

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Prevalence and Correlates of Sleep Disturbance and Depressive symptoms Among Chinese Adolescents: a cross-sectional survey study
AUTHORS	Lan, Guo; Jianxiong, Deng; Yuan, He; Xueqing, Deng; Jinghui, Huang; Guoliang, Huang; Xue, Gao; Lu, CiYong

VERSION 1 - REVIEW

REVIEWER	Serge Brand Psychiatric Hospital of the University of Basel, Basel, Switzerland
REVIEW RETURNED	17-May-2014

GENERAL COMMENTS	<p>The authors investigated the relation between sleep and psychological functioning among a large sample of Chinese children and adolescents. The key findings were that a greater amount of participants complained about poor sleep, along with greater psychological difficulties.</p> <p>The study has been very well conducted; the sample size is amazing; references are timely, though, as regards sleep of adolescents in relation to family functioning, please add Kalak et al 2012 and Bajoghli et al 2013. The tools are very well chosen in that the questionnaires are internationally accepted, established and validated. To strengthen the manuscript, I suggest to formulate some study questions/hypotheses and to state to what extent the present study does add to the current literature. P8 I5: OR 1.27 is not significant ('1' within the confidence interval), p8 I13: 3.17: please add CI.</p> <p>References Kalak N, Gerber M, Kirov R, Mikoteit T, Pühse U, Holsboer-Trachsler E, Brand S. The relation of objective sleep patterns, depressive symptoms, and sleep disturbances in adolescent children and their parents: a sleep-EEG study with 47 families. <i>J Psychiatr Res.</i> 2012 Oct;46(10):1374-82. doi: 10.1016/j.jpsychires.2012.07.006. Bajoghli H, Alipouri A, Holsboer-Trachsler E, Brand S. Sleep patterns and psychological functioning in families in northeastern Iran; evidence for similarities between adolescent children and their parents. <i>J Adolesc.</i> 2013 Dec;36(6):1103-13. doi: 10.1016/j.adolescence.2013.08.016.</p>
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REVIEWER	Caitlin Murray, MA and Grayson Holmbeck, PHD Loyola University Chicago Chicago, IL USA
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GENERAL COMMENTS

The purpose of ms 2014-005517 was to examine prevalence and predictors of sleep disturbance and depressive symptoms in Chinese adolescents. The manuscript has several strengths: (1) the large sample size, (2) the examination of several relevant predictors, and (3) the examination of classroom-level effects.

On the other hand, we had several concerns with this manuscript:

1. The authors did not just study adolescents. Children are ages 9 to 21, which crosses several developmental stages.

2. Terminology for sleep disturbance is often used interchangeable with sleep quality, insomnia, etc. which makes reading and comprehension of this manuscript difficult. The terminology needs to be consistent.

3. The introduction mixes adult, adolescent, Western and Eastern literature without a clear indication of how this study builds on previous research. The introduction also does not delineate the studies that have been China (as stated in the discussion), which also makes it difficult to understand how this study is a unique contribution.

4. Similarly, a compelling rationale is not provided for conducting this study in China. The authors should provide clear rationale for why sleep and depression may vary according to Chinese ethnicity, and whether this study contributes beyond those that have already been conducted. It is not enough to say that there is not enough research- the authors need to provide a compelling rationale for why this study is important.

5. This is a cross-sectional study with a single respondent. As such, the authors are not able to rule out common method variance interpretations for the findings.

6. Many of the measures are single items; this strategy calls into question the degree to which the measures have adequate content validity. Additionally, no clear rationale is provided for the set of predictors included.

7. The authors are encouraged to explain how the Chinese version of the PSQI is different from its original form, and cite a study that has established its validity and reliability. It is not enough to say that this measure is commonly used and "adapted to the realities of China."

8. The CES-D has different cut-off scores for males and females. Why wasn't this used?

9. The CES-D is also not validated for youth younger than 13 years old. Similarly, were separate adolescent (13-17) and adult (18-64) versions used according to age? I also do not believe that you can use the PSQI in individuals younger than 18.

10. Under study design and participants, the authors state that the participants were all middle school students, which is not true.

