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

TITLE (PROVISIONAL)	Factors associated with quality of life in elderly hospitalized patients undergoing post-acute rehabilitation: a cross-sectional analytic study in Switzerland
AUTHORS	Bornet, Marc-Antoine; Rubli Truchard, Eve; Rochat, Etienne; Pasquier, Jérôme; Monod, Stefanie

VERSION 1 – REVIEW

REVIEWER	Federico Manuel Daray Researcher of the National Scientific and Technical Research Council (CONICET). Institute of Pharmacology. School of Medicine. University of Buenos Aires Paraguay 2155, piso 9 (C1121ABG) Ciudad Autónoma de Buenos Aires, Argentina Tel. (54+11) 5950-9500 (interno: 2161) www.institutodefarmacologia.org.ar None competing interests
REVIEW RETURNED	18-Aug-2017

GENERAL COMMENTS	• General Comments: The manuscript is a cross-sectional analysis of the association between biopsychosocial, spiritual factors and satisfaction with care with patient perceived quality of life using multinomial linear regression model. The author's main conclusion is that depression, functional status and satisfaction of care are strongly associated with perceived quality of life. The study is well written and the topic is interesting; however, I consider that it has important methodological points to address most of them regarding the linear regression model and also with the interpretation of the results. • Specific Comments: 1. I have some concerns with the linear regression model. There is no information in methods regarding how the multivariate linear regression model was built: a) which criteria's were employed to select independent variables? b) It is not clear why authors included or excluded variables, e.g. they state that they exclude some very important variables as spiritual factors because of missing data (69 /167), however, they include QPP-SF with 30 missing values? So, which is the criterion? c) How authors build the model, how they include the independent variables? They do it stepwise from the least to the most significant in preliminary testing or how? Furthermore, how the assumptions of the model were tested: a) Did they make an analysis to account for heteroscedasticity? b) How did authors check for outliers? All this points should be explained in methods.

	2. The estimation of the sample size for each of the hypothesis should be described. 3. The conclusion of the study didn´t correlated with the results of the linear regression model. Based on the results of the model, only depression and QPP-SF (although with a high level of missing data) where associated with QoL. Therefore, the main conclusion should be that both depression and satisfaction with care were independently associated with QoL. However, the authors conclude that biopsychosocial and spiritual descriptors are associated with QoL (page 16). That is not correct and should be change. The same in the title of the manuscript “biopsychosocial and spiritual descriptors” should be replaced by depression because at last, this was the only factor associate with QoL. The current title is not consistent with the results obtained. 4. Why authors strength the finding with spiritual factors if they have so much missing data (69 /167)? They included in this in the title of the manuscript! I think this is not appropriated; they should modify this point and only described as a minor finding. 5. The discussion started with “Elderly patients undergoing rehabilitation after acute care perceived a relatively high level of quality of life”. Compared to which patients they established that levels are high? This should be discussed. 6. In the discussion authors state “Quality of life has a strong relationship with mood and functional status in this study with those who claim that the QoL values are high”. Why they said the association was strong? Based on which result? In table 3, I can only see that the association is statistically significant but there is no data to establish that the association is “strong”. Authors should estimate some association measure in order to make this affirmation. 7. Why some data is obtained at time of admission and other during the second week? This should be explained. Can hospitalization affect some measures, e.g. depression? I think this point should be discuses. 8. The authors established as a limitation that he cross-sectional study cannot conclude any causal relationships between descriptors and quality of life. This is not a limitation; this is something inherent to the study design. If someone choose that design is because they are not interesting in causality. This should be removed from limitations.

REVIEWER	Jojo Kwok The Chinese University of Hong Kong
REVIEW RETURNED	22-Aug-2017

GENERAL COMMENTS	Overall this was a nice idea to investigate the association between biopsychosocial and spiritual factors associated with QOL among people in post-acute setting. However, the discussion and conclusion based on its current analytical methods seem unconvincing. Despite the authors acknowledged missing data as one of the study limitations, ignoring missing data notably introduced bias and hindered the study validity. Since spiritual factor is one of the major investigation of the study, I would suggest authors to use other analytical methods to handle missing data. Major concerns: - Page 12-16: Certain factors which associated with QOL (Table 2), including SDAT and CIRS, were not entered into the multivariate analysis (Table 3) due to the many missing value. I suggest the authors to select appropriate analytical methods for handling missing data. Also, to examine the sensitivity of different approaches. Results, discussion and conclusion should be made upon using appropriate analytic methods. - Page 11: It would be more informative to include the outcome scores in terms of gender and specialties of admission (Table 1). - Page 12: Any rationale of using Spearman's rank correlation over Pearson correlation among continuous variables?

