

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Association of volunteering with mental well-being: a lifecourse analysis of a national population based longitudinal study in the UK
AUTHORS	Tabassum, Faiza; Mohan, John; Smith, Peter

VERSION 1 - REVIEW

REVIEWER	Stefan Priebe Queen Mary, university of London United Kingdom
REVIEW RETURNED	06-Feb-2016

GENERAL COMMENTS	<p>The paper addresses a most relevant question and uses a unique data set for the analysis. The findings are surely of interest and show a non-linear relationship between age and a global measure of mental well-being.</p> <p>My main concern about the paper is that it suggests the study has tested an effect. It analyses exclusively correlations which do not allow conclusions on a causal relationship. Throughout the paper, the terminology varies. Sometimes, appropriate terms such as association are used, but frequently there is - explicitly or implicitly - the assumption of a direct causal impact. Of course, volunteering may impact on mental health, but one might also argue that the mental health status of a person can influence the motivation and ability to engage in voluntary work. This is particularly relevant as the percentage of people who volunteer appears to vary across age groups. For example, one might speculate as to whether at a young age people who feel well are more focused on their professional career and private life than volunteering, whilst at a higher age people have settled and more people, including those who feel well, consider volunteering. Again, this is speculation, but may illustrate that there are many more explanations (including those that assume that both volunteering and mental well-being are influenced by further factors) that would be consistent with the data.</p> <p>I also wonder whether the question as to whether people did unpaid voluntary work is equally understood across all age groups and parts of the society. What exactly is included? When my son mows the lawn in our garden, when I help a friend filling in a form, when my wife does the shopping for a neighbour, or only more formalised roles in voluntary organisations (in which case the % of positive answers would be other surprising)? I know that the authors cannot clarify this, but this limitation might be discussed.</p> <p>I am not a statistician, but would have preferred an analysis showing the amount of variance that is explained by the associations so that one has a clearer picture of the practical relevance. A very minor</p>
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	remark: a significance level of 0.000 does not exist.
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REVIEWER	Dr Colette Fegan Sheffield Hallam University Sheffield United Kingdom
REVIEW RETURNED	21-Feb-2016

GENERAL COMMENTS	<p>Thank you for inviting me to review this manuscript. Whilst I have an interest in this area, I feel that a future review of the manuscript should be allocated to a reviewer with more statistics experience than myself.</p> <p>There was some repetition in the earlier parts of the paper. The use of GHQ is fairly limiting in terms of a wellbeing outcome and I think the paper would benefit from further consideration of these limitations. The paper has the advantage of addressing a population over a longer period of time which is the most interesting factor of this paper. I would like to see how this benefit is balanced with consideration of further research using other outcome measures, aimed at populations of volunteers in this age range, or using other approaches to ascertain what aspects of wellbeing are enhanced through volunteering and types of volunteering. The paper has left me wanting to understand more to further triangulate these findings.</p>
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REVIEWER	Gareth Lloyd Institute for Volunteering Research London, UK
REVIEW RETURNED	21-Feb-2016

GENERAL COMMENTS	There are some very minor proofing errors in the introduction and discussion (and in some cases this impacts clarity of some statements/arguments) so minor revision is recommended; however overall the paper and study is methodologically strong, well argued and highly relevant to current policy context.
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REVIEWER	Richard McNally Institute of Health & Society, Newcastle University, England, United Kingdom
REVIEW RETURNED	17-Mar-2016

