PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Acute Stress of the Healthcare Workforce during the COVID-19 pandemic evolution. A cross-sectional study in Spain
AUTHORS	Mira, José; Carrillo, Irene; Guilabert, Mercedes; Mula, Aurora; Martin, Jimmy; Pérez-Jover, Mª Virtudes; Vicente, Mª Asunción; Fernández, César

VERSION 1 – REVIEW

REVIEWER	Ty Lees Penn State University, USA
REVIEW RETURNED	19-Jul-2020

GENERAL COMMENTS	The presented manuscript aimed to determine the prevalence of high levels of acute stress in health professionals due to COVID-19 care, and evaluate the response capacity of those individuals in the evolving context of COVID-19. The manuscript is promising, and I am always encouraged to see researchers investigating the impact of stress/mental health in the health professions, and the authors are to be commended for that. However, as written the manuscript does require some work. There are some minor grammatical and syntactical fixes needed (e.g. Page 13, paragraph 2 "Even though of the different…", but it is not particularly onerous when reading the manuscript. My specific comments are as follows: 1. According to the strengths and limitations highlights, no socio-demographic data was collected. Is this true even for basic information .e.g. Age, sex, gender, etc.? I understand wanting to maintain anonymity of responders, but that seems a huge oversight given how much influence those factors can have on stress experience and resilience. 1a. The authors also make a passing mention of this directly in the introduction "individual differences in stress response". 2. The ethics approval statement is contradictory, it states that approval was not applicable, but then states that the study was approved by the Hospital committee? 3. Some form of hypotheses relevant to the two study aims would be a welcome addition to the manuscript. 4. The first aim of the study presents a quandary, namely in how you can separate the base level stress experienced by these individuals from that directly relevant to caring for COVID patients. Some of the questions asked in the EASE scale certainly do this, others not so much. 4a. This quandary also bleeds into the design and methods of the study. How exactly are you isolating the covid related impact? 5. What was the consenting procedure relevant to the study? Was it

	formalised, or inherent in the use of the web portal, or the app? 6. How many primary care centers and hospitals were participants recruited from? And what was the breakdown between them? 7. Was there any confirmatory process that checked that the respondents had actually worked in treating COVID patients? 8. The validation study relevant to the EASE is currently unpublished (as per reference 21); it may be worthwhile considering including at least a brief summary of the validation results in this manuscript. 8a. Particularly, how it tracks onto other scales that map affective factors, and fear/anxiety. 9. How was the grouping of the EASE scale values determined? Was this part of the validation study? 10. The authors write: "For comparison purposes, a total of 336 participants working in the health institutions of these territories were included in the analyses?" 10a. Comparison of what, and to what? Do you mean to say in the dataset, 336 participants were located in the two chosen regions (which looks to be correct given Table 2)? Please clarify. 11. What was the data source used in separating the evolutionary phases of the outbreak? Please provide these details. 12. 32.3% of participants were classified as "other healthcare staff". What did this include? 13. In the context of this analysis, and my second comment, I think basic participant demographics (Age and sex) need to be reported. 14. The authors write: "The total score on the scale was 11.1 points (…" 14a. I assume this was the mean total score, please clarify. 15. Page 12, Paragraphs 3 and 4 – The authors include both percentages and sample counts, I would suggest to format the counts as (n = XXX) for clarity. 16. The results specifically include comparisons of single items from the EASE scale, this information should be added to the statistical analysis section as it is currently absent. 17. Did the comparison between the most and least affected territories include any covariates e.g. equipment access, staff counts, degrees of funding, SES status? 18. I wonder if it's worth further segmenting the analysis by the individual health professions/categories. It could add a particularly poignant perspective to the manuscript as there are marked differences in demands between the fields that lead to variance in the individuals experience, coping strategies, resilience, etc. 19. As written, I'm not sure that the present results point towards the author's suggestion that this research confirms the impact of the pandemic on health professional well-being, and parts of their subsequent discussion. No measure of well-being has been presented, and only two dimensions of mental health were examined. 19a. Even more so considering the average EASE score was within the "affordable level of distress" 20. I think the discussion could do with restructuring (to follow the order of presented results), or some additional subheadings. At the moment it feels somewhat disjointed. 21. Page 14, Paragraphs 1 and 2 need to be clarified. The point intended by the author is unclear. 22. Page 15, Paragraph 3 – This needs to be elaborated upon, particularly in how the present results suggest the three recommendations. 23. The limitations certainly need to be further acknowledged, e.g. the lack of basic demographic data renders contextualising the results very difficult, and also presents a dimensionally limited view

	of what is going on. 24. All tables need to include a definition of what each factor represents. Be it in the table itself, or in the caption.