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name: Federico Manuel Daray
Institution and Country: Researcher of the National Scientific and Technical Research Council (CONICET). Institute of Pharmacology. School of Medicine. University of Buenos Aires, Paraguay 2155, piso 9 (C1121ABG), Ciudad Autónoma de Buenos Aires, Argentina, Tel. (54+11) 5950-9500 (interno: 2161), www.institutodefarmacologia.org.ar
Please state any competing interests: None competing interests

General Comments: The manuscript is a cross-sectional analysis of the association between biopsychosocial, spiritual factors and satisfaction with care with patient perceived quality of life using multinomial linear regression model. The author's main conclusion is that depression, functional status and satisfaction of care are strongly associated with perceived quality of life. The study is well written and the topic is interesting.

ANSWER: Thank you for the positive feedback.

Comment: However, I consider that it has important methodological points to address most of them regarding the linear regression model and also with the interpretation of the results.

ANSWER: Thank you for your useful suggestions, which are discussed point by point below.

1. I have some concerns with the linear regression model. There is no information in methods regarding how the multivariate linear regression model was built: a) which criteria's were employed to select independent variables? b) It is not clear why authors included or excluded variables, e.g. they state that they exclude some very important variables as spiritual factors because of missing data (69 /167), however, they include QPP-SF with 30 missing values? So, which is the criterion? c) How authors build the model, how they include the independent variables? They do it stepwise from the least to the most significant in preliminary testing or how? Furthermore, how the assumptions of the model were tested: a') Did they make an analysis to account for heteroscedasticity? b') How did authors check for outliers?
All this points should be explained in methods.

ANSWER: a-c) We agree your suggestions and understand that exclusion from the outset of variables with the most missing values was not a relevant choice. Thus, a new multivariate analysis was performed (pages 12-13, lines 217-225). For this new analysis, all variables of Table 2 are included (but only the total score of QPP-SF and not its subscores). a') The residual variance is homogeneous, excluding any heteroscedasticity. b') Diagnostic plots were made and showed no outliers that emerge clearly. We have updated the Method section to reflect these points (page 9, lines 173-180).

2. The estimation of the sample size for each of the hypothesis should be described.

ANSWER: The number of participants required was estimated based on the number of participants needed to complete the regression model and not overfit it. We initially used a rule of thumb of 10 times the number of coefficients (10 variables requiring 100 patients). Then, owing to missing values, the number of patients included was majored. This explanation has been added in the text (page 9, lines 176-178).

3. The conclusion of the study didn´t correlated with the results of the linear regression model. Based on the results of the model, only depression and QPP-SF (although with a high level of missing data) where associated with QoL. Therefore, the main conclusion should be that both depression and satisfaction with care were independently associated with QoL. However, the authors conclude that biopsychosocial and spiritual descriptors are associated with QoL (page 16). That is not correct and should be change. The same in the title of the manuscript "biopsychosocial and spiritual descriptors" should be replaced by depression because at last, this was the only factor associate with QoL. The current title is not consistent with the results obtained.

ANSWER: The conclusion of the study has been adapted to match the results of the analyses (page 16, line 293). It is correct that all biopsychosocial and spiritual descriptors are not associated with quality of life, and we have made a comment about this (page 14, lines 245-247). The title has also been modified (see above, editorial requirements).

4. Why authors strength the finding with spiritual factors if they have so much missing data (69 /167)? They included in this in the title of the manuscript! I think this is not appropriated; they should modify this point and only described as a minor finding.

ANSWER: As described in the objectives and owing to the amount of missing data, analysis of the spiritual dimension is a secondary focus. We have removed any reference to spiritual factors from the title. But, as suggested (comment number 1b of Reviewer 1 and comment number 1 of Reviewer 2), the spiritual dimension was added in the multivariate analysis.

5. The discussion started with “Elderly patients undergoing rehabilitation after acute care perceived a relatively high level of quality of life”. Compared to which patients they established that levels are high? This should be discussed.

