Author’s response to reviews

Title: The prevalence and socio-demographic correlates of depressive symptoms among Cypriot university students: a cross-sectional descriptive co-relational study

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Author’s response to reviews: see over
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Revised MS: 1249290652123762: The prevalence and socio-demographic correlates of depressive symptoms among Cypriot university students: a cross-sectional descriptive co-relational study

Sokratous S, Anastasios Merkouis, Nicos Middleton and Maria Karanikola

Dear Editor,

Thank you for considering our manuscript for publication in the BMC Psychiatric journal.

We would like to thank the three reviewers and yourself for taking the time to evaluate the above manuscript. We are very grateful for the constructive comments and suggestions, which helped further clarify some important issues and improve the paper.

We have known revised our manuscript according to reviewers’ comments to meet the demands of the journal.

Please refer to the changes below and follow step by step the alterationation that occurred suggested by reviewers’.

Warm regards,

Sokratis Sokratous
Response to reviewers’ comments:

Reviewer’s report
Version: 2 Date: 22 April 2014

Reviewer 1: Seblewengel Lemma

Abstract:

R2C1: In the background of the abstract the author proposed to identify the causal relationship between the factors mentioned and depressive symptoms; similar descriptions were made in the document somewhere else. Given the design of the study it is not possible to establish causal relationship with the mentioned factors except for the inherent characteristics such as gender. So it wouldn’t be appropriate to talk about causality as this level.

Response

We agree with the reviewer’s comment that due to the cross sectional design of the study, it was not appropriate to imply causality, explicitly or implicitly. The text in the background of the abstract (underlined text) as well the text and the sub-titles in the Results and Discussion sections of the manuscript have been re-written accordingly.

R2C2: In the result sections of the abstract, the authors reported odds ratios which are not statically significant as statistically significant findings. I suggest they should be reported as non-significant findings (line 22 - 24).

Response

This has now been corrected. Mention of the association between depressive symptoms and drugs/tobacco use has been eliminated from the results section of the abstract. Please see also our response to the comment RC9 page 4 in the response letter).

R2C3: In the conclusion part, the conclusion is made only for the socio-demographic characteristics but the key findings reported in the result section are beyond socio-demography so good to add more expressive terminology which embraces everything.

Response

The Conclusions section of the manuscript has been re-written to best reflect the key findings of the present study.
Introduction:

R2C4: In this section the author clearly stated the problem this research work is expected to answer, however, the inconsistencies reported by the author as a gap is not well supported by references and did not mention what those inconsistencies were.

Response

We have now made several revisions in this part of the manuscript. In particular, any previous inconsistencies have been clarified and have been supported by appropriate references. Additionally, the long list of factors previously mentioned has been removed and replaced by three groups of factors (Background, 3rd paragraph, underlined text).

R2C5: Besides, the author repeatedly mentioned clinically significant depression; well this has to be defined somewhere at the beginning and must use the term consistently across the document.

Response

In the Background section of the manuscript, the term “clinically significant depressive symptoms” has been replaced by “depressive symptoms. However, the term “clinical depressive symptoms” is used to describe the main variable of this study and its definition has been added in the Methods section of the manuscript (Instruments, lines 162-164, 2nd paragraph, underlined text).

Objectives:

R2C6: Here the author mentioned the objectives of the study as aim. It is good to simplify when writing the objectives in a measurable form and provide other detail descriptions in the methods.

Response

This section has been reduced to four lines (Aim section, underlined text, line 90-95).

R2C7: I suggest the author to be cautious when use the word ‘effect’ because it shows direction which cannot be established by the design used in this research.

Response

We agree with the reviewer. The word “effect” has been eliminated from the text (Aim section, 2nd & 4th line, underlined text).
Methods:

R2C8: In this section the author tried to state clearly the methodological process they went through and was clearly described. However, there are redundancies for instance under data analysis, the description made about the instrument is already dealt above. Such redundancies make readers loss interest (line 201-212).

Response

Redundancies have been eliminated from the text, thus the section of Methods has been significantly reduced in size (Methods section, underlined text, line 100).