	<p>11. The authors are studying depressive symptoms, not depression.</p> <p>12. I recommend that a native speaker of English review the paper for word choice and grammar.</p> <p>13. The participation rate is very high, given that active parental consent was employed. Did the researchers actually receive over 99% of consent forms back from parents?</p> <p>14. I did not understand why the DVs were dichotomized, given the loss of information/variability inherent in such a strategy. In general, the statistical analyses section was extremely hard to follow.</p> <p>15. The authors use sleep as an IV in the depressive symptoms analysis and depressive symptoms as an IV in the sleep analyses. These analyses are redundant in a cross-sectional study. In addition, because the study is cross-sectional, the authors cannot make assumptions about directionality (e.g., page 10, lines 9-10).</p> <p>16. I did not understand what the authors meant by “to minimize subjectivity, students were provided with a detailed definition of the information about the PSQI and CES-D” (page 10, lines 18-19).</p>
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VERSION 1 – AUTHOR RESPONSE

Review 1

Comment 1: As regard sleep of adolescents in relation to family functioning, please add Kalak et al 2012 and Bajoghli et al 2013 .

Response 1: As requested, we have added the two references in the Introduction (Page 5, lines 9-10 and the Discussion (Page 13, lines 8-11) to strengthen the discussion of sleep in adolescents and family functioning.

Comment 2: Please formulate some study questions/ hypotheses and state to what extent the present study does add to the current literature.

Response 2: Thank you for your kind suggestion. We have formulated three hypotheses in the revised Introduction (Page 6, lines 1-7) to make our study purpose and expected outcomes more clear. Furthermore, we have cited several similar studies 1-4 in the Introduction to summarize the present research progress in China, and we have adjusted the structure and contents of the Introduction to make it more logical (Pages 4-6).

Comment 3: The OR=1.27 in Page 8, Line 5 is not significant ('1' within the confidence interval).

Response 3 : Thank you for carefully and patiently reviewing our manuscript, and noticing this mistake in the original manuscript. We have re-examined the manuscript and re-analyzed the data. That sentence has been changed to “Adolescents with below-average family relationships (AOR=1.54, 95% CI=1.06-2.26) had a slightly higher probability of sleep disturbance” in the revised manuscript (Page 11, lines 9-11).

Comment 4: Please add 95% Confidence interval (CI) of the OR=3.17 in Page 8, Line 13.

Response 4: We have changed that sentence to “...students who had depressive symptoms were

2.47 (95% CI=1.61-3.79) times more likely to suffer from sleep disturbance than those who did not.” (Page 11, lines 20-22)

Review 2

Comment 1: The authors did not just study adolescents. Children are ages 9 to 21, which cross several developmental stages.

Response 1: We thank the reviewer for these comments. We have excluded the sampled students who were not 13 to 18 years old. This information has been added to our revised manuscript (Page 9, lines 19-21).

Comment 2: Terminology for sleep disturbance is often used interchangeable with sleep quality, insomnia, etc. which makes reading and comprehension of this manuscript difficult. The terminology needs to be consistent.

Response 2: Based on this suggestion, we have unified the terminology as “sleep disturbance” to make the paper more clear and understandable.

Comment 3: The introduction mixes adult, adolescent, Western and Eastern literature without a clear indication of how this study builds on previous research. The introduction also does not delineate the studies have been China (as stated in the discussion), which also make it difficult to understand how this study is a unique contribution.

Response 3: We thank the reviewer for this suggestion. We have adjusted the structure and contents of the Introduction to make it more logical, and we have cited several similar studies in the Introduction to explain the context of the present study in China.1-4 Furthermore, we have clearly stated the purposes of our study: to estimate the prevalence of sleep disturbance and depressive symptoms, to comprehensively examine the possible factors contributing to sleep disturbance and depressive symptoms (among the demographic, school, family, and psychosocial domains), and to discuss the link between sleep disturbance and depressive symptoms. We have added three hypotheses in the Introduction to clarify the importance of our study (Pages 4-6).

Comment 4: The authors should provide clear rationale for why sleep and depressive symptoms may vary according to Chinese ethnicity, and whether this study contributes beyond those that have already been conducted.

