PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (http://bmjopen.bmj.com/site/about/resources/checklist.pdf) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

ARTICLE DETAILS

<table>
<thead>
<tr>
<th>TITLE (PROVISIONAL)</th>
<th>Levels of burnout among healthcare workers during the COVID-19 pandemic and their associated factors. A cross-sectional study in a tertiary hospital of a highly burdened area of north-east Italy</th>
</tr>
</thead>
<tbody>
<tr>
<td>AUTHORS</td>
<td>Lasalvia, Antonio; Amaddeo, Francesco; Porru, Stefano; Carta, Angela; Tardivo, Stefano; Bovo, Chiara; Ruggeri, Mirella; Bonetto, Chiara</td>
</tr>
</tbody>
</table>

VERSION 1 – REVIEW

<table>
<thead>
<tr>
<th>REVIEWER</th>
<th>Margae Knox</th>
</tr>
</thead>
<tbody>
<tr>
<td>University of California, Berkeley School of Public Health, USA</td>
<td></td>
</tr>
<tr>
<td>REVIEW RETURNED</td>
<td>06-Oct-2020</td>
</tr>
</tbody>
</table>

GENERAL COMMENTS

This article uses a large survey of healthcare workers to describe burnout levels and factors associated with higher burnout early in the Covid-19 pandemic. The survey took place in one University hospital in Northeast Italy—a pandemic hot spot at the time of the survey. The study finds that levels of burnout are considerably higher than those reported from other sources in Italy before the pandemic. Key factors associated with higher burnout were nurse or resident occupation, pre-existing psychological problems, experiencing a pandemic-related traumatic event, and interpersonal avoidance.

Study strengths include its large sample size (2195 out of 5,940 personnel) and exploration of both structural and experience factors. Although results may not be directly externally valid since the survey took place in just one hospital, the trends offer important insight as hospitals around the world strive to maintain physician and staff well-being amidst new pressures from Covid-19 care.

The paper’s research question could be further strengthened by stratifying results (including Table 1 & 2) by whether workplace site put the respondent in more direct contact with patients with Covid-19 (e.g., intensive care unit, sub intensive care wards, and frontline units dealing with covid-19 vs. other sites). In addition, throughout the manuscript, results need to include “in comparison to what” whenever relative measures such as odds ratios are described.

Additional specific suggestions to improve the paper follow.

- **Abstract:**
  - Statements of “increased risk” should note in comparison to what.

- **Methods:**
| REVIEWER               | Georgios Tsivgoulis  
|                       | Second Department of Neurology, National and Kapodistrian University of Athens, Greece |
| REVIEW RETURNED       | 21-Oct-2020 |
| GENERAL COMMENTS      | This is a single-center, cross sectional study with the aim to investigate burnout levels and associated factors among healthcare professionals working in a tertiary hospital in Veneto region. This is a well-designed and highly informative study. Authors may consider commenting on a recent study which is also assessing psychological outcomes amongst healthcare workers during COVID-19 outbreak: (DOI: 10.1016/j.bbi.2020.04.049) |

| REVIEWER               | Yuguero, Oriol  
|                       | Institut de Recerca Biomedica de Lleida |
| REVIEW RETURNED       | 08-Nov-2020 |
| GENERAL COMMENTS      | This is a really interesting article. However I have some concerns about this article. I think that the problem is that the sample is not representaive. you only have 36% of answer rate. Usually is recommend to obtaine 60% or at least 50% of answer rate. |
Probably you can adjust it, and center in one of the professional groups. For example just physicians or nurses or administration professionals. If not, I think that results are not representative of the population. You have less than 15% of the physicians and administration professionals. Moreover I am not sure if the stress or situation that can produce burnout is similar in all those professionals groups.

In addition, I think that the Burnout scale is not used correctly. You have used the MBI general survey, but for health workers there is an specific one, the MBI-HSS. Of course, all people can answer the general one, but if you want to publish results of burnout in health workers you should use the specific test. If not, you should state it in the limitations section. Finally, I haven’t seen the Ethics Committee statement in the manuscript.

VERSION 1 – AUTHOR RESPONSE

Reviewer 1

This article uses a large survey of healthcare workers to describe burnout levels and factors associated with higher burnout early in the Covid-19 pandemic. The survey took place in one University hospital in Northeast Italy—a pandemic hot spot at the time of the survey. The study finds that levels of burnout are considerably higher than those reported from other sources in Italy before the pandemic. Key factors associated with higher burnout were nurse or resident occupation, pre-existing psychological problems, experiencing a pandemic-related traumatic event, and interpersonal avoidance.

