PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Social and behavioral factors associated with depressive symptoms among university students in Cambodia: A cross-sectional study
AUTHORS	Ngin, Chanrith; Pal, Khuondyla; Tuot, Sovannary; Chhoun, Pheak; Yi, Rosa; Yi, Siyan

VERSION 1 – REVIEW

REVIEWER	Dr Laura Pass University of Reading, UK
REVIEW RETURNED	05-Mar-2018

GENERAL COMMENTS	This is an interesting paper in an area of need for further research. I feel the authors need to attend to a few areas before this can be published, as outlined below: Introduction: • What exactly does “conducts” mean in the phrase “suicidal ideation, attempts, and conducts”? • “Mental health defects” (line 97) is a very pejorative term, please rephrase to something less stigmatising. Also the suggestion that screening and intervention could prevent depression is misleading, it is much more likely to identify students who could benefit from early intervention but less certain you could offer preventative input. Method: • Line 120: Clarify what “Epi Info” means • Measures should include reference to reliability and validity for this type of population: Particularly important to highlight which measures were not validated for this group given this is mentioned as a limitation in the discussion • Include mention of how long it took to complete the questionnaires, and whether there were any incentives for participants to take part. Were they approached in person or by phone/email, and if so, how was this arranged? It’s important to clarify there were no coercive practices where participants felt obliged to take part. Results: • Need to include the % of participation: How many students were approached but declined to take part? This is needed to evaluate whether there might be a self-selection bias in participation, which could impact interpretation of results (i.e. if a significant proportion of those approached declined, it may be that depressed students were more likely to take part, so the prevalence estimates might be over-estimates) • Multiple univariate analyses reported: I suggest the p value is adjusted to account for multiple testing • What potential factors could be behind the difference in depression

	prevalence across the 2 University sites? Are there any contextual factors to be aware of, other than 1 is apparently in a rural area and 1 is in a city? This has significant implications for the Universities sampled, so should be discussed Discussion: • Consider the wider literature about gender differences in depression prevalence • Be careful when considering the relationship between sleep problems, self-perceptions and depression symptoms, as these are 2 aspects that are directly measured on the CSED so a strong correlation would be expected anyway (this holds for any associations between measures where there is overlap between items on the separate measures). This needs to be considered further in both the analyses and discussion of results with these measures. • Line 421: “This reflects scientific facts that lack of physical activities may cause blue feelings and subsequently depression”- be careful about what claims can be from a cross-sectional study • The discussion points about culture and family environments is confusing (lines 432 onwards): This section could be made much clearer and more closely related to the data from the study (e.g. the lack of social support posited presumably would only be a factor for students not living at home, and you could argue those living with relatives/friends/spouse would still have this connection) • Line 463: “ this study employed self-reported data, which might have been subject to recall bias of over-reporting and under-reporting”: Reference should be made to the interpretation and memory biases found in depressed adults, as well as how negative self-perceptions (a symptom of depression) may have influenced their responses (e.g. academic performance appears not to be substantiated by any objective measure- this could easily be a negative bias about self-performance rather than reflect a real difficulty) • Consider how opportunities to gain more objective data could be used in future (e.g. linking participant responses to University records of attainment) • Presumably an extra (and key) recommendation is to further develop and evaluate self-report questionnaires to ensure they are reliable and valid for the population you want to survey? Results can only be as robust as the measures themselves, and there appears to be a significant need to assess the quality of measures rather than continue to rely on non-validated questionnaires for this group of people

REVIEWER	Ziggi Ivan Santini The Danish National Institute of Public Health, Denmark
REVIEW RETURNED	31-Mar-2018

