PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	MENTAL HEALTH DISORDERS AMONG HEALTH CARE WORKERS DURING THE COVID-19 PANDEMIC: A CROSS-SECTIONAL SURVEY FROM THREE MAJOR HOSPITALS IN KENYA.
AUTHORS	Shah, Jasmit; Monroe-Wise, Aliza; Talib, Zohray; Nabiswa, Alphonse; Said, Mohammed; Abeid, Abdulaziz; Ali Mohamed, Mohamed; Mohamed, Sood; Ali, Sayed

VERSION 1 – REVIEW

REVIEWER	Magnavita, Nicola Università Cattolica Sacro Cuore, Public Health
REVIEW RETURNED	02-Mar-2021

GENERAL COMMENTS	This study is interesting because it provides data on the mental health status of a sample of health care workers (HCWs) from Kenya. The study is cross-sectional and was carried out during the Covid-19 pandemic. Consequently, this study cannot describe the evolution of psychiatric disease or its incidence, but only its prevalence. Throughout the article, the authors often make the mistake of speaking of "increased symptoms"; it is a fact that they do not possess, they can only speak of a "high rate of symptoms". This problem is common to most of the studies conducted during the Covid-19 pandemic. The abundance of research has also made it possible to produce several systematic reviews and meta-analyses. All the studies retrieved in these reviews, however, were cross-sectional. Depressive symptoms and anxiety in HCWs were compared to "normal values", administrative staff, or external sample; moreover, some studies had no control group. No longitudinal study has been produced so far. Some studies had negative results. Overall, there is still little evidence of an increase in mental health problems and sleep disturbances in HCWs during the outbreak [Magnavita N, Di Prinzio RR, Chirico F, Sacco A, Quintavalle G. COVID-19 and staff mental health: is there a piece of evidence? An Italian field study. Eur J Public Health, 2020;30 (2 Suppl): ckaa165.565, https://doi.org/10.1093/eurpub/ckaa165.565]. The authors can consider this data, and correct the statement they made on page 8, line 25, "…have shown increased rates of stress, depression, anxiety and burnout among healthcare workers (HCW)". The studies they cite report prevalence, not incidence; moreover, most of the cited studies do not

	make any comparison between "(HCW) taking care of COVID-19 patients compared with those not caring for COVID-19 patients". This part should be corrected. Of course, the lack of longitudinal studies does not mean that there is no association between pandemic and mental health. However, we must admit that we do not know what the mental health condition of the HCWs was before the pandemic. It may be that in many cases they were suffering from insomnia, anxiety and depression even before Covid-19. The conditions of the HCWs in Africa, effectively described in the second part of the Introduction, suggest that this was possible. The authors should also explain why doctors outnumber nurses; exactly the opposite happens in hospitals. The classification of frontline and non-front line workers seems rather artificial and could be a source of bias. Authors need to better explain this classification. In an article based on the comparison between two groups of workers, the homogeneity of the two groups is fundamental. The authors should have taken care to have two groups at least equally numerous. This is a limitation of the study, coupled with the rather low response. Authors should better explain their methods and clarify this point. The sample is quite small, only 433 people, of which only 135 were not directly exposed to Covid-19 patients. The authors should explain how they are sure these people had no contact with Covid-19 cases. If these doctors and nurses were working in the office, no comparison is possible between the two groups. This point is also very important, the authors need to better explain the features. A major limitation of the study is that the authors did not ask workers if they had unprotected exposure to patients with Covid19 and if they contracted Covid19 themselves. An Italian study has shown that sleep disorders, anxiety and depression increase in workers who have unprotected exposure or who develop Covid19 [Magnavita N, Tripepi G, Di Prinzio RR. Symptoms in Health Care Workers during the COVID-19 Epidemic. A Cross-Sectional Survey. Int J Environ Res Public Health. 2020;17(14):5218. doi: 10.3390/ijerph17145218.]. On page 10, lines 10-20, the authors observe that many workers were dissatisfied with the security measures. An Italian study shows that intensivists assigned to Covid19 patients often have low confidence in security measures, and this reduced confidence is associated with stress, anxiety and depression [Magnavita N, Soave PM, Ricciardi W, Antonelli M. Occupational stress and mental health of anaesthetists during the COVID-19 pandemic. Int J Environ Res Public Health 2020, 17, 8245; doi:10.3390/ijerph17218245]

REVIEWER	Oe, Misari Kurume University School of Medicine, Department of Neuropsychiatry
REVIEW RETURNED	15-Mar-2021

