

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Anxiety and Depressive Symptoms are Associated with Poor Sleep Health During a Period of COVID-19 Induced Nation-wide Lockdown: A Cross-Sectional Analysis of Adults in Jordan
AUTHORS	Al-Ajlouni, Yazan; Park, Su Hyun; Alawa, Jude; Shamaileh, Ghaith; Bawab, Aziz; El-Sadr, Wafaa M.; Duncan, Dustin

VERSION 1 – REVIEW

REVIEWER	Rosalind Elliott Northern Sydney Local Health District
REVIEW RETURNED	06-Jul-2020

GENERAL COMMENTS	<p>Please state which Facebook page the original invitation to participants was posted. Presumably 'snow ball' recruitment occurred but this is not clear. Was the original post able to be shared by anyone?</p> <p>Please provide brief details about the population size of the regions that the FB post reached and the estimated proportion of Jordanians who use FB / internet social media.</p> <p>This sentence is not clear and I recommend you rewrite it, 'The post was advertised for a period of four consecutive days and included assessment of eligibility and a total of 30 items including demographics and measures for various constructs.' The post was an advertisement for a survey.</p> <p>Please explain the rationale for using only 3 items from the PSQI. Sleep is complexly subjective and all factors which characterise sleep interrelate when considering sleep quality. This is why the composite score for PSQI is considered reliable and valid. I am not convinced by the approach for the analysis used to address the hypothesis I think your models would be better specified if the composite PSQI score was used. Please note: My recommendation and reviewer report was made with the assumption that you took advice from a sleep expert researcher with regard to this. If you have not already requested the input of a sleep (particularly with regard to the PSQI) research expert I recommend that you do this.</p> <p>Present 'n' in parentheses when reporting percentages.</p>
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REVIEWER	A.F IUMS-IRAN
REVIEW RETURNED	16-Jul-2020

GENERAL COMMENTS	<p>Authors investigated the association of anxiety and depressive symptoms with sleep quality during COVID-19 outbreak in a country located in middle east</p> <p>The following points are suggested</p> <p>Title: I am not agree with the title focusing on middle east, they</p>
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	<p>conducted study in Jordan not in all around the middle east, then please revise it in which title reflects the study population and content accurately.</p> <p>Abstract</p> <ul style="list-style-type: none"> - First subsection should be reduced effectively for presenting the background more sound and relevant, the first sentence is not relevant. - Surprisingly the number of participants has not been reported. The study design name should be presented in methods section. study instruments for evaluating of main variables should be introduced. - Please follow a same manner for presenting the results in results section, the presented aRR for depression should be presented along with 95%CI such as those you presented for anxiety. - The second and third points in strengths and limitation need concerns. <p>Introduction</p> <ul style="list-style-type: none"> - The second and third paragraphs are not relevant and some sections from these should be transferred to methods. You can present some data based on relevant literature about the effects of lockdown on mental health. - Please provide more relevant literature about the association of mental health and sleep health particularly in the context of lockdown. - The sentences started with "Research has shown that sleep" To the end of paragraph are not suitable for covering the current study content and objective. - Generally the introduction section has bot been structured relevantly, it needs major concerns. - The sentences started with "This study is one of the first to examine ..." to the end of paragraph are repetitive, these sentence have been presented in previous sentences! <p>Methods</p> <ul style="list-style-type: none"> - How about the study design name? - How about the some relevant and important variables regarding mental health and sleep health such education, marital status in demographic variables domain? - Please present more relevant data about validity and reliability of used instruments. Particularly among Jordanian population. - First sentence in statistical analysis is repetitive. - I am not agreed with the used main statistical method i.e. Poisson regression! Binary logistic regression should be used. If you used Poisson regression why "P-values for trend were calculated using logistic regression." This is wrong approach; you should use logistic and in its context p for trend should be provided. Please be clear about the categories of all three outcomes and define their categories clearly in this section. some other statistical tests are needed according to my comments you will see in results section. please present them in this section during revision. <p>Results</p> <ul style="list-style-type: none"> - Major defect in results is that your data in table 1 have been presented irrelevantly, you should present the data on demographic and predictors (mental health) between categories of three outcome (sleep), however you have presented outcomes across categories of demographic and predictors. - Please present the mean age and scores of mental health problems and compare them between categories outcomes (sleep variables) by using relevant statistical tests. - As stated you should use binary logistic regression. Please present both crude OR and adjusted based on, then you can present p for trend too! Yes!
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	<p>- Results section should be revised and rewritten based on above points.</p> <p>Conclusion</p> <p>- As stated regarding the some irrelevant literature in introduction, please consider this point in discussion section too.</p> <p>- Many variables such education, marital statues, BMI, PHYSICAL ACTIVITY in current study have not been considered as cofounding variables. These variables are associated with both predictors and outcomes you studied them! they are important confounder you missed them</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer #1

