will be very welcome to gain insight in the kind of emotional distress experienced by the respondents.</p>
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<b>REVIEWER</b>	Adrian Barnett Queensland University of Technology, Australia
<b>REVIEW RETURNED</b>	18-Jan-2018

<b>GENERAL COMMENTS</b>	<p>This was an interesting study of how being connected might impact on mental health. It appeared to be well conducted and was well described.</p> <p>The improvement in mental health per additional friend is relatively small and there's no indication of whether the change has public health significance. It appears that around three additional friends are needed to give the same benefit as being married, although this is an internal comparison. Some comment on the size of the change for public health should be given. There is a comment on the overall mean in the discussion, but there also needs to be some mention of the size of the change. For example, is it large enough that there would be a noticeable benefit from programs that aimed to increase connectedness?</p> <p>I am not familiar with some of the network summary statistics such as the average all-closeness centrality score and betweenness centrality score. It would be helpful to describe what these numbers mean, either in general or for this study.</p> <p>When describing the GHQ questionnaire you need to say if higher scores are better or worse (page 6). This should also go in the abstract and the key table.</p> <p>It's no surprise that the overall distribution failed the normality test (page 9). It would be more interesting to check the distribution of the residuals of the model for normality. I would also look for residual outliers as a further check of the model.</p> <p>Minor comments (using page numbers in bottom-right corner of the manuscript's page)</p> <ul style="list-style-type: none"> <li>- I think the authors mean "multiple variable model" which is a single dependent variables and multiple predictors, rather than "multivariate model" which is multiple dependent variables and potentially multiple predictors.</li> <li>- page 9, line 4, i would round the mean age to an integer</li> <li>- there are no details given on how the participants gave their informed consent.</li> </ul>
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	<ul style="list-style-type: none"> <li>- Figure 2, what do the different shades of grey indicate?</li> <li>- The hyphens are getting confused in table 3. I would use true minus signs in place of hyphens and use "to" or a comma to separate the lower and upper intervals.</li> <li>- Table 3, it may be worth a reminder in the legend or footnote that higher scores indicate worse mental health</li> <li>- Page 13, "Interestingly, respondents born outside the community have better mental health outcomes" The table shows the opposite.</li> </ul>
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## VERSION 1 – AUTHOR RESPONSE

Reviewers' Comments to Author:

1. Editorial Requests:

Please revise the fourth bullet point of the 'Strengths and Limitations' section on page 2. This is not a strength or limitation of the study's design or methods.

As a reminder, this section should contain up to five short bullet points, no longer than one sentence each, that relate specifically to the methods or design of the study reported (see: <http://bmjopen.bmj.com/site/about/guidelines.xhtml#articletypes>). It should not be a summary of the study and its findings.

I would like to thank you for the opportunity to re-submit the paper. I have addressed all the comments raised by the referees and explain the changes in the letter for the referees.

We have also revised the bullet points of the “Strengths and Limitations” section on page 2. We have dropped the fourth bullet point from the original version of the paper.

Once again, thank you for the opportunity to resubmit. We feel that the paper has substantially improved and we hope that you share our view.

Reviewer: 1

Reviewer Name: Professor Richard J Lilford

Institution and Country: University of Warwick, United Kingdom

Competing Interests: None declared

1. This is an interesting study based on a slum in Dhaka.

We really appreciate the kind words from the reviewer. We would also like to thank the reviewer for the suggestions on how to improve the manuscript.

2. Regarding the methods, I felt the sampling frame could have been better described. The census was presumably used as the first stage in a three stage process? How many data collectors were involved and how were they selected?

We employed nine experienced data collectors to carry out all the 824 interviews in 26 days during the month of December 2016. All data collectors had prior experiences with quantitative survey through Survey CTO using tablets (e.g. Samsung Galaxy Tab S4). They were recruited based on their previous performances of working with different projects. We include a short description of this process in the revised manuscript. Please see page 5, paragraph 3, lines 22-23 and page 6, paragraph 1, lines 1-2.

We have also further elaborated the sampling process in the section on Sample and sampling technique. Please see pages 5-6.