GENERAL COMMENTS	This manuscript investigated mental health impact of

	COVID-19 among health care workers in Kenya. The topic of COVID is an area where there is currently a great need for more data, and it is valuable in that it is a report from Africa. However, there seem to be some fundamental problems with the statistical analysis and discussion as a research paper. My main concerns are as follows. 1 Study design: While this study states that the data was obtained anonymously, it reads as if the email addresses of the respondents, who were hospital workers, were captured. Does this method ensure that the data is anonymous? An explanation on this point is needed. 2 Study design: There is no indication of which language was used for the questionnaires in this study. It is necessary to describe whether the reliability and validity of each scale in the used language have been verified, and if not, how this issue has been dealt with. In addition, the authors are encouraged to provide the Cronbach's alpha for each scale for the present study. 3 Statistical Analysis: It is understandable that the scale used in this study was dichotomized using cutoffs and used in the multivariate logistic regression. However, I do not agree with the inclusion of both categorical and continuous variable notations in Table 3. Also, in Table 5, there is no need to use the categorical notation, and the statistical analysis should be done as a continuous variable. 3.1 In relation to this point, I think that it is preferable to delete the sentences in page 12, line 40-49 4 Discussion: Overall, there are many places where the results are repeated in the further discussion section. The discussion section should start by stating what the most important results of this study are, and then describe the authors' thoughts on the results obtained, using previous literature. 4.1 Page 14, line 9-25: The sentence “Most participants…doctors.” is not connected to the sentences before and after it. If the results are correct (in case the statistical analysis is redone), I would like to have some additional insight as to why women and doctors have more severe anxiety and depression than men and nurses. Is there a cultural aspect to this? Do doctors have more knowledge and therefore feel more anxious when they do not get enough PPE? 4.2 Page 14, line 28-44: If you want to state the relationship with age, it is advisable to calculate the correlation coefficient and explain it. The median alone is not sufficient to discuss in comparison with other papers. I think it is also useful to use the age dichotomized by the median as a variable in multivariate logistic regression analysis. It is difficult to say that the percentage of depression and anxiety in this study is higher than the percentage in other studies. This is because other papers may not use the same scale, and even if they do, the cutoff values may differ by language. 4.3 Page 15, line 23-44: A comparison between government hospitals and private hospitals should be

	discussed after explaining the general characteristics of both in Kenya in a way that readers from other countries can understand. Are there populations with higher levels of education working in government hospitals? Or do you find that more COVID-19 with higher severity are admitted in government hospitals? 4.4 Page 15, line 47-page 16, line 36: I had the impression that it is a bit of a jump to describe about a formal mental health response plan here. It might be more desirable to focus on health care workers and describe the future prospects based on the findings of the study. There are also minor points to be addressed. 5 Abstract, Discussion: The authors used the expression "this is the first study…", but there is already a publication on this topic. Htay MNN, Marzo RR, AlRifai A, et al. Immediate impact of COVID-19 on mental health and its associated factors among healthcare workers: A global perspective across 31 countries. J Glob Health. 2020;10(2):020381. doi:10.7189/jogh.10.020381 6 In general, I think that the IES-R is a measure of PTSD symptoms, and it is not desirable to simply refer to it as "distress". 7 Results, page 12, line 21: 433 (91.5%) should be 433 (59.7%). 8 Limitations: this study was conducted as a cross-sectional study and this is one of the limitations (It is difficult to attribute the results to the effects of COVID-19 because the authors cannot look at the status prior to COVID-19). 9 There is no statement about the STROBE checklist.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

1. The study is cross-sectional and was carried out during the Covid-19 pandemic. Consequently, this study cannot describe the evolution of psychiatric disease or its incidence, but only its prevalence. Throughout the article, the authors often make the mistake of speaking of "increased symptoms"; it is a fact that they do not possess, they can only speak of a "high rate of symptoms".]. The authors can consider this data, and correct the statement they made on page 8, line 25, "…have shown increased rates of stress, depression, anxiety and burnout among healthcare workers (HCW)".

Response: We have corrected for it to be mentioned as higher rates and not increased rates. We have mentioned also in the limitations that the study can only identify the prevalence and not the evolution of psychiatric disease or its incidence.

2. The studies they cite report prevalence, not incidence; moreover, most of the cited studies do not make any comparison between "(HCW) taking care of COVID-19 patients compared with those not caring for COVID-19 patients". This part should be corrected.