Response 4: Thank you for your kind suggestion. Because 99% of the students involved were of Han ethnicity, we did not analyze the different Chinese ethnicities in this paper. We have deleted this sentence. We think that the revised Introduction clearly summarizes the different results among studies in different countries, showing that they have not reported uniform results about sleep disturbance and depressive symptoms (Pages 4-6).

In addition, we have adjusted the structure and contents of the Discussion to make it flow more logically. We have added several similar studies to compare with our study, 2 5-10 and their results are largely consistent with ours. Our study reports more representative results about the current prevalence of sleep disturbance and depressive symptoms among Chinese adolescents, detected more comprehensive contributing factors to sleep disturbance and depressive symptoms, and clearly showed there was a link between sleep disturbance and depressive symptoms (Pages 12-15).

Comment 5: This is a cross-sectional study with a single respondent. As such, the authors are not able to rule out common method variance interpretations for the findings.

Response 5: We entirely agree with the reviewer that a cross-sectional study cannot rule out common-method variance interpretations for the findings. Unfortunately, we are unable to correct this in the current study, but we will adopt other study designs in our future research. This limitation has been added to the Discussion (Page 14, lines 22-25).

Comment 6: Many of the measures are single items; this strategy calls into question the degree to which the measures have adequate content validity. Additionally, no clear rationale is provided for the set of predictors included.

Response 6: We thank the reviewer for these comments. We chose those potential risk factors based on a review of the literature, so we think the measures have adequate content validity. Additionally, most of the measures of independent variables were designed according to the real status of an adolescent, meaning there is no correct way to evaluate the content validity.

Comment 7: The authors are encouraged to explain how the Chinese version of the PSQI is different from its original form, and cite a study that has established its validity and reliability.

Response 7: We apologize for the incomplete information about CPSQI in the original manuscript. We have explained there is no substantive difference between the Chinese version of Sleep Quality Index (CPSQI) and the original form. The CPSQI was translated into Mandarin Chinese to better correspond to the meaning of the original, and it has been shown to be valid and reliable and is commonly used (Page 8, lines 12-14). Furthermore, we added a reference about the CPSQI to emphasize that it is not different from the original in structure or content. 11

Comment 8: The CES-D has different cut-off scores for males and females. Why wasn't this used?

Response 8: The original recommended cut-off score for having depressive symptoms was 16 points (corresponding to the 80th percentile) by the founder of the CES-D in 1977, and the original cut-off score was uniform for males and females. 12 With the changing times and social development, there have been no definitely uniform cut-off scores of the CES-D worldwide, and many researchers have adopted different cut-off scores for having depressive symptoms. 13 14 Furthermore, most of the previous studies did not provide definitely recommended cut-off scores for males and females, 13 15-17 except that a study in 1991 by Garrison referred to the different cut-off scores. 18 In our study, we adopted the 80th percentile (a score greater than 28 indicating "having depressive symptoms") as the cut-off score, and the area under the ROC curve was 0.78 (Page 8, lines 21-27). We have provided a statistical analysis of the gender difference in CES-D scores, which showed that the difference was not significant ($P>0.05$) (Page 10, lines 19-21). Therefore, we used a uniform cut-off score for boys and girls. Considering our research is an epidemiological study using a questionnaire, there existed measuring error about the presence of depressive symptoms, so we will add clinical objective diagnoses of depressive symptoms in a future study.

Comment 9: The CES-D is also not validated for youth younger than 13 years old. Similarly, were separate adolescent (13-17) and adult (18-64) versions used according to age? I also do not believe that you can use the PSQI in individuals younger than 18.

Response 9: As requested, we have excluded the sampled students who were not 13 to 18 years old, and this information have been added in our revised manuscript (Page 9, lines 20-21). Thus, the adolescents' version of CES-D is validated for the participants in our study.