3. I question controlling for socio-economic characteristics when these are likely to be on the causal pathway linking explanatory and outcome variables. However, the authors do analyse the coefficients on these controls separately.

As we have mentioned in the manuscript, in absence of credible identification strategy or exogenous variation, we cannot aim to infer any causal relationship in this paper. Having said that we have controlled for the variables to block the back-door influences resulting from these factors (using a framework suggested by Pearl, 2009). We include this in the discussion section. See page 15, paragraph 2, lines 10-13.

4. Turning to the results, the differences in score by degree of isolation seems small and this warrants some comment.

We also think this point needs more elaborated discussions. First, one should acknowledge, because GHQ measures unspecified psychological morbidity, there would be measurement errors in measuring respondents' mental status. Hence, our model (or any) will be able to explain only a part of the overall variation of the mental status among the respondents. Moreover, the community ties capture only one aspect of social ties of the respondents. For example, we do not include the outside friends in our social network analyses and the regression coefficients will generally be underestimated when we have measurement errors in the explanatory variables. We should also consider other variables (being born within the community, being married) that capture different aspects of network ties also contribute towards explaining mental status. We discuss this in page 15, paragraph 3, lines 14-23.

5. Scores are not compared to those in other populations in the discussion. I think further comment on reverse causality and the literature on this topic demands more discussion. This is overall on reverse causality and the literature on this topic demands more discussion.

We now report the mean GHQ score among diagnosed mental patients as reported in Islam and Iqbal (2008). Please see page 14, paragraph 3, lines 12-14.

Because of the cross-sectional nature of the data and absence of exogenous variation in social network, we cannot make any causal inference in the present study. This is an important limitation of the study that we have acknowledged in the original manuscript. We underscore this point further in the discussion section of the revised manuscript. See page 15, paragraph 2, lines 10-11.

6. A very good study with good statistics.

We really appreciate it.

Reviewer: 2

Reviewer Name: Angel Martinez-Hernaez

Institution and Country: Rovira i Virgili University (Spain)

Competing Interests: None declared

1. This paper, entitled "A social network analysis of psychological morbidity in an urban slum of Bangladesh: a cross-sectional study based on a community census", has as main purpose to test whether social ties play any roles in mitigating depression and anxiety among young men living in a poor urban Community in Dhaka (Bangladesh). The study is well-conducted, the study design is robust, and the text is well-written. The paper is interesting and needs only minor revisions:

We really appreciate the comments and suggestions that have helped us to improve our manuscript even further.

2. 1) The authors developed a cross-sectional study design based on individual respondents from a census of young men living in an urban slum. The sample is adequate even though the authors should describe more in detail why this census was limited only to young men. I recommend explaining more clearly this point.

The study was primarily conceived to understand the risky sexual behavior and its determinants among the young men in an urban slum and also to provide a proof of concept to address mental health (stress) and gender issues using a novel intervention in this particular context. Most of the previous studies in this area have addressed adolescents and also female population. Hence, we proposed to study post-adolescent young men between 18 and 29 years of age and received the research grant to pursue the study. We believe focusing on young men between 18 and 29 was a particular strength of the proposed research. However, this also constrained us to focus on this group. We highlight this in the paper. Please see page 5, paragraph 2, lines 9-13.

3. Discretionary Revisions:

- 2) It is well-known that GHQ-12 was originally developed as a unitary screening measure for psychological problems. GHQ-12 include items such as Loss of sleep over worry, Capable of making decisions, Able to face problems, Feeling unhappy and depressed, among others. At this point, the question is what kind of symptoms are more frequent in Vashantek? A table with an item per item analysis will be very welcome to gain insight in the kind of emotional distress experienced by the respondents.

Thanks so much for this suggestions. We include a table in the appendix with the item analyses. Please see page 1, Appendix Table 1.

Reviewer: 3

Reviewer Name: Adrian Barnett

Institution and Country: Queensland University of Technology, Australia

Competing Interests: None declared

1. This was an interesting study of how being connected might impact on mental health. It appeared to be well conducted and was well described.