- 1. Please state which Facebook page the original invitation to participants was posted. Presumably 'snowball' recruitment occurred but this is not clear. Was the original post able to be shared by anyone?**

Response: We thank the reviewer for this note and have added details regarding this in the methodology section (page 5). An independent research assistant worked with the team by sharing the post on their timeline and then advertising the post to the public. Hence, individuals recruited from the study were presented with an independent advertised (sponsored) post. The link for the post was open to the public to additionally enable snowball sampling.

- 2. Please provide brief details about the population size of the regions that the FB post reached and the estimated proportion of Jordanians who use FB / internet social media.**

Response: We thank the reviewer for this valuable comment. Unfortunately, we do not have data regarding the reach of the post in general or specific to regions within the country. However, data regarding the general prevalence of Facebook use among Jordanians has been added on page 5.

- 3. This sentence is not clear and I recommend you rewrite it, 'The post was advertised for a period of four consecutive days and included assessment of eligibility and a total of 30 items including demographics and measures for various constructs.' The post was an advertisement for a survey.**

Response: We thank the reviewer for this comment. We have now made this change and now added more details in the methods section regarding the advertisement of the post, which should allow this sentence to be clear to the reader (page 5). Thank you for this suggestion.

- 4. Please explain the rationale for using only 3 items from the PSQI. Sleep is complexly subjective and all factors which characterize sleep interrelate when considering sleep quality. This is why the composite score for PSQI is considered reliable and valid. I am not convinced by the approach for the analysis used to address the hypothesis I think your models would be better specified if the composite PSQI score was used. Please note: My recommendation and reviewer report was made with the assumption that you took advice from a sleep expert researcher with regard to this. If you have not already requested the input of a sleep (particularly with regard to the PSQI) research expert I recommend that you do this.**

Response: We thank the reviewer for this comment. To increase the response and completion rate, we included a limited number of survey items (i.e., 30 items), so we could include a limited number of sleep health items. With that said, we agree that sleep is complex and selected sleep health measures that frequently examined (namely sleep quality and sleep duration), which are two of the most commonly studied components of sleep health. We respectfully note that we have included an epidemiologist with expertise in sleep in the project and manuscript, and we believe that including select items from the PSQI is appropriate given our study's focus. The approach we used has been used in previous research (e.g., Millar et al., 2019), which we have mentioned in the revised manuscript.

Millar BM, Parsons JT, Redline S, Duncan DT. What's Sleep Got to Do with It?: Sleep Health and Sexual Risk-Taking Among Men Who have Sex with Men. *AIDS Behav.* 2019 Mar;23(3):572-579.

- 5. Present 'n' in parentheses when reporting percentages.**

Response: We have made this change.

Reviewer #2

- 1. Title: I am not agree with the title focusing on middle east, they conducted study in Jordan not in all around the middle east, then please revise it in which title reflects the study population and content accurately.**

Response: We thank the reviewer for this comment and note that the title has now been amended to reflect the exact population, which the study recruitment focused on.

Comments regarding the abstract

- 2. First subsection should be reduced effectively for presenting the background sounder and more relevant, the first sentence is not relevant.**

Response: We have removed the first sentence.

- 3. Surprisingly the number of participants has not been reported. The study design name should be presented in methods section. Study instruments for evaluating of main variables should be introduced.**

Response: We have addressed the reviewer's concerns, including the number of participants. Because this study did not have a study name, we have not included it. In the revised manuscript, we have now included the study instruments for the main variables. Thank you for these suggestions.

- 4. Please follow a same manner for presenting the results in results section, the presented aRR for depression should be presented along with 95%CI such as those you presented for anxiety.**

Response: As suggested by the reviewer, we have presented the aPR and 95%CI's in the tables of the revised manuscript.

- 5. The second and third points in strengths and limitation need concerns.**

Response: We thank the reviewer for this note. The strengths and limitations section has been reviewed and all statements made were fact-checked, which we agree has improved our manuscript.