As for the use of the PSQI, we referred to previous studies that used the PSQI to evaluate sleep among adolescents, 19-24 and many Chinese studies also used the PSQI to evaluate adolescents' sleep. 25 26 Given a lack of consensus when defining and measuring sleep among adolescents, we used the PSQI. These previous studies demonstrate that the PSQI may be applicable to the population we studied, although further studies are required to confirm it. Furthermore, we have added that the questionnaires involved in our study were completed and qualified for our survey in the Results section, so the contents of PSQI filled out by the students confirm the practical situation (Page 9, lines 19-20).

Comment 10: Under study design and participants, the authors state that the participants were all middle school students, which is not true.

Response: In our study, all the participants were students sampled from grades 7-9 (junior high school) and grades 10-12 (high school). We apologize for the misunderstanding caused by our unclear descriptions, and we have corrected "middle school students" students to "high school students" in our revised manuscript (Page 6, lines 15).

Comment 11: The authors are studying depressive symptoms, not depression.

Response 11: Thank you for noticing this. We have replaced "depression" with "depressive symptoms" throughout the revised manuscript.

Comment 12: I recommend that a native speaker of English review the paper for word choice and grammar.

Response 12: According to the reviewer's suggestion, we have invited a native English speaker to revise the whole manuscript carefully. We hope that the language is now acceptable for the next review process.

Comment 13: The participation rate is very high, given that active parental was employed. Did the researchers actually receive over 99% of consent forms back from parents?

Response 13: Yes. The written consent letters were obtained from the school, from each participating student and from one of student's parents after our researcher had informed them of the purpose of our study. We have clarified this point in the subsection on data collection (Page 6, lines 25-28). Furthermore, we must gratefully acknowledge the contribution of the Guangdong Education Bureau, its participating schools, and the teachers of the participating schools to help use receive over 99% of the consent forms from the parents without pay; we have expressed great thanks in the Acknowledgments section (Page 15, lines 20-22).

Comment 14: I did not understand why the DVs were dichotomized, given the loss of information/variability inherent in such a strategy. In general, the statistical analyses section was extremely hard to follow.

Response 14: Thank you for your kind suggestion. First of all, our main purpose is to investigate the prevalence and correlates of depressive symptoms among Chinese adolescents, so the variable of depressive symptoms was categorized into two levels, presence and absence, to estimate the prevalence of adolescents with depressive symptoms. We used logistic regression analysis to explore the potentially contributing factors to depressive symptoms, and this analysis required the dependent variable be categorized. We agree that we will lose information by adopting the strategy of dichotomizing the DVs, and we would like to address this issue in our future studies.

In addition, according to the reviewer's suggestion, we have improved the statistical analysis section to make it more understandable (Page 9, lines 11-15). We used a cross-sectional survey utilizing the stratified-cluster random-sampling technique for subject recruitment. Hence, students were clustered in classes. Because the sample was generated as a clustered sample, analyses of data took into consideration the clustering effects of classes, so we adopted multi-level analyses (the generalized linear mixed-effects models adopting the GLMMIX procedure in SAS) in which classes were treated as clusters in the multivariate logistic regression analyses.

Comment 15: The authors use sleep as an IV in the depressive symptoms analysis and depressive symptoms as an IV in the sleep analyses. These analyses are redundant in a cross-sectional study. In addition, because the study is cross-sectional, the authors cannot make assumptions about directionally. (e.g., page 10, lines 9-10)

Response 15: The reviewer is correct. We know that the data are cross-sectional, so no causal inference can be made regarding the observed relationships between sleep disturbance and depressive symptoms, and that was an expression error in the original manuscript. We have deleted the sentence "we observed that depression can lead to sleep disturbances, and vice versa", and clearly written that there was a link between sleep disturbance and depressive symptoms, whose direction was difficult to determine due to the nature of this cross-sectional study (Page 14, lines 11-15).

Comment 16: I did not understand what the authors meant by "to minimize subjectivity, students were provided with a definition of the information about the PSQI and CES-D" (Page 10, lines 18-19).

Response 16: We apologize for the misunderstanding caused by our unclear statements. This statement has been clarified as, "To minimize incorrect or unavailable data given by students who did not fully understand the contents of the PSQI and the CES-D, we have provided a detailed explanation of the PSQI and CES-D" (Page 15, lines 1-3).

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