R2C9: The other issue is, in the methods the author mentioned use of backward stepwise regression for the final model. In public health, we don't usually use such methods in regression because, this method eliminates variables from the final model and these variables excluded from the model are no more controlled for their possible confounding effect which raises the issue of validity of findings. I suggest the author re-calculate the multivariate analysis using the enter method to control the confounders.

Response

We would like to thank the reviewer for the suggestion. Indeed, we absolutely agree that the potentially confounding effect of variables eliminated during a stepwise regression model are not longer controlled for in the final model. Thus, such techniques are particularly not appropriate when investigating the effect of particular “exposure” variables, in which case a careful a priori selection of potential confounders should be performed. For instance in Sokratous S. BMC Public Health 2013; 13: 1121, where the association between stressful life events and depressive symptoms was investigated, we used the “traditional” public health approach suggested by the reviewer to investigate the association with stressful life event before and after adjusting for all potential confounders using the “enter” method irrespective of the statistical significance of the variables included in the model. However, in this case, the aim and hence the approach we used is somewhat different since we are not a priori investigating certain variables but rather the intention is to select among a large number the group of variables that best predict depressive symptoms in University students of variables. Hence, a stepwise approach was preferable. Furthermore, it should be noted that unlike multivariable linear regression where all variables associated with the outcome under study should be included, in logistic regression models, such as the one used here, the inclusion of additional unnecessary (e.g. non statistically significant variables) is known to increase the standard error of the effect estimates of the remaining variables in the
model (Kirkwood and Sterne. Essential Medical Statistics, 2nd Ed. Blackwell Science 2003. Ch. Regression modeling, pp. 340). In fact, in this study more than 30 variables were considered, many of which highly intercorrelated, which is an additional consideration against using the enter method. We have briefly explained this in Data Analysis, pp. 6, lines 192-202 – “Backward stepwise multivariable logistic regression models were used in order to select among the large number of variables considered here the final set associated with depressive symptoms controlling for the potential confounding effect of the rest of the variables in the model” and so on.

In addition to the above, we like to thanks the reviewer for the comment in the line 387 referring to multy regression analysis. It was a spelling mistake (the correct is p= 0.10 rather than p= 0.010 for statistical significance). This spelling mistake could confused the reviewers (for statistical invalid results, e.g. line 388 and in table 6).

Finally, by spelling mistake drug use statistical results presented invalid (OR =5.44 95% CI: 0.55 – 53.41) rather than OR =5.44 95% CI: 2.95 – 8.84 which is the correct statistical results, (p value <0.10, for each variable). It has known be corrected in the paper and table 6.

Results:

R2C10: In the section, I suggest the author to avoid the word ‘effect’ from the subtitles.

Response

The word “effect” has been eliminated, and the subtitles have been rephrased accordingly to reflect the cross-sectional design of the study.

R2C11: This section required some more organization. The author spends more time on describing the bi-variate analysis when the final model is yet to come. It is good to provide adequate emphasis for the final multivariate model because that is where the adjusted odds ratios are reported.

Response

We have now focused the Discussion of the manuscript on the results of the final multivariate model.

R2C12: Besides, there are unnecessary details given in the result which confuses the reader for instance the description made in line (349-366) is not that relevant for the overall objective of the study.

Response

The length of the results has been diminished (lines 204 to 318 vs. 222 to 388 prior to reviewing), thus unnecessary details (i.e lines 349-366) have been eliminated from the text.
**R2C13:** As mentioned earlier the author should focus on the multivariate analysis result and report it as it did for the bivariate result. Make sure you have done the analysis again based on the ‘enter’ method. When selecting variables for the final model, it is good to consider both the statically significance and the biological significance of a given variable. For instance we usually put age in the model even when it doesn’t fulfill the statistical criteria we set because age is a known confounder which affect both morbidity and mortality in a population.

**Response**

This issue has been clarified to the response of the comment 9. With respect to age, it should be noted that the sample is largely heterogeneous in terms of age and only a small proportion of participants are above 25 years of age. Furthermore, among the rest there a strong association between gender and age since in Cyprus boys start University two years later than girls as a result of their national service. Due to this collinearity, it was not appropriate to force age in the model as any effect of age is largely captured in the estimate of gender.