GENERAL COMMENTS	The authors have done a good job in investigating an important area that is under-researched in Cambodia. The paper has potential, but should undergo a major revision. Intro: 1) The opening sentence claims that university students have higher rates of mental disorders as compared to peers. I highly doubt that. The authors then go on to cite studies that only investigate students, but do not compare to non-students. Only Stewart-Brown seems to be doing this. Either way, the opening sentence appears to be claiming more than appropriate. I suggest something like “Some research has suggested that some aspects of mental health among

	university students are considerably poorer than that of their peers in the local population (REF). Methods and results: 2) Does the cesd apply in eastern countries? I'm not so sure. Please provide better references for this scale's validity in non-western settings. The authors cite a study from Japan in Japanese by Shima et al, but I'm not convinced this study even involves a validation of the cesd. If the cesd is not validated properly in Asian settings, this should be discussed as a limitation of the study. 3) Covariate: year of study. I was initially confused, as I thought this had to do with the year the current study was carried out. Please correct it to something like "year of study at university" or "academic year". I also wonder why this is important to include as a covariate. I'm not saying that it isn't, but in that case, justify with theory. 4) Why does the study use "perceived" academic performance? Doesn't the university have access to actual academic data rather than just relying on subjective perceptions of performance? If it is not possible to use real university data, this should also be discussed as a limitation, since depressed students may view their own performance in a more negative light. 5) Sleep problems is a symptom of depression, so why include this as a predictor? You would expect sleep problems to be highly correlated with depression, since sleep problems is a major symptom of depression. 6) Why are ACE's "never" and "rarely" grouped together? This means for example that "rarely" having been sexually abused as a child has the same impact as never having been exposed to such an event. However, just one occurrence can be incredibly traumatic. Similar argument for the other types of ACEs. 7) I can't follow the logic in this sentence: "To control for potential confounding factors, two multivariate logistic regression models were constructed, one for depressive symptoms and the other for severe depressive symptoms." In any case, it seems redundant. The whole reason for doing an adjusted model is to control for confounders. But doing so both for depression and severe depression does not in itself have anything to do with controlling for confounders. Also, since the authors are interested in a range of factors associated with the outcomes, they do not identify possible confounders. They only control for sociodemographics. This is fine, but in that case, it is not appropriate to say that. I would just say that the models were adjusted for sociodemographics. 8) There's considerable confusion regarding the way the authors refer to the predictors and outcomes in the study. Please make it clear throughout the text that depression is the outcome of interest. 9) Also, throughout the text, make sure to describe the findings properly. For example, the abstract reads "Students with depressive symptoms were significantly more likely to report poor academic performance". This is not correct, because academic performance is not the outcome. The proper wording should be "Students who reported poor academic performance were significantly more likely to have depression". Again, this has to be changed throughout all the text. 10) Another problem is the descriptive statistics (table 1, 2, 3, and 4), as well as the text that goes with it in the results section. Here, the authors talk about likelihood, although these are only comparisons. For example, the authors conclude from table 3 that students with depression are more likely to perceive that their health is poor. This is not correct. Here, it can only be concluded that

	students with depression had significantly worse health than students without depression. It's first in table 5 that the authors can conclude that students with poorer health are more likely to screen positive for depression or severe depression. It's important that the authors understand this difference, because if there was no difference between these two analyses, there would be no reason for doing both descriptive and analytical stats. 11) Why remove covariates with p-value > 0.05 ? Just because a covariate is not significant, doesn't mean that it is not important. Since the study aims to investigate a wide range of factors associated with depression, I suggest including all covariates in the table, both significant and non-significant. Sociodemographis can be left out if the authors choose so, as long as this is specified. I recommend that the authors read the below paper closely and follow their methods in a similar manner. Only the outcome of interest is different in this paper, but otherwise the study design and focus is similar: Sourander, Andre, et al. "Psychosocial risk factors associated with cyberbullying among adolescents: A population-based study." Archives of general psychiatry 67.7 (2010): 720-728. 12) When using the cesd cut-point, it is more appropriate to use the term "depression" and "severe depression" than "depressive symptoms". Using a cut-point means that most of those falling below the cut-point also will have some depressive symptoms, so saying that they don't have "depressive symptoms" is in a sense misleading. They probably do, but they don't have "depression". This is just a matter of terminology. In general, when the depression outcome is continuous, "depressive symptoms" is appropriate, and when the outcome is categorical, the appropriate term is "depression". 13) I couldn't find the information about the suicidal ideation variable used in the methods section. Which scale was used? Also, I wonder why this was used as a predictor? It would be expected that people with suicidal ideation are highly likely to also screen positive for depression. Why not use suicidal ideation as an outcome in the study, alongside the two depression outcomes? Since suicidal ideation is a serious adverse health indicator, closely related to depression, but not included as a symptom in the cesd, it would make a lot of sense to look at suicidal ideation as one of the primary outcomes. It would make the paper more valuable, since it would provide some info about a very serious matter. Of course, if a non-validated single-item scale was used, this should also be mentioned in the discussion. 14) I would say "blue feelings" is not a scientific term and should be avoided. I realize the survey question may have involved the wording for the sake of communicating to lay-people. However, it's not appropriate in a scientific discussion context. 15) The authors say that students with depression were more likely to encounter physical violence by a parent or guardian, etc… This is not correct. The ACEs enquired about past abuse while growing up. The proper conclusion is that students who were abused as children were more likely to screen positive for depression. 16) Depression and other mental health problems should not be referred to as "defects". Use instead politically correct terminology, like mental health problems or mental disorders. Discussion: 17) In the discussion, the authors emphasize the need for better mental health care as their primary and almost single solution to the problem at hand. While this may very well be one necessary