GENERAL COMMENTS	<p>GENERAL COMMENTS</p> <p>This is an interesting paper. However, the current version suffers from a lack of clarity throughout. Furthermore, the English language needs improving. Specific comments are given below.</p> <p>SPECIFIC COMMENTS</p> <ol style="list-style-type: none"> 1. Abstract, Objectives. It is not clear what is meant by the 'younger population'. This needs to be defined. 2. Abstract, Participants. It is not clear what is meant by '63433 observations (person-years)'. 3. Abstract, Results. There is a lack of clarity. For example, it is
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	<p>stated that 'Interaction terms fitted between age and volunteering revealed variations in the associations between volunteering and mental well-being across the lifecourse' - however is not really very informative - the meaning is unclear to the reader. What sort of interaction terms were fitted? What sort of variations were found? The final sentence of the Results section is difficult to interpret. The effect of never volunteering is not very clear.</p> <p>4. Abstract, Conclusions. The conclusions are not very informative. How can the results be interpreted? This section should precis the conclusions from the main text.</p> <p>5. Strengths and limitations of the study. The second point is too long. There is no need to mention the methods here, simply say 'after adjusting for potential risk factors'.</p> <p>6. Introduction. This section should finish with a clear statement of the aims and objectives of the study.</p> <p>7. Materials and Methods - Data, second paragraph, final sentence (and elsewhere). The link between observations and person-years is not clear and requires further careful explanation.</p> <p>8. Materials and Methods - Data, third paragraph, second sentence. The word 'data' is plural, so it should be 'Our data have...'</p> <p>9. Materials and Methods, final sentence. It is stated that 'Those missing in analysis data were younger, more likely to be female; were more likely to have a low level of education, and from manual social class'. Could this have caused any bias in the analyses? Some discussion should be given.</p> <p>10. Statistical methods, first sentence. It should be 'likelihood ratio test' - not 'ration'.</p> <p>11. Results. This section suffers from a lack of clarity and needs re-writing. Methods are used, which should be first mentioned in the Methods section, e.g. page 10, final sentence - chi-square tests and ANOVA (indeed this sentence is far from clear).</p> <p>12. Results - GHQ and volunteering. What is meant by 'not much difference in GHQ scores'? A more precise explanation is required.</p> <p>13. Results from random intercept model. This section again suffers from a lack of clarity and needs re-writing. Statistical significance does not appear to have been defined (in the Methods section).</p> <p>14. Table 2. SEs of '0.000' don't make sense. Give precise values.</p> <p>15. Discussion - Principal findings. More interpretation of the results should be provided.</p> <p>16. The English language needs correcting and polishing throughout the paper.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: Stefan Priebe

Institution and Country: Queen Mary, university of London, United Kingdom

Please state any competing interests or state 'None declared':

None declared

Please leave your comments for the authors below The paper addresses a most relevant question and uses a unique data set for the analysis. The findings are surely of interest and show a non-linear relationship between age and a global measure of mental well-being.

1) My main concern about the paper is that it suggests the study has tested an effect. It analyses

exclusively correlations which do not allow conclusions on a causal relationship. Throughout the paper, the terminology varies. Sometimes, appropriate terms such as association are used, but frequently there is - explicitly or implicitly - the assumption of a direct causal impact. Of course, volunteering may impact on mental health, but one might also argue that the mental health status of a person can influence the motivation and ability to engage in voluntary work. This is particularly relevant as the percentage of people who volunteer appears to vary across age groups. For example, one might speculate as to whether at a young age people who feel well are more focused on their professional career and private life than volunteering, whilst at a higher age people have settled and more people, including those who feel well, consider volunteering. Again, this is speculation, but may illustrate that there are many more explanations (including those that assume that both volunteering and mental well-being are influenced by further factors) that would be consistent with the data.

R) We are thankful to the reviewer for this comment. We feel that at some points in the paper our terminology might have given the impression that our results suggested a causal relationship. We have amended our terminology accordingly. The 'causal relationship' is an important issue in epidemiological studies. However, any exploration on this issue is beyond the scope of this study. We already have mentioned this point of causal relationship in the limitations of the study. .

2) I also wonder whether the question as to whether people did unpaid voluntary work is equally understood across all age groups and parts of the society. What exactly is included? When my son mows the lawn in our garden, when I help a friend filling in a form, when my wife does the shopping for a neighbour, or only more formalised roles in voluntary organisations (in which case the % of positive answers would be other surprising)? I know that the authors cannot clarify this, but this limitation might be discussed.

R) We are thankful to the reviewer in mentioning this very interesting point about volunteering questions. We agree with the reviewer but unfortunately 'informal volunteering' has not been collected in BHPS. Previous research has indicated that formal and informal volunteering are two separate phenomena, governed by different forces (Wilson and Musick 1997)*. In addition, formal volunteering has proved the most comparable across different surveys. Now in view of the reviewer's comment, we have incorporated this point in the revised version of the paper.

* Wilson, John; Musick, Marc. Who cares? Toward an integrated theory of volunteer work. American Sociological Review, Vol 62(5), 1997, 694-713.

3) I am not a statistician, but would have preferred an analysis showing the amount of variance that is explained by the associations so that one has a clearer picture of the practical relevance.

R) We are thankful for this comment and now added a section in results on variances (page 17; 1st paragraph).

4) A very minor remark: a significance level of 0.000 does not exist.

R) It is now modified.

Reviewer: 2

Reviewer Name: Dr Colette Fegan

Institution and Country: Sheffield Hallam University, Sheffield, United Kingdom

Please state any competing interests or state 'None declared':

None declared

Please leave your comments for the authors below Thank you for inviting me to review this manuscript. Whilst I have an interest in this area, I feel that a future review of the manuscript should

be allocated to a reviewer with more statistics experience than myself.

1) There was some repetition in the earlier parts of the paper.

R) Thanks for mentioning this point and now we have removed such repetition.