Response: We have modified this language to reflect that certain references demonstrated overall high rates of mental health symptoms, while others did compare COVID-19 frontline workers to other HCWs.

3. The authors should also explain why doctors outnumber nurses; exactly the opposite happens in hospitals.

Response: The reviewer is absolutely right that the number of nurses is most certainly greater than doctors at most hospitals. However, in our study the number of responses from doctors was higher than the number of responses from nurses. It could be that nurses less seldom check their emails and hence were unable to access the surveys. We have added this as part of a limitation of the study.

4. The classification of frontline and non-front line workers seems rather artificial and could be a source of bias. Authors need to better explain this classification.

Response: We based these definitions on published literature on COVID-19 and mental health, choosing to utilize terminology that has already been defined. We have added a sentence in the Methods section explaining this distinction and hope this further clarifies the distinction.

5. In an article based on the comparison between two groups of workers, the homogeneity of the two groups is fundamental. The authors should have taken care to have two groups at least equally numerous. This is a limitation of the study, coupled with the rather low response. Authors should better explain their methods and clarify this point.

Response: Thank you for this important point. We carried out a cross-sectional survey and it is difficult to accommodate equal number of both groups being compared because this depends on the number of healthcare workers who respond to the surveys. This is certainly a limitation and we have added a sentence to the limitations to address this.

6. The sample is quite small, only 433 people, of which only 135 were not directly exposed to Covid-19 patients. The authors should explain how they are sure these people had no contact with Covid-19 cases. If these doctors and nurses were working in the office, no comparison is possible between the two groups. This point is also very important, the authors need to better explain the features.

Response: These doctors and nurses that responded were either working with confirmed COVID-19 patients (in COVID wards) or were not. However, the healthcare workers who identified as not working with COVID cases were mostly from the outpatient clinics as well as the non-COVID wards. Since COVID-19 is now prevalent in the community, incidental exposure in the clinics is possible but hard to quantify since most patient in the out-patient clinics are not confirmed cases. We add a sentence in the methodology to better explain this point.

7. A major limitation of the study is that the authors did not ask workers if they had unprotected exposure to patients with Covid19 and if they contracted Covid19 themselves.

Response: Unfortunately, we did not ask the participants if they had contracted COVID-19 themselves or if they had unprotected exposure to COVID-19. COVID-19 is prevalent in the community; it is hard to track if any HCWs contracted the virus from working with positive patients or from the community at large. We have added a comment about the lack of this information as a limitation.

8. On page 10, lines 10-20, the authors observe that many workers were dissatisfied with the security measures. An Italian study shows that intensivists assigned to Covid19 patients often have low confidence in security measures, and this reduced confidence is associated with stress, anxiety and depression.

Response: We're sorry, but we are unable to find this comment in the paper. We would appreciate further clarification from the reviewer. Page 10 describes the Methods section from the submission.

Reviewer: 2

1. Study design: While this study states that the data was obtained anonymously, it reads as if the email addresses of the respondents, who were hospital workers, were captured. Does this method ensure that the data is anonymous? An explanation on this point is needed.

Response: Email addresses of all healthcare workers in each hospital were obtained by the principal site investigator in each hospital with help of the Information Technology Department. This was to allow us to send the electronic survey to all the healthcare workers. The questionnaire did not capture the email addresses and the investigators did not know which individual didn't answer the survey. This has been added in the manuscript.

2. Study design: There is no indication of which language was used for the questionnaires in this study. It is necessary to describe whether the reliability and validity of each scale in the used language have been verified, and if not, how this issue has been dealt with. In addition, the authors are encouraged to provide the Cronbach's alpha for each scale for the present study.

Response: The survey was in English. We have corrected and added the language in the Procedures section. Multiple studies looking at mental health outcomes have used the same validated questions and hence have been identified in literature to be reliable and valid. We have mentioned in the procedures section of the questionnaires being validated. Furthermore, we have also reported in the results section the internal validity based on the Cronbach's alpha for each of the questionnaire used, which ranged from 0.896 – 0.971.

3. Statistical Analysis: It is understandable that the scale used in this study was dichotomized using cutoffs and used in the multivariate logistic regression. However, I do not agree with the inclusion of both categorical and continuous variable notations in Table 3. Also, in Table 5, there is no need to use the categorical notation, and the statistical analysis should be done as a continuous variable.