	solution, it is far from the only one. The provision of mental health care is extremely costly and requires trained staff that are not necessarily available. Further, treatment is not always effective, and psychiatric medication can be both ineffective and associated with a range of side-effects. I miss some discussion from the authors about what can actually be done to prevent or minimize depression among students from a structural, political, behavioral and social point of view. Especially, what can universities and policy makers do to address this problem, to encourage more mentally healthy behaviors and facilitate norms and structures that promote mental health? For example, the authors mention financial hardship as a factor that predicts mental health problems. It's not a surprise that constant worry about making ends meet can result in feelings of hopelessness and powerlessness. This factor shouldn't be medicalized. What should be done to address financial hardship and avoid that students are overly burdened by financial distress while also attempting to complete a demanding university degree? Also, the authors do well in discussing the need for healthier eating habits. However, what is the universities' role in this? Could universities make it easier for students to obtain healthier food at campus e.g a behavioral economics approach? These are just suggestions. The point is that the medical sector should not be the only sector that is held responsible for the mental health of students. Prevention is key.

VERSION 1 – AUTHOR RESPONSE

Reviewer 1: Dr Laura Pass, University of Reading, UK

2. General comments: This is an interesting paper in an area of need for further research. I feel the authors need to attend to a few areas before this can be published, as outlined below

RESPONSE: Thank you for your support and constructive comments. We have made all efforts to carefully address your important points.

Introduction:

3. What exactly does "conducts" mean in the phrase "suicidal ideation, attempts, and conducts"?

RESPONSE: "Conducts" means "commitments". We have changed it. Please see line 83.

4. "Mental health defects" (line 97) is a very pejorative term, please rephrase to something less stigmatising. Also the suggestion that screening and intervention could prevent depression is misleading, it is much more likely to identify students who could benefit from early intervention but less certain you could offer preventative input.

RESPONSE: We have changed "defects" to "problems". Line 93. We have modified it to "would enable tailor-made and early screening and intervention programs to reduce mental health problems in this population." Please see lines 91-93.

Method:

5. Line 120: Clarify what "Epi Info" means

RESPONSE: “Epi Info” is the name of the software we used. Further details were provided for clarification. Please see line 116-117.

6. Measures should include reference to reliability and validity for this type of population: Particularly important to highlight which measures were not validated for this group given this is mentioned as a limitation in the discussion

RESPONSE: Scales used to measure constructs (CES-D, ACEs, Health Behavior Survey, SF-12) in this study are well known and have been validated in different populations as cited with references, although it has not been validated among transgender women in Cambodia. This has been included in the limitations.

7. Include mention of how long it took to complete the questionnaires, and whether there were any incentives for participants to take part. Were they approached in person or by phone/email, and if so, how was this arranged? It’s important to clarify there were no coercive practices where participants felt obliged to take part.

RESPONSE: These points have been addressed in “Sampling and data collection procedure” (lines 129-132) and “Ethical considerations” (lines 217-223).