We appreciate this. We have also gained much from the comments and suggestions below. We have addressed those to improve the manuscript.

2. The improvement in mental health per additional friend is relatively small and there's no indication of whether the change has public health significance. It appears that around three additional friends are needed to give the same benefit as being married, although this is an internal comparison. Some comment on the size of the change for public health should be given. There is a comment on the overall mean in the discussion, but there also needs to be some mention of the size of the change. For example, is it large enough that there would be a noticeable benefit from programs that aimed to increase connectedness?

These are very important points. As we mentioned in a reply to the first reviewer, because of the natures of measurement, our empirical models (included in Table 3 and Appendix Table 3) can only explain only a part of the variation in the outcome variable. Social policies to address community mental health through investing in community organization and fostering social ties can indeed contributes towards between mental health status among the young men living in low income urban communities in general. We highlight these issues in the discussion section. Please see page 15, paragraph 3, lines 12-21 and our replies to comment #4 by reviewer #1,

3. I am not familiar with some of the network summary statistics such as the average all-closeness centrality score and betweenness centrality score. It would be helpful to describe what these numbers mean, either in general or for this study.

This is a very useful suggestion. We have included definitions of different social network parameters used in the appendix. Please see Appendix B: Centrality measures, pages 2-4.

4. When describing the GHQ questionnaire you need to say if higher scores are better or worse (page 6). This should also go in the abstract and the key table.

We appreciate this suggestion. We have included a note on page 6 (see paragraph 3, lines 12-13). We have also included it in the note for Table 3 (on page 12-13).

5. It's no surprise that the overall distribution failed the normality test (page 9). It would be more interesting to check the distribution of the residuals of the model for normality. I would also look for residual outliers as a further check of the model.

We have included more detailed diagnostic tests as per this suggestion. Please see Appendix C: Diagnostic Tests for Regression Results in Table 3. We also mention this in page 8, paragraph 1, lines 8-12.

Minor comments (using page numbers in bottom-right corner of the manuscript's page)

- I think the authors mean "multiple variable model" which is a single dependent variables and multiple predictors, rather than "multivariate model" which is multiple dependent variables and potentially multiple predictors.

In the literature the two terms often used interchangeably. But we agree with the reviewer that multivariate may not be the appropriate term here. We have replaced multivariate with multivariable as also suggested in, for example, Hidalgo, B., & Goodman, M. (2013). Multivariate or multivariable regression? *American journal of public health*, 103(1), 39-40.

- page 9, line 4, I would round the mean age to an integer

We have modified the mean age to integer as per reviewer's suggestion. Please see page 9, paragraph 1, line 9.

- there are no details given on how the participants gave their informed consent.

All the participants provided written informed consent prior to the survey. Data collectors at first explained about the research objective and mentioned about the confidentiality that will be maintained after gathering the information from the young men. Moreover, it was also mentioned that the participation in the survey is completely voluntary and the participant may wish to withdraw himself at any point of the survey even after signing the consent form. One copy of the written consent form was given to the participant and one copy was kept for the official record. We include this information in the Ethical approval section. Please see page 17, lines 9-15 in the revised manuscript.

- Figure 2, what do the different shades of grey indicate?

The different shades in the sociogram in Figure 2 signified different sub-networks within the larger community-wide network structure. After further reckoning, we think the shades are not adding useful information we have replaced the original sociogram with a new one, which shows the network structure more clearly. Please see page 22, Figure 2.

- The hyphens are getting confused in table 3. I would use true minus signs in place of hyphens and use "to" or a comma to separate the lower and upper intervals.

Thanks so much for the suggestions. We have modified as such. See page 12-13, Table 3, also Appendix Table 3.

- Table 3, it may be worth a reminder in the legend or footnote that higher scores indicate worse mental health

We appreciate this suggestion. We have included the reminder in the note. Please see page 13, note for Table 3.

- Page 13, "Interestingly, respondents born outside the community have better mental health outcomes" The table shows the opposite.