REVIEWER	Albert Wu Johns Hopkins Bloomberg School of Public Health
REVIEW RETURNED	01-Aug-2020

GENERAL COMMENTS	This is a useful cross-sectional survey of a convenient sample of 685 health care staff in Spain focusing on their reported stress and functioning due to care of COVID-19 patients. Strengths include the use of a standardized stress impact scale, and availability of surrogate data for correlates of stress including number of deaths per day per geographic region and stage of the pandemic. The examination of 4 points in time was a unique aspect and strength of the study. There are some questions. Was the objective of the study really to determine the proportion of health professionals who present with a high level of acute stress? Is the focus on excessive stress rather than stress, and is it acute stress rather than simply stress? Was it to assess the level of stress? The authors should consider this and consider reframing the objective if it is the second choice. Minor language editing of the otherwise excellent English would be helpful. The paper could be improved by paying attention to the following points. 1. In the abstract, line 47, “tolerable” might be a better synonym for “affordable,” and “load” might be better than “overload.” 2. Line 39 the statistical tests used should be stated. 3. Line 54, rather than saying slightly more intense, it would be preferable to give the scores in the different territories and phases. 4. Line 56, the interpretation that “the response could be determined by accumulated stress and fatigue” belongs in the discussion or conclusions rather than in the results section. 5. Page 5, line 6, resilience was not measured directly. For this reason, the conclusion about reduced resilience is not supported by the results. At very least, this assertion should be qualified, e.g., stating “may reduce their resilience.” Rather than “possible rebound” it might be better to write “a second surge” or “future waves” of COVID-19. 6. Line 55. I think the important point is not that the design was not randomized – rather, the importance is that the survey was not administered to a random sample of the population. This could limit generalizability of the findings. 7. Page 7, line 6, the authors may want to update the number of deaths worldwide and in Spain. 8. Line 34, “compassion fatigue” is preferable to “compassionate fatigue.” The point should be made that the extent of trauma experienced by professionals is also influenced by factors that are not directly related to the health care response, such as family income and living situation, and personal health status. 9. Page 8, line 22 a recent meta analysis in the Annals of Internal Medicine might be cited. Chou R, Dana T, Buckley DI, Selph S, Fu R, Totten AM. Epidemiology of and Risk Factors for Coronavirus Infection in Health Care Workers: A Living Rapid Review. Ann Intern Med. 2020;173(2):120-136. doi:10.7326/M20-1632 10. Page 9, line 30, it is not clear what is meant by “…the opposite would be difficult to explain.” 11. Page 9, line 47. It would be useful to know if there are clinical