Study strengths include its large sample size (2195 out of 5,940 personnel) and exploration of both structural and experience factors. Although results may not be directly externally valid since the survey took place in just one hospital, the trends offer important insight as hospitals around the world strive to maintain physician and staff well-being amidst new pressures from Covid-19 care.

Authors’ response: we thank Reviewer 1 for her helpful and constructive comments which greatly help improve the paper. Below the responses to each comment raised. We have revised the manuscript accordingly.

The paper’s research question could be further strengthened by stratifying results (including Table 1 & 2) by whether workplace site put the respondent in more direct contact with patients with Covid-19 (e.g., intensive care unit, sub intensive care wards, and frontline units dealing with covid-19 vs. other sites).

Authors’ response: many thanks for this suggestion. We however had decided not to dichotomised the sample according to the workplace site as we aimed to explore not only the effect of contact with COVID-19 patients (i.e. “exposure” to COVID-19 patients), but also contact with COVID-19 patients showing levels of increasing severity. Indeed, our findings provide evidence that for eg emotional exhaustion, staff working in frontline services dealing with Covid-19 pts (emergency departments and radiology) show the same risk of developing an adverse outcome of those working in non COVID units, and significantly lower than staff working in sub-intensive or ICUs , thus suggesting that is not the exposure to COVID-19 patients per se a risk factor, but rather the exposure to severely ill (or critical) COVID-19 patients (Revised version, Table 4 Multivariate logistic regression models). If we had dichotomised our sample (ie, intensive care unit+sub intensive care wards+frontline units dealing with covid-19 vs. other sites) we would have missed this relevant information.

We better clarify this issue in the Methods, Results and Discussion sections of the revised version of the manuscript:
"...For the purpose of analysis, the various hospital units were stratified according to the degree of clinical engagement with COVID-19 patients, from most engaged to least engaged: Intensive Care Units (that during the lock-down phase were entirely dedicated to critically ill COVID-19 patients), sub-intensive COVID wards (i.e., infectious disease, pulmonary medicine and internal medicine wards specifically dedicated to COVID-19), frontline services dealing with COVID patients (i.e., radiology and emergency department), non-COVID wards, laboratory diagnostic services (i.e., laboratory medicine, transfusion medicine, immunology, pathology, microbiology), and administration..." (Revised version, Methods, page 6, lines 18-25).

"...On the other hand, working in frontline services dealing with COVID-19 patients, in non-COVID wards and in the administration was associated with a reduced risk of emotional exhaustion as compared to working in ICUs..." (Revised version, Results, page 10, lines 8-10).

"...It should be noted that it is not simply the direct contact with COVID-19 patients per se a risk factor for developing burnout, but rather the engagement with severely (or critically) ill COVID-19 patients requiring sub-intensive or intensive care, since, e.g. for emotional exhaustion, staff working in services dealing on daily basis with great numbers of COVID-19 patients (such as radiology and emergency department) have significantly lower risk of developing burnout than staff working in sub-intensive or ICUs, and however the same risk of those working in non-COVID units..." (Revised version, Discussion, page 11, lines 16-22).

In addition, throughout the manuscript, results need to include "in comparison to what" whenever relative measures such as odds ratios are described.

Authors’ response: following the Reviewer’s comment, throughout the Results section of the revised version of the paper we specified "in comparison to what" whenever ORs are described.

Additional specific suggestions to improve the paper follow.

Abstract
Statements of “increased risk” should note in comparison to what.

Authors’ response: following the Reviewer’s comment, we specified as follows:

“...Being a resident increased the risk of burnout (by nearly 2.5 times) in all the three MBI subscales and being a nurse increased the risk of burnout in the EX dimension in comparison to physicians...”

Methods
I’m curious why the authors used cut-offs from an earlier paper rather than referring to the guidelines from the MBI handbook itself.

Authors’ response: as reported in the webpage of the MBI handbook publisher, cut-off scores set up to identify which people are “high” in burnout were published up through the third edition of the MBI Manual (Maslach C, Jackson S, Leiter M. MBI: Maslach Burnout Inventory Manual. Third ed: Consulting Psychologists Press; 1996). In the 2016 publication of the MBI Manual 4th edition, the cut-off scores were removed due to having no diagnostic validity. The previously published cut-off scores were in fact considered by the MBI authors themselves as problematic and arbitrary. For this reason, the cut-offs were not published in the MBI Manual fourth edition and were removed from all associated MBI materials (see https://www.mindgarden.com/documents/MBI-Cutoff-Caveat.pdf).

On the other hand, fifteen years ago the authors of the MBI-GS (Schaufeli WB, Van Dierendonck D. A cautionary note about the cross-national and clinical validity of cut-off points for the Maslach Burnout Inventory. Psychol Rep. 1995; 76:1083-90) recommended extreme caution when using cut-off points for the classification of burnout levels, as it was considered inappropriate the use cut-off scores obtained in one country to classify subjects in another county.