We understand this was confusing. We have modified the presentation of the results. We have redefined the indicator variable to have a value of 1 if the respondent reported being born at Vashantek and 0 otherwise. We have also added a note in the table suggesting higher GHQ value meaning lower mental health status. Hence, a negative coefficient associated with being born within the community suggests better mental health outcomes (see page 12-13, Table 3). We have also made a mistake reporting the direction of the association and we want to thank the reviewer for pointing this out. We have corrected this. See page 13, paragraph 1, line 10.

#### VERSION 2 – REVIEW

<b>REVIEWER</b>	Angel Martinez Hernaez Rovira i Virgili University (Spain)
<b>REVIEW RETURNED</b>	03-Mar-2018

<b>GENERAL COMMENTS</b>	No comments to the resubmitted paper.
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<b>REVIEWER</b>	Adrian Barnett Queensland University of Technology Australia
<b>REVIEW RETURNED</b>	05-Mar-2018

<b>GENERAL COMMENTS</b>	<p>The authors have answered most of my questions. Some minor points remain.</p> <ul style="list-style-type: none"> <li>- Page 6, line 7. How many did not consent?</li> <li>- Page 11, line 14. What does this all-closeness centrality score mean? Is it low or high? Was it a surprise?</li> <li>- Page 12, line 21. The estimates do not look "more precise" based on the width of the confidence interval.</li> <li>- Page 16, line 6. Needs rewording</li> <li>- Keyword of "urban slum" rather than "regression analysis"</li> </ul>
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#### VERSION 2 – AUTHOR RESPONSE

Reviewers' Comments to Author:

Reviewer: 2

Reviewer Name: Angel Martinez Hernaez

Institution and Country: Rovira i Virgili University (Spain)

Competing Interests: None declared

No comments to the resubmitted paper.

Reviewer: 3

Reviewer Name: Adrian Barnett

Institution and Country: Queensland University of Technology, Australia

Competing Interests: None declared

The authors have answered most of my questions. Some minor points remain.

4. Page 6, line 7. How many did not consent?

Only two respondents refused to provide a written consent. We did not interview these individuals. We now report this in the manuscript. Please see page 6, paragraph 2, lines 7-8.

5. Page 11, line 14. What does this all-closeness centrality score mean? Is it low or high? Was it a surprise?

We appreciate this query. First, we replace drop "all-" from the term and retain only "closeness", which, in the context of social network analysis, means "is the inverse of the average distance within a network." We define the concept in more details in Appendix B. Second, we did not have a strong prior about its value as we have not found a study similar to our context. For example, only other study with similar social network analyses in Bangladesh is by Gayen and Raeside (2010); however, they do not report the mean closeness centrality score. Shakya et al. (2014) report a value of 0.31 (see Table 1, p.4 in the pre-publication version) and Rosenquist et al. (2011) report a value of 0.087 (see Table 1, p.275). Hence, our estimate is on the lower side. In absence of a strong prior, we do not find the value surprising. We have not made any modification to the manuscript as a response to this comment to maintain the brevity of the current version. We will be happy to include a short discussion in the paper. We leave to the judgement of the editor and/or the reviewer.

6. Page 12, line 21. The estimates do not look "more precise" based on the width of the confidence interval.



We agree and have dropped the phrase from the current version of the manuscript. See page 12, paragraph 2, lines 15-16.

7. Page 16, line 6. Needs rewording

We have rephrased the sentence. Please see page 15, paragraph 2, lines 15-17.

8. Keyword of "urban slum" rather than "regression analysis"

We have revised the keywords accordingly. See page 1, line 20.

### VERSION 3 – REVIEW

<b>REVIEWER</b>	Adrian Barnett Queensland University of Technology
<b>REVIEW RETURNED</b>	04-Apr-2018
<b>GENERAL COMMENTS</b>	Thanks. I have only the following minor comments: - The bullet points mention a "roster-based approach" but this is not used elsewhere. I suggest using the same wording as the methods. - page 3, line 8, needs re-wording; something missing. - Table 3, "atVashantek" missing space - Contributorship statement, "AR led the analysis ", needs to be "AR1"