	cutoffs for the EASE Scale, and for the affective response and fears/anxiety scale. Since the validation study is pending, can the authors share some of the evidence for reliability and validity? And, how were the score ranges determined? (page 10, line 55) 12. Page 10, line 35. It should be noted in the limitations that the survey was accessed via their well-being repository. This may have influenced the level of distress that professionals were experiencing – for example, those going to the website may have been feeling more distress. Were there difference in the scores that were accessed on the website vs on the BE+ app? 13. Page 11, Line 52. The Myers and Zunin model postulates disillusionment phase that follows the heroic/honeymoon phase, but precedes recovery. Did the authors consider this? This could explain why scores were higher in what they categorized as "restoration" 14. Page 12, line 29, what other kinds of healthcare staff were included in the 32.3%? 15. In the Discussion, page 13, line 50, I think it is fair to say that health care workers are experiencing stress during the pandemic, and that as hypothesized, this is proportional to an indicator of the regional burden of disease. However, the conclusion of line 52 might be qualified since the analysis was ecological – the burden experienced by individual professionals was not measured. 16. Line 21, it is unclear to me how losing empathy and fear of becoming ill were identified as being related preventing work. Was a multivariable analysis done? 17. The evolution in stress response could be better explained by the substitution of the disillusionment phase for recovery phase, which in my opinion has not yet occurred. 18. Page 17, line 30 – previous studies of health care workers have used general scale to measure anxiety, depression and burnout that have be previously validated. I agree that a strength of the current study is the use of a purpose built measure.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Please leave your comments for the authors below
The presented manuscript aimed to determine the prevalence of high levels of acute stress in health professionals due to COVID-19 care and evaluate the response capacity of those individuals in the evolving context of COVID-19. The manuscript is promising, and I am always encouraged to see researchers investigating the impact of stress/mental health in the health professions, and the authors are to be commended for that. However, as written the manuscript does require some work.
Thank you for your appreciation. We have introduced changes following your helpful comments.

There are some minor grammatical and syntactical fixes needed (e.g. Page 13, paragraph 2 "Even though of the different…", but it is not particularly onerous when reading the manuscript.
We have modified this expression to "Despite the different affectation between territories, […]"

My specific comments are as follows:
1. According to the strengths and limitations highlights, no socio-demographic data was collected. Is this true even for basic information .e.g. Age, sex, gender, etc.? I understand wanting to maintain anonymity of responders, but that seems a huge oversight given how much influence those factors can have on stress experience and resilience.
1a. The authors also make a passing mention of this directly in the introduction "individual differences

in stress response".

Given the extreme situation to which the professionals of our country were subjected, we prioritized the purpose of helping the healthcare workforce. They were reluctant to receive psychological aids and support and, to avoid the refusal to complete the scale, we decided not to include all typical socio-demographic variables such as sex and age, even though this decision was to the detriment of potential comparisons.

2. The ethics approval statement is contradictory, it states that approval was not applicable, but then states that the study was approved by the Hospital committee?
Thank you for your comment. We are sorry that we have not explained this clearly. The study protocol was approved by the Sant Joan hospital's Research Committee.
We have simplified the wording of this section so as not to generate confusion.

3. Some form of hypotheses relevant to the two study aims would be a welcome addition to the manuscript.
We have defined the hypotheses, one for each study objective, based on data from previous research. We have incorporated the hypotheses at the beginning of the method section.

4. The first aim of the study presents a quandary, namely in how you can separate the base level stress experienced by these individuals from that directly relevant to caring for COVID patients. Some of the questions asked in the EASE scale certainly do this, others not so much.
Thank you for your comment. It has made us realize that the wording of the objective is confusing. Although we understand that the experience of the professionals may have been different depending on their previous levels of stress, the items on the scale and the instructions for completion were written specifically for the COVID context.
We have modified the wording of the objective as follows: The objectives of this study were, first, to determine the volume of health professionals who, because of the impact of the COVID-19 pandemic on the healthcare environment in which they work, experienced an excessive level of acute stress that prevented them from performing their role.

4a. This quandary also bleeds into the design and methods of the study. How exactly are you isolating the covid related impact?
We have explained in the participants' section the reasons why all the professionals were affected by the COVID-19 impact in their usual routine. At the time the study was conducted, the entire public health system was involved in the care of COVID-19 patients. Care for patients suffering other pathologies was suspended except for emergencies and those that could not be delayed, in other situations care was provided by telephone. Prudently, we consider that the fact that scores in the territories most affected by the impact of the pandemic are higher than elsewhere, reinforces the measure of acute stress performed.

5. What was the consenting procedure relevant to the study? Was it formalised, or inherent in the use of the web portal, or the app?
The consenting procedure was inherent in the use of the website and app. We have specified this aspect in the manuscript. Thanks for pointing this out.