Results:

8. Need to include the % of participation: How many students were approached but declined to take part? This is needed to evaluate whether there might be a self-selection bias in participation, which could impact interpretation of results (i.e. if a significant proportion of those approached declined, it may be that depressed students were more likely to take part, so the prevalence estimates might be over-estimates)

RESPONSE: Less than 2.0% (n= 26) of the students initially selected from the name list declined participation in the study mostly due to their time constrains. They were then replaced by the next student in the list. We have added this information on lines 229-231.

9. Multiple univariate analyses reported: I suggest the p value is adjusted to account for multiple testing

RESPONSE: P-values were all adjusted.

10. What potential factors could be behind the difference in depression prevalence across the 2 University sites? Are there any contextual factors to be aware of, other than 1 is apparently in a rural area and 1 is in a city? This has significant implications for the Universities sampled, so should be discussed

RESPONSE: This important point has been discussed on lines 361-378.

Discussion:

11. Consider the wider literature about gender differences in depression prevalence

RESPONSE: The higher prevalence of depression among female students has been discussed on lines 379-387.

12. Be careful when considering the relationship between sleep problems, self-perceptions and depression symptoms, as these are 2 aspects that are directly measured on the CSED so a strong correlation would be expected anyway (this holds for any associations between measures where

there is overlap between items on the separate measures). This needs to be considered further in both the analyses and discussion of results with these measures.

RESPONSE: We have addressed this comment on lines 404-405. To also address comments from another reviewer, sleep problems have been removed from the analyses.

13. Line 421: “This reflects scientific facts that lack of physical activities may cause blue feelings and subsequently depression”- be careful about what claims can be from a cross-sectional study

RESPONSE: We have deleted this sentence to avoid over-interpretation of the finding.

14. The discussion points about culture and family environments is confusing (lines 432 onwards): This section could be made much clearer and more closely related to the data from the study (e.g. the lack of social support posited presumably would only be a factor for students not living at home, and you could argue those living with relatives/friends/spouse would still have this connection)

RESPONSE: We have modified this point to “The lack of social support from the family presumably would only be a factor for students living independently. But, for those living with relatives/friends/spouse, they would still have this support.” Please see lines 421-424.

15. Line 463: “ this study employed self-reported data, which might have been subject to recall bias of over-reporting and under-reporting”: Reference should be made to the interpretation and memory biases found in depressed adults, as well as how negative self-perceptions (a symptom of depression) may have influenced their responses (e.g. academic performance appears not to be substantiated by any objective measure- this could easily be a negative bias about self-performance rather than reflect a real difficulty)

RESPONSE: We believe that the issue of recall bias, underreporting or over-reporting apply to any populations under studies in which self-report measures are used regardless of their mental health status. We are not sure if providing references regarding memory biases found in depressed adults would help.

16. Consider how opportunities to gain more objective data could be used in future (e.g. linking participant responses to University records of attainment)

RESPONSE: To also address a comment from another reviewer, we have included this point as part of the limitations. Please see lines 448-450.

17. Presumably an extra (and key) recommendation is to further develop and evaluate self-report questionnaires to ensure they are reliable and valid for the population you want to survey? Results can only be as robust as the measures themselves, and there appears to be a significant need to assess the quality of measures rather than continue to rely on non-validated questionnaires for this group of people

RESPONSE: We have added this recommendation on lines 474-477.

Reviewer 2: Ziggi Ivan Santini, The Danish National Institute of Public Health, Denmark
18. General comments: The authors have done a good job in investigating an important area that is under-researched in Cambodia. The paper has potential, but should undergo a major revision.

RESPONSE: Thank you for your support and very important comments.

Introduction:
19. The opening sentence claims that university students have higher rates of mental disorders as compared to peers. I highly doubt that. The authors then go on to cite studies that only investigate students, but do not compare to non-students. Only Stewart-Brown seems to be doing this. Either way, the opening sentence appears to be claiming more than appropriate. I suggest something like "Some research has suggested that some aspects of mental health among university students are considerably poorer than that of their peers in the local population (REF).

RESPONSE: We have changed the sentence to "Several studies have suggested that the aspects of mental health among university students are considerably poorer than that of their peers in the general population.1-5" Please see lines 68-69.