2) The use of GHQ is fairly limiting in terms of a wellbeing outcome and I think the paper would benefit from further consideration of these limitations. The paper has the advantage of addressing a population over a longer period of time which is the most interesting factor of this paper. I would like to see how this benefit is balanced with consideration of further research using other outcome measures, aimed at populations of volunteers in this age range, or using other approaches to ascertain what aspects of wellbeing are enhanced through volunteering and types of volunteering. The paper has left me wanting to understand more to further triangulate these findings.

R) We are thankful to the reviewer for mentioning this very important point. However, we were constrained to use GHQ as a proxy measure of mental well-being. There are other measures such as WEMWBS (which is not collected in BHPS) or CASP-19 (but this is only included in Wave 11 and further it is a measure to capture well-being among elderly populations). Having said this, GHQ has been used widely as a proxy measure of mental well-being. GHQ consists of twelve questions (administered via a self-completion questionnaire) covering feelings of strain, depression, inability to cope, anxiety-based insomnia, and lack of confidence, amongst others. So, GHQ-12 includes six positive and six negative states and a choice of four options for each in which the presence or intensity of the state over the last few weeks is related to its usual frequency or intensity, thereby creating a 36 point 'Likert' scale. GHQ has been widely used in health research to measure prevalence and determinants of probable mental illness, and also for evaluation in intervention studies. Therefore, we are unable to use other outcomes of mental well-being in our analyses as they are not collected as part of BHPS. We already have mentioned this issue in limitations of the study.

Reviewer: 3

Reviewer Name: Gareth Lloyd

Institution and Country: Institute for Volunteering Research, London, UK

Please state any competing interests or state 'None declared':

None declared

Please leave your comments for the authors below

There are some very minor proofing errors in the introduction and discussion (and in some cases this impacts clarity of some statements/arguments) so minor revision is recommended; however overall the paper and study is methodologically strong, well argued and highly relevant to current policy context.

R) We are thankful to the reviewer and we believe that our revisions have responded to the proofing errors and improved the clarity of our argument.

Reviewer: 4

Reviewer Name: Richard McNally

Institution and Country: Institute of Health & Society, Newcastle University, England, UK

Please state any competing interests or state 'None declared':

None declared

Please leave your comments for the authors below

GENERAL COMMENTS This is an interesting paper. However, the current version suffers from a lack

of clarity throughout. Furthermore, the English language needs improving. Specific comments are given below.

SPECIFIC COMMENTS

1. Abstract, Objectives. It is not clear what is meant by the 'younger population'. This needs to be defined.

R) We have taken out 'younger population' from the paragraph.

2. Abstract, Participants. It is not clear what is meant by '63433 observations (person-years)'.

R) It means approximately, 10000 people per year over a period of 7 years ($63433/7$). So, 63433 are total number of cases over 7 waves chosen for the analyses. The term 'person-years' is well-understood in the literature of disciplines such as Epidemiology, Statistics and demography. Therefore, we feel that most of the readers should be able to understand it and there is no need to provide any further explanation to it.

3. Abstract, Results. There is a lack of clarity. For example, it is stated that 'Interaction terms fitted between age and volunteering revealed variations in the associations between volunteering and mental well-being across the lifecourse' - however is not really very informative - the meaning is unclear to the reader. What sort of interaction terms were fitted? What sort of variations were found? The final sentence of the Results section is difficult to interpret. The effect of never volunteering is not very clear.

R) We are thankful to the reviewer in raising these points, we have incorporated these points in the abstract now and highlighted the changes we have made.

4. Abstract, Conclusions. The conclusions are not very informative. How can the results be interpreted? This section should precis the conclusions from the main text.

R) We have revised the conclusions now, thanks for the comments.

5. Strengths and limitations of the study. The second point is too long. There is no need to mention the methods here, simply say 'after adjusting for potential risk factors'.

R) We have made the changes accordingly.

6. Introduction. This section should finish with a clear statement of the aims and objectives of the study.

R) We are thankful to the reviewer and now the changes are made.

7. Materials and Methods - Data, second paragraph, final sentence (and elsewhere). The link between observations and person-years is not clear and requires further careful explanation.

R) The term 'person-years' is a recognised and a commonly used concept in epidemiology and Statistics. Mostly the readers to such journals or articles have a reasonable knowledge of such concepts, e.g. the way they know SD (standard deviation). Therefore, in my opinion, there is no need to provide any explanation of this concept as most of the readers may find it waste of time.

8. Materials and Methods - Data, third paragraph, second sentence. The word 'data' is plural, so it should be 'Our data have...'.

R) Corrected

9. Materials and Methods, final sentence. It is stated that 'Those missing in analysis data were younger, more likely to be female; were more likely to have a low level of education, and from manual social class'. Could this have caused any bias in the analyses? Some discussion should be given.