**R2C14:** Finally the result should be organized based on the objectives to have a flow.

**Response**

The section of the Results has been organized according to the objectives of the study (lines 90-95, underlined text), and the proper subtitles have been added to follow these objectives (lines 205, 225, 236, 248, 272, underlined text).

**Discussion:**

**R2C15:** In this section the author discussed the results by comparing the findings with other literature. However, this section again lacks clarity and there are visible redundancies. For instance, in the first paragraph, line 395-397, there is clear repetition.

**Response**

Lines 395-399 have been rephrased in order to eliminate redundancies (lines 323-328, underlined text).

**R2C16:** In the same paragraph, the author, reported the result as consistent with other studies, however the prevalence reported raged from some 2% to 70% but I don’t see any consistency here. Instead it is appropriate to appreciate the difference and explain why this difference happens.

**Response**

This paragraph has been re-written. The focus now is on those data which are in accordance with the findings of the present study, whilst a possible explanation for
this has also been given. Moreover, any reported differences between the present findings and previews literature are also discussed (Discussion, 2nd paragraph).

**R2C17:** Similarly in the same paragraph, the author cited many prevalence studies which make the discussion very confusing. So in the presence of such ample literature in the area, it good to focus on findings coming from similar context or closer to own context and explain if there is any observed result. The other way of doing this could be reporting similar findings together, then findings higher than what was observed together and provide reason for the difference then findings lower than what was observed and provide similar explanation. This will help to follow through the document easily.

**Response**

This paragraph has been re-written, according to the comments 16.

**R2C18:** From line 430 onwards, the author focused on the crude result. I suggest this part is taken out. Because the author is interested to look at the relationship between the factors and depression; the final multivariate model is the best result to be discussed.

**Response**

The factors involved in the multivariate model are now discussed (line 320 – 508).

**R2C19:** In line 465, the author talked about risk of depression among students who live alone just by reporting chi-square. This is not appropriate, chi-square doesn’t provide such evidence and I suggest the author re-think on this line.

**Response**

This part of the text has been eliminated, since this factor is not included in the multivariate analysis.

**R2C20:** In line 482-485, the author needs to provide more literature.

**Response**

As a result of the general re-organization of the manuscript, this part of the text has been eliminated from the text.

**R2C21:** The other important point, the author tends to repeat results in the discussion sections. This makes the result bulky and redundant. For instance line 502 to 505 is already described in this detail in the result section.

**Response**

Repetitions have been eliminated from the section of Discussion.
R2C22: The author also mixes discussion with recommendation for instance line 507-509.

Response

Any recommendations have been eliminated from the section of Discussion. The section of Conclusions includes a brief text with recommendations (lines 547-558, underlined text).

Lastly the author considered the possible limitations of the study.

R2C23: In general, the author addressed the points in each section of the document, however it requires, huge re-organization especially the result and the discussion sections.

Response

This has been done (see response to comments 24, 25)

R2C24: The author must focus on the main objectives and answer them instead providing sideline information and make the paper bulky.

Response

According to the suggestions made in comment 25, the section of the Discussion has been re-written.

R2C25: I am saying this for the second time; the author should focus on the main findings in the discussion section. For me the main findings are the prevalence and the results coming out of the multivariate analysis. The others may support the explanations made by the author in the discussion.

Response

The section of the Discussion has been re-written in the suggested way, focusing on the prevalence of the clinical depressive symptoms and the interpretation of the results coming from the multivariate analysis.

R2C26: The document also need language edition!

Response

There has been a professional editing throughout the text.
Reviewer’s report

Reviewer 2 : Yemane Berhane

Reviewer’s report:

General
R1C1. This is a very well done paper. It would however improve readability if its volume is substantially reduced.

Response
The volume of the manuscript is substantially reduced to improve readability (28 pages length rather than 38 pages in last version of manuscript).

Compulsory revisions
R1C2. Avoid causality discussion as the study did not use the proper design.

Response
Text in the Abstract, Aim, Results and Discussion sections of the paper has been rephrased accordingly to reflect the cross-sectional design of the study (underlined text).

R1C3. Be consistent about the study design throughout the paper- ‘cross sectional study with internal comparison’ can describe it best.

Response
The study design has been re-written (line 100, underlined text).

R1C4. Line 25-29: results difficult to understand.