Response: We agree that the inclusion of both categorical and continuous variables in Table 3 was confusing, and we have therefore omitted the continuous variables for the scores since the categorical gives more information to the readers. For Table 5, we would respectfully request that we keep the categorical variables as this allows the readers to compare the categories of mental health symptoms based on the severity. Furthermore, we feel that the analyses for Tables 3 and 5 are similar, and keeping categorical variable reporting in Table 5 allows for standardization across the tables.

4. In relation to this point, I think that it is preferable to delete the sentences in page 12, line 40-49.

Response: Similar to our response to the previous question, we believe that reporting the percentage of participants in each category allows us to emphasize the difference in severity of the symptoms; For example, severe depression was reported among 11.6% of frontline HCWs (n=34) compared with 3.8% (n=5) among workers not directly caring for COVID-19 patients. Using this language shows the effect size more clearly than reporting the difference in means.

5. Discussion: Overall, there are many places where the results are repeated in the further discussion section. The discussion section should start by stating what the most important results of this study are, and then describe the authors' thoughts on the results obtained, using previous literature.

Response: We have made the necessary changes as suggested by the reviewer and avoided repetition of our results in the discussion section.

6. Page 14, line 9-25: The sentence “Most participants…doctors.” is not connected to the sentences before and after it. If the results are correct (in case the statistical analysis is redone), I would like to have some additional insight as to why women and doctors have more severe anxiety and depression than men and nurses. Is there a cultural aspect to this? Do doctors have more knowledge and therefore feel more anxious when they do not get enough PPE?

Response: We have deleted the sentence “Most participants were married, female and doctors.” The reviewer raises pertinent questions that is beyond the scope of this project and we feel this question might need further investigation possibly by means of a focus group which we did not do.

7. Page 14, line 28-44: If you want to state the relationship with age, it is advisable to calculate the correlation coefficient and explain it. The median alone is not sufficient to discuss in comparison with other papers. I think it is also useful to use the age dichotomized by the median as a variable in multivariate logistic regression analysis.

Response: This change has been made in the manuscript.

8. It is difficult to say that the percentage of depression and anxiety in this study is higher than the percentage in other studies. This is because other papers may not use the same scale, and even if they do, the cutoff values may differ by language.

Response: We have modified the wording of this sentence to read: “Furthermore, although methods vary slightly in different studies, our study showed higher overall percentages of depression, anxiety, and insomnia among frontline HCWs than have been found other studies conducted in higher income countries using the same validated scales and diagnostic criteria”

9. A comparison between government hospitals and private hospitals should be discussed after explaining the general characteristics of both in Kenya in a way that readers from other countries can understand. Are there populations with higher levels of education working in government hospitals? Or do you find that more COVID-19 with higher severity are admitted in government hospitals?

Response: Most government hospitals have generally few resources that are often hard to quantify. The government hospitals are of various levels and resources greatly vary depending on their geographic locations in the country. It would prove very difficult and unjust to define such health care facilities with general comments. There is no study, to our knowledge, comparing level of education of healthcare workers at the different hospitals. Neither is a way to know if patients at governmental hospitals were much sicker. Providing this data would at best be speculation. We have added some comments on the resources available in Kenya in the discussions.

10. I had the impression that it is a bit of a jump to describe about a formal mental health response plan here. It might be more desirable to focus on health care workers and describe the future prospects based on the findings of the study.

Response: We have changed the wording in the Discussion section to highlight that a formal mental health response plan has been publicly and formally called for by other researchers in Kenya most notably by Florence Jaguga and Edith Kwobah in their paper: Mental health response to the COVID-19 pandemic in Kenya: a review where they describe 5 fundamental recommendations. Our findings fully support this call for a response plan.

11. Abstract, Discussion: The authors used the expression “this is the first study…”, but there is already a publication on this topic. Htay MNN, Marzo RR, AlRifai A, et al. Immediate impact of COVID-19 on mental health and its associated factors among healthcare workers: A global perspective across 31 countries.

Response: We have modified this to read “this is among the first studies to…”. However, our study looks at a larger population as well as additional mental health outcomes compared to the study quoted above. The study mentioned was conducted early during the pandemic – April – May 2020 and had a low representation from the Africa Region (11 countries in Africa with only 49 responses).

12. In general, I think that the IES-R is a measure of PTSD symptoms, and it is not desirable to simply refer to it as "distress”.

Response: Although the IES-R was initially designed for PTSD, its use includes measurement of general distress, and it has been used in multiple published COVID-19 studies. We have added a sentence to the Methods section further explaining the use of this scale with references using as “distress”.