6. How many primary care centers and hospitals were participants recruited from? And what was the breakdown between them?
We have included a description in the results section
"During the outbreak, many primary care professionals went on to work in field hospitals or hospital

emergency rooms or nursing homes. These changes in resources make the reliability of information about their work less than at other times. However, we have included the information available.
A majority of them reported working in a hospital setting (58, 82%), in primary care (55, 8%) and both care levels (68, 10%). 40.4% worked in areas where the pandemic had had a greater impact."

7. Was there any confirmatory process that checked that the respondents had actually worked in treating COVID patients?
During the study period, health care for non-COVID-19 pathologies was suspended, except for emergencies and non-delayable consultations. The attention to NON-COVID-19 patients were done by telephone calls in most of cases. To some extent, the entire healthcare workforce was oriented towards the care of COVID-19 patients, whether they were mild, moderate or severe. Depending on the extent of the pandemic, physicians or nurses from other services than internal medicine, infectious diseases, or intensive care settings collaborated in caring for this patient profile. In some cases, after a brief training.
The reviewer's comment is accurate and can certainly be considered a limitation of the study that did not ask directly whether they had cared for COVID-19 patients (number and severity). However, for the reasons mentioned above, the experience of caring for COVID-19 patients has been widespread in all centres.

8. The validation study relevant to the EASE is currently unpublished (as per reference 21); it may be worthwhile considering including at least a brief summary of the validation results in this manuscript.
8a. Particularly, how it tracks onto other scales that map affective factors, and fear/anxiety.
In the section of variables and instruments, we have described the process of development and validation of the scale, we have highlighted the phases of development of the statements until reaching the consensus of 10 items, as a result of the experiences of health professionals and the review of the existing literature.
This is the complementary information that we have incorporated:
"This scale was previously validated, first a pragmatic literature review of items assessing acute stress in healthcare professionals was conducted for possible inclusion, in addition, the most relevant sources of acute stress, pointed by the professional's experiences were represented into 17 reactive items; this number was finally reduced to 10 items, once participants considered their representativeness and comprehension. The instrument was validated (COSMIN protocol was applied) involving 228 Spanish physicians and nurses. It is composed of 10 items to which responses are given using a 4-level Likert type scale (0 = It is not happening to me, 1 = It happens to me in concrete situations, 2 = It often happens to me and 3 = I am like this all the time). The total score on the scale can range from 0 to 30 points, with greater scores being interpreted as higher levels of stress. Reliability was calculated using OMEGA (0.87) and Cronbach's Alpha (0.85). The items are grouped by Exploratory Factor Analysis into two factors that evaluate: affective response and fears and anxiety, explaining 55% of the variance were isolated. Factor 1, referring to the affective response, is composed of 6 items, so that the direct score on this factor ranges from 0 to 18 points. The factor 2 that evaluates fears and anxiety is composed of 4 items and its minimum and maximum possible scores are 0 and 12 respectively. The interpretability of the score ranges was established: 0-9 points (good emotional adjustment), 10-14 points (emotional distress), 15-24 points (medium-high emotional overload), >25 points (extreme acute stress)."

9. How was the grouping of the EASE scale values determined? Was this part of the validation study?
A brief overview of the EASE scale validation process, content validity, construct validity and scale interpretability following the COSMIN checklist has been included. The total score of the scale was 30 points, so it was taken as a reference and according to the trend of responses, distribution of responses in 4 fairly symmetrical cuts, where the first range was somewhat wider since many professionals did not experience high levels of stress or request help. The last range for those who exceeded 25 points was left only for those health professionals who truly suffered extreme stress.

10. The authors write: “For comparison purposes, a total of 336 participants working in the health institutions of these territories were included in the analyses?”
10a. Comparison of what, and to what? Do you mean to say in the dataset, 336 participants were located in the two chosen regions (which looks to be correct given Table 2)? Please clarify.
Yes, that is what we intended to express. From the total sample of participants, the 336 who worked in health institutions in the regions indicated in the text were selected to make comparisons between more and less affected territories. We have modified the wording of the text to clarify this point.

11. What was the data source used in separating the evolutionary phases of the outbreak? Please provide these details.
The data source used has been included in the Procedure section.