Response
This section in the conclusions (results) has been re-organized and decreased in length to improve readability.

R1C5. Line 72-85: no reference given to the factors described in this section. It is good to relate each factor to a specific reference.
Response

As part of a more general re-organization of the manuscript based on all the reviewers’ recommendations (including reducing the manuscript in length), this part has been eliminated.

R1C6. Line 121-125: better to move to the first paragraph in results section.

Response

We did consider the reviewer’s suggestion regarding this point during the re-structuring of the manuscript, however, it was decided that this information was more suited for the Methods (rather than the Results section of the manuscript) since it refers to the data collection procedure (line 119 - 121).

R1C7. Page 7-10: The crude results can be summarized briefly. The multivariate analysis is the one that need to be emphasized.

Response

The Results section has been re-organized (and decrease in length) giving more emphasis on the multivariable analysis (page 6 - 9).

R1C8. Discussion (page 11 - 21): too long and very difficult to comprehend. Authors must significantly reduce this section.

Response

This section has been re-written substantially and as such limited to 188 lines (320-508). Discussion (page 9 – 13 vs. 11 – 21 in the last version of manuscript).

R1C9. References (page 22-31): too many for a journal article. Authors need to select the most relevant references for this paper. Since the text has been eliminated in length, subsequently there have been eliminated many references, including the most relevant.

Response

Indeed, after re-writing much of the Discussion, a lot of the references have now been removed, keeping only the most relevant. We keep only 62 references instead Of 95 in last version.
Reviewer's report

Version: 2 Date: 8 May 2014

Reviewer 3: Daniel Pagnin

Reviewer's report:

General Overview

The readers will benefit with a more concise text. For example, the Result section did not use the tables to illustrate points in the text, the tables were the subjects of your text.

Response

The section of the Result has been re-written in the suggested way (page 6 - 9).

Methodology

Often, univariate analysis is used to identify possible explanatory variables for Inclusion in the multivariate model. Using all independent variables in the Backward regression tends to capitalize on chance and overfit data. In this case, it is important to specify whether the explanatory variables were previously tested for interaction.

Response

We agree with the reviewer about their initial concern regarding the use of a backward technique. A similar comment was made by the third reviewer. We have now explained these limitations of the analytical approach used in the Limitations section of the manuscript. However, unlike a previous analysis of the same data focusing on the association between stressful life events and depressive symptoms [Sokratous et al. BMC Public Health 2013], the aim here was not to test specific variables based on a priori defined hypothesis. Since the intention was to reduce the set of variables from the over 30 variables considered (many of which highly intercorrelated) that best describe an increased odds of depressive symptoms in a University student population, the use of a backward technique was deemed appropriate. Furthermore, there was no a priori defined hypothesis in terms of interaction effects, and testing for such interactions was largely hindered by the large number of variables considered.

Results

An odds ratio of 1 means that both groups have a similar likelihood. Considering the 95% confidence intervals that included 1, the interpretation of some variables (children, faculty, physical health, smoking habit, drug addiction) should be reviewed in the backward regression.
Response
We like to thanks the reviewer for the comment referring to multy regression analysis. Similar comment reported also by referrer 1 (Dr Seblewengel Lemma).

In the backward regression statistical significance was 0.10 for each variables. Unfortunately, in the manuscript, there was a spelling mistake (the correct is p= 0.10 rather than p= 0.010 for statistical significance). This spelling mistake confused the reviewers for statistical invalid results (e.g. children, faculty, physical health, smoking habit, drug addiction and in table 6).

In addition, presentation of the results was amended to exclude variables with borderline statistical significance or when statistical significance was restricted to certain levels/categories of the variable, such as in the case of Faculty.

Discussion
The Discussion should reinterpret the results mentioned above. I would also suggest that the discussion be presented in a different way. The multivariate analysis should receive more emphasis. The multivariate analysis considers the interaction effects between the explanatory variables and can provide a measure of the model’s goodness-of-fit to the data.

Response
The section of the Discussion has been re-written in the suggested way, focusing on the prevalence of the clinical depressive symptoms and the interpretation of the results coming from the multivariate analysis since this provides evidence of an independent association with the outcome of interest controlling for the other variables in the model (line 320– 508).