Methods and results:
20. Does the cesd apply in eastern countries? I'm not so sure. Please provide better references for this scale's validity in non-western settings. The authors cite a study from Japan in Japanese by Shima et al, but I'm not convinced this study even involves a validation of the cesd. If the cesd is not validated properly in Asian settings, this should be discussed as a limitation of the study.

RESPONSE: This important point has been included as a main limitation of the study. Please see lines 452-454.

21. Covariate: year of study. I was initially confused, as I thought this had to do with the year the current study was carried out. Please correct it to something like "year of study at university" or "academic year". I also wonder why this is important to include as a covariate. I'm not saying that it isn't, but in that case, justify with theory.

RESPONSE: We have changed this to "academic year" as suggested. Academic year is an important factors associated with mental health among university students as found in previous studies. Students in early university life tend to have more metal health problems due to their transition from high school to university and in many cases, being away from home and family and exposure to new academic environment and burden. However, in this study, acadeic year was not related to depressive symptoms among university students.

22. Why does the study use "perceived" academic performance? Doesn't the university have access to actual academic data rather than just relying on subjective perceptions of performance? If it is not possible to use real university data, this should also be discussed as a limitation, since depressed students may view their own performance in a more negative light.

RESPONSE: We initially planned to collect academic performance from school record. However, given the complexity and concerns from the school administrators over the confidentiality of the data, we decided to use perceived performance as an alternate. This has been included in the limitations. Please see lines 448-450.

23. Sleep problems is a symptom of depression, so why include this as a predictor? You would expect sleep problems to be highly correlated with depression, since sleep problems is a major symptom of depression.

RESPONSE: Thank you for this important comment. We have removed 'problems with sleeping' from both bi-variate and multivariate analyses.

24. Why are ACE's "never" and "rarely" grouped together? This means for example that "rarely" having been sexually abused as a child has the same impact as never having been exposed to such an event. However, just one occurrence can be incredibly traumatic. Similar argument for the other types of ACEs.

RESPONSE: The description was actually used in the study protocol. However, during the consultative meeting, we were suggested by a technical working group to use only binary response option – no and yes. This has been revised accordingly. Please see lines 186-189.

25. I can't follow the logic in this sentence: "To control for potential confounding factors, two multivariate logistic regression models were constructed, one for depressive symptoms and the other for severe depressive symptoms." In any case, it seems redundant. The whole reason for doing an adjusted model is to control for confounders. But doing so both for depression and severe depression does not in itself have anything to do with controlling for confounders. Also, since the authors are interested in a range of factors associated with the outcomes, they do not identify possible confounders. They only control for sociodemographics. This is fine, but in that case, it is not appropriate to say that. I would just say that the models were adjusted for sociodemographics.

RESPONSE: We wanted to say that two sets of analyses were conducted separately for the two outcomes – depressive symptoms and severe depressive symptoms – as presented in the tables. To avoid the confusion, this sentence has been removed.

26. There's considerable confusion regarding the way the authors refer to the predictors and outcomes in the study. Please make it clear throughout the text that depression is the outcome of interest.

RESPONSE: As suggested, we have revised the wording where necessary to avoid the confusions.

27. Also, throughout the text, make sure to describe the findings properly. For example, the abstract reads "Students with depressive symptoms were significantly more likely to report poor academic performance". This is not correct, because academic performance is not the outcome. The proper wording should be "Students who reported poor academic performance were significantly more likely to have depression". Again, this has to be changed throughout all the text.

RESPONSE: We have revised the description throughout the text.

28. Another problem is the descriptive statistics (table 1, 2, 3, and 4), as well as the text that goes with it in the results section. Here, the authors talk about likelihood, although these are only comparisons. For example, the authors conclude from table 3 that students with depression are more likely to perceive that their health is poor. This is not correct. Here, it can only be concluded that students with depression had significantly worse health than students without depression. It's first in table 5 that the authors can conclude that students with poorer health are more likely to screen positive for depression or severe depression. It's important that the authors understand this difference, because if there was no difference between these two analyses, there would be no reason for doing both descriptive and analytical stats.

RESPONSE: Thank you for this very important comments. We have revised the wording to address the concern.