13. Results, page 12, line 21: 433 (91.5%) should be 433 (59.7%).

Response: The 91.5% is based on the percentage of the respondents that consented to participate in the study. We have added the 59.7% in the Results section.

14. Limitations: This study was conducted as a cross-sectional study and this is one of the limitations (It is difficult to attribute the results to the effects of COVID-19 because the authors cannot look at the status prior to COVID-19).

Response: This is certainly true and we have added this as a limitation.

15. There is no statement about the STROBE checklist.

Response: We have included the STROBE checklist.

VERSION 2 – REVIEW

REVIEWER	Oe, Misari Kurume University School of Medicine, Department of Neuropsychiatry
REVIEW RETURNED	29-Mar-2021

GENERAL COMMENTS	The manuscript has been improved. However, several concerns were remained or newly seen. Among them, I would like to suggest that some of the discussion about statistics be left to the judgment of the editors or specialists, because I am not a statistician. These discussion points are as follows: a) Correlation coefficients for age are not shown in the results, but only some of the results are shown in the discussion section (Discussion section, line 16). Is it acceptable? b) Is it appropriate in this manuscript to show both this categorical data and continuous variable data? (There is some disagreement between authors and me here. Table 3 and Table 5) c) Is it acceptable that one can say “higher” percentages of depression etc., was shown in this study compared with other studies (Discussion section, line 18-25)? Other concerns besides these points are described below. 1. IES-R: The authors claim that the rephrasing to "distress" is possible based on past literature. However, there is no evidence that it can be used as a measure of general distress. The heading should be “distress caused by traumatic events”. 2. Comparison of government hospitals with private institutes: I am sorry, but it was hard to understand the explanation in the revised manuscript. If there are no particular characteristics of the government hospitals in Kenya compared to the private hospitals, then I am not sure that there is much point in the comparison between them. What about the possibility that there was not a problem with the management style of the hospitals, but simply a difference among the three hospitals? The number of hospitals surveyed is small, so it may not be possible to discuss this in depth.

VERSION 2 – AUTHOR RESPONSE

Reviewer 2
I would like to suggest that some of the discussion about statistics be left to the judgment of the editors or specialists, because I am not a statistician.

1. These discussion points are as follows:
a) Correlation coefficients for age are not shown in the results, but only some of the results are shown in the discussion section (Discussion section, line 16). Is it acceptable?
Response: We have included the correlations for age with mental health symptoms in the results section.

b) Is it appropriate in this manuscript to show both this categorical data and continuous variable data? (There is some disagreement between authors and me here. Table 3 and Table 5)
Response: Table 3 shows both categorical and continuous data because both the IESR questionnaire (distress) and SPFI questionnaire (burnout) has subscales where the scores are aggregated on certain questions as per the domains and are to be reported in continuous form. Regarding the scores for depression, anxiety and insomnia, we have removed the continuous scale as per the recommendation from the first review.

c) Is it acceptable that one can say "higher" percentages of depression etc., was shown in this study compared with other studies (Discussion section, line 18-25)?
Response: The comparison was made with the studies that had used the same questionnaires as we did and we reported it based on the overall results reported from these studies. We have modified our language describing these studies to ensure that is well understood.

2. IES-R: The authors claim that the rephrasing to "distress" is possible based on past literature. However, there is no evidence that it can be used as a measure of general distress. The heading should be "distress caused by traumatic events".
Response: We have changed our wording to "distress caused by traumatic events" in parts of the manuscript.

3. Comparison of government hospitals with private institutes: I am sorry, but it was hard to understand the explanation in the revised manuscript. If there are no particular characteristics of the government hospitals in Kenya compared to the private hospitals, then I am not sure that there is much point in the comparison between them. What about the possibility that there was not a problem with the management style of the hospitals, but simply a difference among the three hospitals? The number of hospitals surveyed is small, so it may not be possible to discuss this in depth.
Response: We have revised the discussion section to include more detail about the differences between public and private hospitals in Kenya as a means of providing more sufficient explanation for these findings.

VERSION 3 – REVIEW

REVIEWER	Oe, Misari Kurume University School of Medicine, Department of Neuropsychiatry
REVIEW RETURNED	14-May-2021

GENERAL COMMENTS	The revised manuscript has been much improved. All of my comments have been answered appropriately. I appreciated that authors' great efforts, especially that they added

	explanations to help us understand the current structure and status of hospitals in Kenya.