R) This is a very valid point and to check it, we conducted a sensitivity analysis by imputing the missing values through an appropriate statistical procedure. The results based on the imputed data are the same which are reported here in the paper. Therefore, the absence of some data has not caused any bias (this is already been mentioned in the MS).

10. Statistical methods, first sentence. It should be 'likelihood ratio test' - not 'ration'.

R) Thanks, corrected now.

11. Results. This section suffers from a lack of clarity and needs re-writing. Methods are used, which should be first mentioned in the Methods section, e.g. page 10, final sentence - chi-square tests and ANOVA (indeed this sentence is far from clear).

R) Thanks for mentioning it; now these lines have been moved from results section to methods section and necessary editing has been done.

12. Results - GHQ and volunteering. What is meant by 'not much difference in GHQ scores'? A more precise explanation is required.

R) We are thankful to the reviewer in mentioning this point and now we have fixed the problem.

13. Results from random intercept model. This section again suffers from a lack of clarity and needs re-writing. Statistical significance does not appear to have been defined (in the Methods section).

R) We have defined statistical significance in the methods section and modified the discussion accordingly.

14. Table 2. SEs of '0.000' don't make sense. Give precise values.

R) Thanks for your observation, it has now been fixed.

15. Discussion - Principal findings. More interpretation of the results should be provided.

R) Now it has been modified.

6. The English language needs correcting and polishing throughout the paper.

R) Thank you. Amendments to the grammar and punctuation have been made.

VERSION 2 – REVIEW

REVIEWER	Stefan Priebe Queen Mary, University of London United Kingdom
REVIEW RETURNED	01-May-2016

GENERAL COMMENTS	I think that overall the authors have addressed the comments of the reviewers very well and the manuscript has improved as a result of it.
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	<p>My main concern about the original version was that it suggested a causal relationship, when in fact the authors just report a correlation and cannot claim that they provided evidence that volunteering improves mental well-being.</p> <p>The authors responded that they accepted the point and changed the terminology, removing all suggestions that they studied a causal relationship.</p> <p>However, I feel that this has not consistently happened. Even the title says that they assessed 'effects of volunteering on mental well-being'. Some statisticians may use the term 'effect' more generally, but in the common language the term 'effect' describes a causal relationship (although it may be indirect). If ones goes through the manuscript, a similar misleading terminology is used at several places and the discussion also focuses on one-directional effects, although then acknowledging the limitations of the claim.</p> <p>I can just repeat my view that good research requires a precise language, and that the data presented in this paper cannot provide evidence for an 'effect' of volunteering on mental well-being.</p>
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REVIEWER	Richard McNally Institute of Health & Society, Newcastle University, England, UK
REVIEW RETURNED	27-Apr-2016

GENERAL COMMENTS	The reviewer completed the checklist but made no further comments.
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VERSION 2 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: Stefan Priebe

Institution and Country: Queen Mary, University of London, United Kingdom

Please state any competing interests or state 'None declared':

None declared

My main concern about the original version was that it suggested a causal relationship, when in fact the authors just report a correlation and cannot claim that they provided evidence that volunteering improves mental well-being. The authors responded that they accepted the point and changed the terminology, removing all suggestions that they studied a causal relationship. However, I feel that this has not consistently happened. Even the title says that they assessed 'effects of volunteering on mental well-being'. Some statisticians may use the term 'effect' more generally, but in the common language the term 'effect' describes a causal relationship (although it may be indirect). If ones goes through the manuscript, a similar misleading terminology is used at several places and the discussion also focuses on one-directional effects, although then acknowledging the limitations of the claim. I can just repeat my view that good research requires a precise language, and that the data presented in this paper cannot provide evidence for an 'effect' of volunteering on mental well-being.

Response: we are sorry that our choice of words have caused some confusion. Our objective was not to demonstrate the causal relationship, we only meant to report the 'associations'. Although, as you have mentioned rightly that the term 'effects' has been used more generally in our paper. As a result now we have taken away the word 'effects' from the paper hopefully, it has solved the matter now.

Correction: Association of volunteering with mental well-being: a lifecourse analysis of a national population-based longitudinal study in the UK

Tabassum F, Mohan J, Smith P. Association of volunteering with mental well-being: a lifecourse analysis of a national population-based longitudinal study in the UK. *BMJ Open* 2016;6:e011327. There are two errors in this paper. (1) On page 2, line 9: 'one UK study...' should read 'one USA study...' and (2) on page 2, section 'Mental Well-being', line 5: 'question has four categories (0-4)' should read '(0-3)'.

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BMJ Open 2016;6:e011327corr1. doi:10.1136/bmjopen-2016-011327corr1



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