12. 32.3% of participants were classified as “other healthcare staff”. What did this include?
We have specified the profiles of health personnel that were categorized as "other" (advanced technicians in nursing auxiliary care, pathological anatomy, radiodiagnosis, and clinical diagnostic laboratory).

13. In the context of this analysis, and my second comment, I think basic participant demographics (Age and sex) need to be reported.
We have responded to this comment in 1 and 1a. As the reviewer point out this is a limitation of this study.

14. The authors write: “The total score on the scale was 11.1 points (…”
14a. I assume this was the mean total score, please clarify.
Yes, we wanted to refer to the mean total score. We have clarified it in the text.

15. Page 12, Paragraphs 3 and 4 – The authors include both percentages and sample counts, I would suggest to format the counts as (n = XXX) for clarity.
Thanks. It is now clearer. We hope that now it is better understood.

16. The results specifically include comparisons of single items from the EASE scale, this information should be added to the statistical analysis section as it is currently absent.
We have incorporated this information in the statistical analysis section.

17. Did the comparison between the most and least affected territories include any covariates e.g. equipment access, staff counts, degrees of funding, SES status?
No covariates were included in the comparative analysis between more and less affected territories. The classification of the territories in these two categories was made exclusively according to the number of deaths/day in each region. We have commented on this aspect in the limitations of the study. It should be considered that during the pandemic, there was an increase in personnel and resources throughout the health system in response to an emergency that could not be quantified.

18. I wonder if it’s worth further segmenting the analysis by the individual health professions/categories. It could add a particularly poignant perspective to the manuscript as there are marked differences in demands between the fields that lead to variance in the individuals experience, coping strategies, resilience, etc.
No differences were observed for the professional category, only in one of the statements, we have referred to this difference in the results section for the scores in the EASE scale.

19. As written, I’m not sure that the present results point towards the author’s suggestion that this research confirms the impact of the pandemic on health professional well-being, and parts of their

subsequent discussion. No measure of well-being has been presented, and only two dimensions of mental health were examined.
19a. Even more so considering the average EASE score was within the “affordable level of distress”
We understand that you are right and that we have made modifications. We have incorporated throughout the manuscript the term mental health.

20. I think the discussion could do with restructuring (to follow the order of presented results), or some additional subheadings. At the moment it feels somewhat disjointed.
We have tried to incorporate subheadings according to the presentation of results.

21. Page 14, Paragraphs 1 and 2 need to be clarified. The point intended by the author is unclear.
In red colour, clarifications have been incorporated.

22. Page 15, Paragraph 3 – This needs to be elaborated upon, particularly in how the present results suggest the three recommendations.
We have included this information in the text.

23. The limitations certainly need to be further acknowledged, e.g. the lack of basic demographic data renders contextualising the results very difficult, and also presents a dimensionally limited view of what is going on.
The context for this study has been described with all the limitations and biases in this section.

24. All tables need to include a definition of what each factor represents. Be it in the table itself, or in the caption.
We have specified in the tables the names of the scale factors (factor 1: affective response and factor 2: fears and anxiety).

Reviewer: 2

Please leave your comments for the authors below
This is a useful cross-sectional survey of a convenient sample of 685 health care staff in Spain focusing on their reported stress and functioning due to care of COVID-19 patients. Strengths include the use of a standardized stress impact scale, and availability of surrogate data for correlates of stress including number of deaths per day per geographic region and stage of the pandemic. The examination of 4 points in time was a unique aspect and strength of the study. There are some questions. Was the objective of the study really to determine the proportion of health professionals who present with a high level of acute stress? Is the focus on excessive stress rather than stress, and is it acute stress rather than simply stress? Was it to assess the level of stress? The authors should consider this and consider reframing the objective if it is the second choice. Minor language editing of the otherwise excellent English would be helpful.
The paper could be improved by paying attention to the following points.
We very much appreciate all comments and hope that we have improved the wording and content

1. In the abstract, line 47, “tolerable” might be a better synonym for “affordable,” and “load” might be better than “overload.”
Thank you for your suggestion. We have replaced the terms indicated by the suggested synonyms.

2. Line 39 the statistical tests used should be stated.
We have specified in the abstract the statistical analyses performed, and the statistical tests used.