29. Why remove covariates with p-value > 0.05 ? Just because a covariate is not significant, doesn't mean that it is not important. Since the study aims to investigate a wide range of factors associated with depression, I suggest including all covariates in the table, both significant and non-

significant. Sociodemographis can be left out if the authors choose so, as long as this is specified. I recommend that the authors read the below paper closely and follow their methods in a similar manner. Only the outcome of interest is different in this paper, but otherwise the study design and focus is similar:
Sourander, Andre, et al. "Psychosocial risk factors associated with cyberbullying among adolescents: A population-based study." Archives of general psychiatry 67.7 (2010): 720-728.

RESPONSE: We employed stepwise approch in regression analyses to identify variables more strongly associated with depressive symptoms given a long list of potential confounders detected in bivariate analyses. We believe that the unadjusted data presented in bivariate tables would provide sufficient information for the readers to justify the importance of a wide range of factors in different domains that may be related to depression in different contexts.

30. When using the cesd cut-point, it is more appropriate to use the term “depression” and “severe depression” than “depressive symptoms”. Using a cut-point means that most of those falling below the cut-point also will have some depressive symptoms, so saying that they don’t have “depressive symptoms” is in a sense misleading. They probably do, but they don’t have “depression”. This is just a matter of terminology. In general, when the depression outcome is continuous, “depressive symptoms” is appropriate, and when the outcome is categorical, the appropriate term is “depression”.

RESPONSE: In our understanding, CES-D is used to screen depressive symptoms, not to diagnose depression. In most of the previous studies, if not all, in which CES-D was used, the term ‘depressive symptoms’ have been used.

31. I couldn’t find the information about the suicidal ideation variable used in the methods section. Which scale was used? Also, I wonder why this was used as a predictor? It would be expected that people with suicidal ideation are highly likely to also screen positive for depression. Why not use suicidal ideation as an outcome in the study, alongside the two depression outcomes? Since suicidal ideation is a serious adverse health indicator, closely related to depression, but not included as a symptom in the cesd, it would make a lot of sense to look at suicidal ideation as one of the primary outcomes. It would make the paper more valuable, since it would provide some info about a very serious matter. Of course, if a non-validated single-item scale was used, this should also be mentioned in the discussion.

RESPONSE: Suicidal ideation and attempt were simply measured by using two yes/no questions to ask whether the participant had thought about ending or attempted to end his/her life. We have now removed these variables from both bivariate and multivariate tables. We have conducted a separate analysis to explore factors associated with suicidal ideation in the same population.

32. I would say “blue feelings” is not a scientific term and should be avoided. I realize the survey question may have involved the wording for the sake of communicating to lay-people. However, it’s not appropriate in a scientific discussion context.

RESPONSE: We have deleted this sentence as it implies causation.

33. The authors say that students with depression were more likely to encounter physical violence by a parent or guardian, etc… This is not correct. The ACEs enquired about past abuse while growing up. The proper conclusion is that students who were abused as children were more likely to screen positive for depression.

RESPONSE: We have revised the wording throughout the text as suggested.

34. Depression and other mental health problems should not be referred to as “defects”. Use instead politically correct terminology, like mental health problems or mental disorders.

RESPONSE: We have changed “defects” to “problems”.

Discussion:
35. In the discussion, the authors emphasize the need for better mental health care as their primary and almost single solution to the problem at hand. While this may very well be one necessary solution, it is far from the only one. The provision of mental health care is extremely costly and requires trained staff that are not necessarily available. Further, treatment is not always effective, and psychiatric medication can be both ineffective and associated with a range of side-effects. I miss some discussion from the authors about what can actually be done to prevent or minimize depression among students from a structural, political, behavioral and social point of view. Especially, what can universities and policy makers do to address this problem, to encourage more mentally healthy behaviors and facilitate norms and structures that promote mental health? For example, the authors mention financial hardship as a factor that predicts mental health problems. It’s not a surprise that constant worry about making ends meet can result in feelings of hopelessness and powerlessness. This factor shouldn’t be medicalized. What should be done to address financial hardship and avoid that students are overly burdened by financial distress while also attempting to complete a demanding university degree? Also, the authors do well in discussing the need for healthier eating habits. However, what is the universities’ role in this? Could universities make it easier for students to obtain healthier food at campus e.g a behavioral economics approach? These are just suggestions. The point is that the medical sector should not be the only sector that is held responsible for the mental health of students. Prevention is key.