Comments regarding the introduction

- 6. The second and third paragraphs are not relevant and some sections from these should be transferred to methods. You can present some data based on relevant literature about the effects of lockdown on mental health.**

Response: We thank the reviewer for this comment. We note that the second paragraph of the revised manuscript explicitly discusses the effects of the COVID-19 pandemic in the context of Jordan, and the strict policies imposed locally that motivated the study of health outcomes among the population in lockdown. However, the introduction has been reviewed in accordance with the feedback. Our revised manuscript includes increased data on relevant literature about the effects of lockdown on mental health (page 4).

- 7. Please provide more relevant literature about the association of mental health and sleep health particularly in the context of lockdown.**

Response: We thank the reviewer for this important comment, which we have addressed in the revised manuscript. Throughout the introduction section, and particularly in the fifth paragraph (page 5), we have added relevant literature about the association of mental health and sleep health in the context of lockdown (pages 4 and 5). Although this information is limited in the literature, the following references have been added.

Gualano MR, Lo Moro G, Voglino G, Bert F, Siliquini R. Effects of Covid-19 Lockdown on Mental Health and Sleep Disturbances in Italy. *Int J Environ Res Public Health*. 2020;17(13):4779. Published 2020 Jul 2. doi:10.3390/ijerph17134779

Huang Y., Zhao N. Generalized anxiety disorder, depressive symptoms and sleep quality during COVID-19 outbreak in China: A web-based cross-sectional survey. *Psychiatry Res*. 2020;288:112954. doi: 10.1016/j.psychres.2020.112954

Zhang C., Yang L., Liu S., Ma S., Wang Y., Cai Z., Du H., Li R., Kang L., Su M., et al. Survey of insomnia and related social psychological factors among medical staff involved in the 2019 novel coronavirus disease outbreak. *Front. Psychiatry*. 2020;11:306. doi: 10.3389/fpsy.2020.00306..

8. The sentences started with "Research has shown that sleep" To the end of paragraph are not suitable for covering the current study content and objective.

Response: We thank the reviewer for this comment. The revised introduction section includes additional discussion and citations of relevant studies investigating the association between mental health and sleep health in the context a lockdown, as well as relevant data on the effects of lockdowns on mental health (pages 4 and 5). We believe that these modifications, among others throughout the introduction section, added information for covering the current study content and objective. The paragraphs explicitly highlighting the objectives have also been revised (page 5). We believe that these modifications have improved our manuscript.

9. Generally the introduction section has bot been structured relevantly, it needs major concerns.

Response: We thank the reviewer for this comment. We have substantially edited the Introduction, which has significantly improved our manuscript.

10. The sentences started with "This study is one of the first to examine ..." to the end of paragraph are repetitive, these sentence have been presented in previous sentences!

Response: We have removed the second sentence.

Comments regarding the methods

11. How about the study design name?

Response: We did not name the study a priori.

12. How about the some relevant and important variables regarding mental health and sleep health such education, marital status in demographic variables domain?

Response: Unfortunately, other socio-demographic variables (i.e., education, marital status) were not collected to ensure that the survey was of a manageable length, to reduce the non-response rate. We have now noted this in the limitation section (page 11).

13. Please present more relevant data about validity and reliability of used instruments. Particularly among Jordanian population.

Response: We thank the reviewer for this note. We have outlined data from the literature that exemplifies the validity and the reliability of all instruments used. Unfortunately, given that the Jordanian population is extremely understudied in public health research, no data is available to support the validity or reliability of the instruments within such specific context. This has been noted in the limitation section (page 11), which we agree is important to state in the revised manuscript.

14. First sentence in statistical analysis is repetitive.

Response: We have removed the first sentence, "The study population included individuals aged 18 years and older (n = 1,240)", as suggested by the reviewer.

15. I am not agreed with the used main statistical method i.e. Poisson regression! Binary logistic regression should be used. If you used Poisson regression why "P-values for trend were calculated using logistic regression." This is wrong approach; you should use logistic and in its context p for trend should be provided. Please be clear about the categories of all three outcomes and define their categories clearly in this section. some other statistical tests are needed according to my comments you will see in results section. please present them in this section during revision.

Response: We thank the reviewer for this comment. Regarding the Poisson regression model, researchers have decided to report the prevalence ratios (PRs) rather than odds ratios

(ORs) because ORs significantly overestimate an association when the outcome is common (Barros and Hirakata, 2003; Behrens et al., 2004; McNutt et al., 2003).