3. Line 54, rather than saying slightly more intense, it would be preferable to give the scores in the different territories and phases.

Thank you for your comment. We have incorporated the scores in the different territories and phases.

4. Line 56, the interpretation that “the response could be determined by accumulated stress and fatigue” belongs in the discussion or conclusions rather than in the results section.
We have removed this statement from the results section.

5. Page 5, line 6, resilience was not measured directly. For this reason, the conclusion about reduced resilience is not supported by the results. At very least, this assertion should be qualified, e.g., stating “may reduce their resilience.” Rather than “possible rebound” it might be better to write “a second surge” or “future waves” of COVID-19.
We agree with your assessment. We have nuanced the sentence to improve its accuracy and congruence with the results. We have also replaced the expression "possible rebound" with "future waves of COVID-19" throughout the manuscript.

6. Line 55. I think the important point is not that the design was not randomized – rather, the importance is that the survey was not administered to a random sample of the population. This could limit generalizability of the findings.
We have reworded this point to emphasize the idea that the scale was not administered to a random sample of the population and the limitations that this implies.

7. Page 7, line 6, the authors may want to update the number of deaths worldwide and in Spain.
Following your suggestion, we have updated the number of deaths by COVID-19 worldwide and in Spain according to the figures published at the date of review of the manuscript.

8. Line 34, “compassion fatigue” is preferable to “compassionate fatigue.” The point should be made that the extent of trauma experienced by professionals is also influenced by factors that are not directly related to the health care response, such as family income and living situation, and personal health status.
We have changed the term and incorporated commentary on non-health response factors that can affect the experience of trauma, including two references on this topic.

9. Page 8, line 22 a recent meta analysis in the Annals of Internal Medicine might be cited. Chou R, Dana T, Buckley DI, Selph S, Fu R, Totten AM. Epidemiology of and Risk Factors for Coronavirus Infection in Health Care Workers: A Living Rapid Review. Ann Intern Med. 2020;173(2):120-136. doi:10.7326/M20-1632
Thank you for your contribution. We have included a brief reference to this recent meta-analysis in the Introduction section of our manuscript.

10. Page 9, line 30, it is not clear what is meant by “…the opposite would be difficult to explain.”
We have tried to clarify this statement, specifying that the complete absence of impact on the staff of health institutions would be difficult to explain given the magnitude of the situation.

11. Page 9, line 47. It would be useful to know if there are clinical cutoffs for the EASE Scale, and for the affective response and fears/anxiety scale. Since the validation study is pending, can the authors share some of the evidence for reliability and validity? And, how were the score ranges determined? (page 10, line 55)
In the section on variables and instrument, the validation process of the instrument is explained in detail.

12. Page 10, line 35. It should be noted in the limitations that the survey was accessed via their well-being repository. This may have influenced the level of distress that professionals were experiencing – for example, those going to the website may have been feeling more distress. Were there difference

in the scores that were accessed on the website vs on the BE+ app?
Thank you for your note. We have included in the limitations section the possible over-representation of the most distressed professionals due to the access way to the survey.
About the differences in scores between the website and the BE+ against COVID app, it should be noted that differences were found in terms of scores, but we detected that these differences were because the professionals who used the BE+ against COVID app were from territories with the greatest expansion of the pandemic.
We have included a commentary regarding the differences in the manuscript.

13. Page 11, Line 52. The Myers and Zunin model postulates disillusionment phase that follows the heroic/honeymoon phase, but precedes recovery. Did the authors consider this? This could explain why scores were higher in what they categorized as “restoration”
We agree, it seems that with the second waves we have not reached the phase of restoration but the data we have are of the so-called phase of disillusionment and therefore are higher. We have replaced the term throughout the manuscript with "disillusionment”.

14. Page 12, line 29, what other kinds of healthcare staff were included in the 32.3%?
We have specified the profiles of health personnel that were categorized as "other" (advanced technicians in nursing auxiliary care, radiodiagnosis, and clinical diagnostic laboratory).