RESPONSE: We have added: “Further to medical care, universities should provide measures, such as student loans and healthy canteens, to mitigate some key predictors of depression among students, such as financial hardship and poor diets.” Please see lines 435-437.

VERSION 2 – REVIEW

REVIEWER	Dr Laura Pass University of Reading, UK
REVIEW RETURNED	10-Jun-2018

GENERAL COMMENTS	There have been improvements to the manuscript but it would be much more helpful to have a summary indicating how the reviewer's comments have been dealt with, and where this is shown in the text. It is not clear whether the marked changes relate to reviewer comments, and which ones/how the authors have interpreted the comments. I think there are still some outstanding issues to resolve before publication, outlines below. Abstract: - Primary outcome measure is CES-D, but this is reported in association with other measures- are you able to mention these in the abstract? I think it needs to be clearer that all the other measures are also self-report as this is not obvious when reading the abstract

	Introduction: - "suicidal ideation, attempts, and commitments": I'm still unclear what this means, are you referring to completed suicides? If so, the description 'deaths by suicide' is preferable Measures: - Are the adapted questionnaires available on request? This would increase transparency, and help promote further research validating these in Cambodia. I suggest this is added in (e.g. "copies of the questionnaires are available from the corresponding author on request"). Results: - Desert should read dessert (i.e. pudding) I think? Discussion: - Need to discuss the depression symptom prevalence at the start, and how this compares to any other research about prevalence of depression symptoms in Cambodia (especially as this is given as the primary outcome variable in the abstract) - Gender difference in prevalence needs to be more cautiously discussed: There is a gender difference across almost all research I know in depression, so the findings here may not be related to University life at all - This sentence needs further explanation for a non-Cambodian audience to understand why this is different for women than men: "In the Cambodian culture, young women would perceive a great deal of challenges when living away from their family or parents since they need to maintain the cultural behaviors and meanwhile cope with independent habitation" - Possibility of physical health problems being the cause of depression needs to be considered more explicitly: If students have health conditions that impair their ability to be physically active, the suggestion of promoting physical activity would be inappropriate. Also very likely students who are suffering from long-term conditions would struggle emotionally- overall the authors need to consider creating a more balanced view of the results in the discussion, given that correlation cannot infer causation. - Discussion of mental health resources in Cambodia should be put into context by relating this to the population (e.g. 1 Psychiatrist for every xx person in the country) - Limitation of self-report measures should also include recognition of the negative cognitive biases associated with depression, as well as possible recall bias (e.g. interpretation of events too, such as whether what they are eating is healthy, are they happy with their weight, their view on how their family treats them etc) - Highlight how Universities themselves can play a huge role in taking this research forwards, and the benefits this would provide them (i.e. research output while at the same time benefitting their students which would likely improve their academic outcomes)

REVIEWER	Ziggi Ivan Santini The Danish National Institute of Public Health
REVIEW RETURNED	19-May-2018

GENERAL COMMENTS	Please make sure to proofread the whole paper again, and do so with the help of a native english speaker. This is important. The papers reads ok, but there are still some errors throughout the text (e.g. "activeness", "findings confirms").

	Also, I could not find the response to reviewer comments. It is customary to respond to reviewer comments. Don't forget that next time. Reviewers want to know what you have changed, what you haven't, and why. The revised manuscript is not enough.

VERSION 2 – AUTHOR RESPONSE

Reviewer 1: Dr Laura Pass, University of Reading, UK

1. General comments: There have been improvements to the manuscript but it would be much more helpful to have a summary indicating how the reviewer's comments have been dealt with, and where this is shown in the text. It is not clear whether the marked changes relate to reviewer comments, and which ones/how the authors have interpreted the comments. I think there are still some outstanding issues to resolve before publication, outlines below.

RESPONSE: We apologize for this great mistake. We doubted that something went wrong during the submission. To avoid this, we would double check the submission to ensure that it is complete with point-by-point response to reviewers included and revised parts highlighted. We have addressed the reviewer's comments very carefully.