ANSWER: In this study, we found higher WHOQOL-OLD scores than those reported by Fang et al. who used data of a developmental study of the WHOQOL-OLD, which included 5566 respondents from 20 international centers (opportunistic sample of ill and well patients). The comparison with this reference is now included in the manuscript (page 13, lines 228-230).

6. In the discussion authors state “Quality of life has a strong relationship with mood and functional status” in this study with those who claim that the QoL values are high. Why they said the association was strong? Based on which result? In table 3, I can only see that the association is statistically significant but there is no data to establish that the association is “strong”. Authors should estimate some association measure in order to make this affirmation.

ANSWER: It is true that the association is statistically significant, but the data do not allow us to say it is a strong relationship. The phrase “strong relationship” has been changed to “a significant relationship” (page 13, line 233).

7. Why some data is obtained at time of admission and other during the second week? This should be explained. Can hospitalization affect some measures, e.g. depression? I think this point should be discuses.

ANSWER: As described in the method, some data were collected on admission and others were obtained in the second week of hospitalization; this is owing to the use of data either from the usual clinical setting or data specifically collected for this study. The research assistant presented the study and provided the consent form at the end of the first week and then made the evaluations on the second week. The first week in the center is very busy with medical admission, liaison nurse evaluation, occupational therapist evaluation, and so forth; these clinical activities should not be disrupted by research. We have no data on potential evolution of the psychological state on the first day of hospitalization in a post-acute geriatric rehabilitation center. It is true that it is a potential bias, which we have now acknowledged in the manuscript (page 15, lines 262-264).

8. The authors established as a limitation that the cross-sectional study cannot conclude any causal relationships between descriptors and quality of life. This is not a limitation; this is something inherent to the study design. If someone choose that design is because they are not interesting in causality. This should be removed from limitations.

ANSWER: This sentence was removed from the manuscript.

Reviewer: 2
Reviewer Name: Jojo Kwok
Institution and Country: The Chinese University of Hong Kong
Please state any competing interests: None declared

Overall this was a nice idea to investigate the association between biopsychosocial and spiritual factors associated with QOL among people in post-acute setting.

ANSWER: Thank you for this comment.

However, the discussion and conclusion based on its current analytical methods seem unconvincing. Despite the authors acknowledged missing data as one of the study limitations, ignoring missing data notably introduced bias and hindered the study validity. Since spiritual factor is one of the major investigation of the study, I would suggest authors to use other analytical methods to handle missing data.

ANSWER: Thank you for your careful review. These points are discussed below.

1. Page 12-16: Certain factors which associated with QOL (Table 2), including SDAT and CIRS, were not entered into the multivariate analysis (Table 3) due to the many missing value. I suggest the authors to select appropriate analytical methods for handling missing data. Also, to examine the sensitivity of different approaches. Results, discussion and conclusion should be made upon using appropriate analytic methods.

ANSWER: As described in the answers to Reviewer 1, a new multivariate analysis including all variables was performed. This was done with multiple imputation. The discussion and conclusion of the study were adapted according to the results of this new analysis (pages 13-14, lines 227-252 and page 16, lines 292-297).

2. Page 11: It would be more informative to include the outcome scores in terms of gender and specialties of admission (Table 1).

ANSWER: Following your suggestion, Table 1 has been revised to include the outcome scores in terms of sex and admission specialties.

3. Page 12: Any rationale of using Spearman's rank correlation over Pearson correlation among continuous variables?

ANSWER: Using Pearson correlation needs to make the hypothesis of a linear relation between the variables, which is not necessarily true in our case. We preferred to use the Spearman's rank correlation test to detect monotonic relationships between our variables

VERSION 2 – REVIEW

REVIEWER	Federico Manuel Daray Researcher of the National Scientific and Technical Research Council (CONICET). Institute of Pharmacology. School of Medicine. University of Buenos Aires. Paraguay 2155, piso 9 (C1121ABG), Ciudad Autónoma de Buenos Aires, Argentina. Tel. (54+11) 5950-9500 (interno: 2161) www.institutodefarmacologia.org.ar No competing interests
REVIEW RETURNED	16-Sep-2017

GENERAL COMMENTS	I consider that the changes made by the authors have improved the manuscript and is eligible for publication in the BMJ