15. In the Discussion, page 13, line 50, I think it is fair to say that health care workers are experiencing stress during the pandemic, and that as hypothesized, this is proportional to an indicator of the regional burden of disease. However, the conclusion of line 52 might be qualified since the analysis was ecological – the burden experienced by individual professionals was not measured.
We have tried to explain how our results suggest the type of emotional responses in those territories with a higher incidence rate.

16. Line 21, it is unclear to me how losing empathy and fear of becoming ill were identified as being related preventing work. Was a multivariable analysis done?
We have 2 statements in the questionnaire that address these issues

17. The evolution in stress response could be better explained by the substitution of the disillusionment phase for recovery phase, which in my opinion has not yet occurred.
We agree. We have made the changes according to this comment

18. Page 17, line 30 – previous studies of health care workers have used general scale to measure anxiety, depression and burnout that have be previously validated. I agree that the strength of the current study is the use of a purpose-built measure.
Thank you for this commentary on our work

VERSION 2 – REVIEW

REVIEWER	Ty Lees Edna Bennett Pierce Prevention Research Center, Pennsylvania State University
REVIEW RETURNED	04-Sep-2020

GENERAL COMMENTS	The authors are to be commended on the reasonably extensive revision of their manuscript; as it stands I think a significant amount of clarity and value has been added to the work.

	My remaining comment/s revolves around the missingness of standard demographic data, and how the absence of such data presents a conundrum. On one hand, I appreciate the author's response regarding why they felt that the collection of demographic data may have been detrimental to participation given the extenuating circumstances of the COVID-19 pandemic. But on the other hand, the absence of such data makes placing the present results within the literature incredibly difficult, and raises the question of "how useful is this work if a picture of the participants and/or comparisons cannot be readily drawn/made?" I think as it stands right now, the manuscript requires a more extensive discussion of the limited individual level demographic data. Currently, the authors have stated their rationale as to why such data wasn't collected, but I think a more detailed justification and discussion of the implications of the missing data is required.

REVIEWER	Albert Wu Johns Hopkins University Bloomberg School of Public Health USA
REVIEW RETURNED	25-Sep-2020

GENERAL COMMENTS	The authors have been very responsive to the suggestions of the reviewers.

VERSION 2 – AUTHOR RESPONSE

Reviewer: 1
Please leave your comments for the authors below
The authors are to be commended on the reasonably extensive revision of their manuscript; as it stands I think a significant amount of clarity and value has been added to the work.

My remaining comment/s revolves around the missingness of standard demographic data, and how the absence of such data presents a conundrum. On one hand, I appreciate the author's response regarding why they felt that the collection of demographic data may have been detrimental to participation given the extenuating circumstances of the COVID-19 pandemic. But on the other hand, the absence of such data makes placing the present results within the literature incredibly difficult, and raises the question of "how useful is this work if a picture of the participants and/or comparisons cannot be readily drawn/made?"

I think as it stands right now, the manuscript requires a more extensive discussion of the limited individual level demographic data. Currently, the authors have stated their rationale as to why such data wasn't collected, but I think a more detailed justification and discussion of the implications of the missing data is required.

We fully agree with and appreciate the comments of reviewer 1. That is why we have expanded the description of the limitations referred to sociodemographic data collection in the Discussion section with emphasis on their implications. Our decision not to collect all desirable sociodemographic data was adopted to preserve the professional's sense of privacy and not to discourage their participation in the study. The collection of responses was taken at a time when 900 deaths per day were being reported by COVID-19 and most of the healthcare workforce was reluctant to speak up about their

experience and emotions. Probably at present, it would have been possible to carry out the study more extensively and completely, without the collection of sociodemographic data negatively affecting participation in the study.

Reviewer: 2

Please leave your comments for the authors below
The authors have been very responsive to the suggestions of the reviewers.

Thank you for contributing your comments to the improvement of the manuscript and appreciating the changes made.

VERSION 3 – REVIEW

REVIEWER	Ty Lees Edna Bennett Pierce Prevention Research Center, Pennsylvania State University, USA
REVIEW RETURNED	14-Oct-2020

GENERAL COMMENTS	I appreciate the authors responsiveness and their efforts in editing their manuscript. I have no further comments.