2. Abstract:

Primary outcome measure is CES-D, but this is reported in association with other measures- are you able to mention these in the abstract? I think it needs to be clearer that all the other measures are also self-report as this is not obvious when reading the abstract

RESPONSE: The recommended structure of the abstract in BMJ Open allows us to present only primary outcome variable.

We have indicated that 'All measures in the study were self-reported.' Please see line 40.

3. Introduction:

"Suicidal ideation, attempts, and commitments": I'm still unclear what this means, are you referring to completed suicides? If so, the description 'deaths by suicide' is preferable

RESPONSE: We have changed it to 'suicidal ideation and attempts' to avoid confusions. Please see line 81.

4. Measures:

Are the adapted questionnaires available on request? This would increase transparency, and help promote further research validating these in Cambodia. I suggest this is added in (e.g. "copies of the questionnaires are available from the corresponding author on request").

RESPONSE: We have added this sentence in the "Questionnaire development and training." Please see lines 148-149.

5. Results:

Desert should read dessert (i.e. pudding) I think?

RESPONSE: Corrected.

6. Discussion:

Need to discuss the depression symptom prevalence at the start, and how this compares to any other research about prevalence of depression symptoms in Cambodia (especially as this is given as the primary outcome variable in the abstract)

RESPONSE: Thank you for this important comment. We have added the prevalence of depressive symptoms and severe depressive symptoms among university students in this study. Please see lines 360-361. However, due to the scarcity of studies in this topic in Cambodia with different study populations and measurements of depression or depressive symptoms, comparison of findings in this study with those from previous studies in Cambodia was not possible.

7. Gender difference in prevalence needs to be more cautiously discussed: There is a gender difference across almost all research I know in depression, so the findings here may not be related to University life at all. This sentence needs further explanation for a non-Cambodian audience to understand why this is different for women than men: "In the Cambodian culture, young women would perceive a great deal of challenges when living away from their family or parents since they need to maintain the cultural behaviors and meanwhile cope with independent habitation"

RESPONSE: The relationship between gender and depressive symptoms did not remain significant after controlling for other variable in multivariate logistic regression model. Therefore, to avoid over-interpretation and reduce the volume of the paper as suggested by other reviewers, we have removed this entire paragraph from the paper.

8. Discussion of mental health resources in Cambodia should be put into context by relating this to the population (e.g. 1 Psychiatrist for every xx person in the country)

RESPONSE: We have revised the part as below:

“In 2012, Cambodia had only 49 trained psychiatrists and 45 psychiatric nurses working in mental health facilities and private practices for a population of approximately 15 million. This number equates to approximately 0.2 psychiatrists per 100,000 population, which is similar to the average in Southeast Asia.” Please see lines 424-428.

10. Limitation of self-report measures should also include recognition of the negative cognitive biases associated with depression, as well as possible recall bias (e.g. interpretation of events too, such as whether what they are eating is healthy, are they happy with their weight, their view on how their family treats them etc)

RESPONSE: We have revised the part as below:

“This study employed self-reported data, which might have been subject to over-reporting and under-reporting caused by the negative cognitive biases associated with depression as well as possible recall bias.” Please see lines 444-447.

11. Highlight how Universities themselves can play a huge role in taking this research forwards, and the benefits this would provide them (i.e. research output while at the same time benefitting their students which would likely improve their academic outcomes) RESPONSE: We have added the following to the recommendations:

“Universities could play very important roles in taking this research forwards by providing future research outputs to improve mental health of the students that would in turn improve their academic outcomes.” Please see lines 470-472.

Reviewer 2: Ziggi Ivan Santini, The Danish National Institute of Public Health

12. Please make sure to proofread the whole paper again, and do so with the help of a native english speaker. This is important. The papers reads ok, but there are still some errors throughout the text (e.g. "activeness", "findings confirms"). Also, I could not find the response to reviewer comments. It is customary to respond to reviewer comments. Don't forget that next time. Reviewers want to know what you have changed, what you haven't, and why. The revised manuscript is not enough.

RESPONSE: We apologize for this great mistake. We doubted that something went wrong during the submission. To avoid this, we would double check the submission to ensure that it is complete with point-by-point response to reviewers included and revised parts highlighted. This has been carefully proofread by a native English speaker.