Therefore, a modified Poisson regression model, also known as “Poisson regression model with a robust error variance (Zou 2004) is more appropriate given the high prevalence of the sleep health outcomes, which we mentioned in the revised manuscript (page 8). To be consistent with the main statistical method (as a reviewer suggested), we now have calculated the P-values for trend, using the same model by treating the categories of mental health as continuous variables. The results are identical. Categorization of three outcome variables (poor sleep quality, short sleep duration and experiencing sleep problems) were mentioned in the Methods section, “Sleep health” (page 7).

Barros AJD, Hirakata VN: Alternatives for logistic regression in cross-sectional studies: an empirical comparison of models that directly estimate the prevalence ratio. BMC Medical Research Methodology. 2003, 3: 21-33. 10.1186/1471-2288-3-21.

Behrens T, Taeger D, Wellmann J, Keil U: Different methods to calculate effect estimates in cross-sectional studies. A comparison between prevalence odds ratio and prevalence ratio. Methods Inf Med. 2004, 43 (5): 505-509.

McNutt LA, Wu C, Xue X, Hafner JP: Estimating the relative risk in cohort studies and clinical trials of common outcomes. Am J Epidemiol. 2003, 157 (10): 940-943.

Zou G. A modified poisson regression approach to prospective studies with binary data. Am J Epidemiol. 2004;159(7):702-706. doi:10.1093/aje/kwh090

Comments regarding the results

16. Major defect in results is that your data in table 1 have been presented irrelevantly, you should present the data on demographic and predictors (mental health) between categories of three outcome (sleep), however you have presented outcomes across categories of demographic and predictors.

Response: Thanks for your comment. We conducted chi-square test or Mann-Whitney U test to show various significant differences between sociodemographic groups and mental health in relation to sleep. To avoid confusion, we now have presented both good and poor sleep health outcomes in Table 1 to show differences between categories of three sleep outcomes, as suggested.

- 17. Please present the mean age and scores of mental health problems and compare them between categories outcomes (sleep variables) by using relevant statistical tests.**

Response: Because both age and scores of anxiety and depression are not normally distributed, we have added the median (with interquartile range [IQR]) age and scores of anxiety and depression, as suggested. In addition, we used a nonparametric Mann-Whitney U test to determine if there is a significant difference between two groups (Table 1). All measures marked with an * indicate significant differences of $p < 0.05$.

- 18. As stated you should use binary logistic regression. Please present both crude OR and adjusted based on, then you can present p for trend too! Yes!**

Response: We appreciate the reviewer's comments. As mentioned in response to the reviewer's previous comment (#15), a Poisson regression model with a robust error variance as opposed to logistic regression should be used.

- 19. Results section should be revised and rewritten based on above points.**

Response: We thank the reviewer for this comment. The revised manuscript includes a modified results section that includes additional results where applicable, in correspondence to any changes made and outlined in the responses above.

Comments regarding the conclusion

- 20. As stated regarding the some irrelevant literature in introduction, please consider this point in discussion section too.**

Response: We thank the reviewer for this note. In the resubmitted version of the manuscript, the discussion has been reviewed and edits have been made, including major restructuring of the arguments and the amount of literature cited.

- 21. Many variables such education, marital statues, BMI, PHYSICAL ACTIVITY in current study have not been considered as cofounding variables. These variables are associated with both predictors and outcomes you studied them! they are important confounder you missed them**

Response: We agree that residual confounding is an issue and have discussed this more clearly in the revised manuscript. On page 11, we state that because the survey was relatively short we omitted several health outcomes/behaviors, such as BMI. Because we assessed physical activity, we included it in our updated regression models, which we report in the revised manuscript. The results remained almost identical after additional adjustment for physical activity.

VERSION 2 – REVIEW

REVIEWER	Rosalind M Elliott Northern Sydney Local Health District, Australia
REVIEW RETURNED	02-Oct-2020

GENERAL COMMENTS	<p>I found the paper quite hard to read and to see if you had attended to my suggestions/comments. I would advise that in future you lay out your responses to the reviewers' comments in a table and use another colour font instead of TCs.</p> <p>The paper requires proof reading there are still some examples of awkward expression contained within.</p> <p>I have nothing substantial to comment on with reference to reporting of the methods, results or discussion at this time.</p>
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