When in 2009 we decided to conduct a study to assess burnout in a large sample of staff working in mental health services in the Veneto region (Italy) (Lasalvia et al, 2009), Italian validated cut-off scores for MBI-GS were not available in the literature. We therefore developed and tested specific cut-off points based on the only (at that time) published European validated cut-off scores provided by the Dutch MBI-GS manual (Schaufeli & Van Dierendonck, 2000). In order to make sure that this procedure was
acceptable, we compared the mean scores of our sample (as well as their respective SDs) with those reported by Schaufeli and colleagues in the Dutch sample, and we found that the Dutch cut-off scores for the three MBI subscales corresponded, respectively, to the 66th, 70th and 25th percentiles of our distribution. We were thus quite confident that the Dutch cut-offs applied to our sample were able to detect those subjects showing very severe distressing condition on the three MBI-GS subscales. To the best of our knowledge, no further study has so far published more recent cut-off points for Italian healthcare workers. Therefore for the purposes of the present study we decided that the best methodological option was to use the cut-off scores we had validated in 2009 in a sample of healthcare staff working in the same geographical area (Veneto).

Based on the Reviewer’s comment, in the revised version of the manuscript we provided the rationale of our choice:

“...Since the MBI-GS authors (Schaufeli & Dierendonck 1995) recommend not to use cut-off scores obtained in one country to classify subjects in another country, we used the only Italian cut-off scores available in the literature for the healthcare sector (>2.20 for EX, >2.00 for CY, and <3.66 for EF); these cut-off scores were tested on a large sample of mental health professionals working in the Veneto region (Lasalvia et al., 2009).” (Revised version, Methods, page 6, lines 10-14).

Please further explain “ongoing treatment of psychological problems”

Authors’ response: this variable aimed to explore whether participants had pre-existing psychological problems (ie, occurred before the COVID-19 outbreak) that were so severe to require some forms of specialized help. We made this clearer in the Methods’ section of the revised version:

“... Personal background information and job-related factors were collected, including sex, age, living condition, occupation, having pre-existing psychological problems (ie, developed before the COVID-19 outbreak) requiring specialised help, length of working experience...” (Revised version, Methods, page 6, lines 16-17).

Results
Rephrase “completers differed from non-completers” to improve clarity: Physicians and residents were more likely to complete the MBI-GS compared to ..., while nurses and other healthcare staff were less likely to complete the MBI-GS compared to ... [May need to recode role variable into multiple dummy variables].

Authors’ response:

“... Completers differed from non-completers (n=234) only in terms of occupational profile and exposure to COVID-related traumatic events. Specifically, physicians (14.6% completers vs. 8.5% non-completers) and residents (17.1% completers vs. 11.1% non-completers) were more likely to complete the MBI-GS compared to nurses (35.0% completers vs. 41.0% non-completers), other health care staff (23.8% completers vs. 28.6% non-completers) and administrative staff (9.5% completers vs. 10.7% non-completers) (Chi-square test p=0.006). Moreover, staff not exposed to COVID-related traumatic events were more likely to complete the MBI-GS (38.7% completers vs. 16.8% non-completers) than exposed staff (61.3% completers vs. 83.2% non-completers) (Fisher’s exact test p<0.001).” (Revised version, Results, page 8, lines 5-14).

Page 9 line 43: “working in usual non-COVID wards reduced...” implies a causal connection. “Was associated with” is more accurate given that data is cross sectional. Noting “in comparison to _____” is also important

Authors’ response: we rephrased the sentence as follows:

“... working in frontline services dealing with COVID-19 patients, in non-COVID wards and in the administration was associated with a reduced risk of emotional exhaustion as compared to working in ICUs.” (Revised version, Results, page 9, lines 8-10).

Page 9 line 57: “Being a resident double the risk...” also needs “in comparison to ____”
Authors’ response: we rephrased the sentence as follows:

“Being a resident doubled the risk of cynicism in comparison to physicians.” (Revised version, Discussion, page 10, line 20).

Table 4a conveys similar information as Table 3; would favor keeping only Table 3 and adding the variables from 4b to Table 3.

Authors’ response: based on the Reviewer’s comment, in the revised version of the manuscript we kept Table 3 and gave findings of univariate logistic regression models in the online supplementary part 3.

Discussion
The recommendations for reducing burnout would be stronger if interwoven with the discussion of study findings. For example, recommendations specific to residents should be moved to page 11, paragraph 1.

Authors’ response: Following the Reviewer’s comment, in the revised version of the manuscript the paragraph “Recommendation to reduce burnout” has been deleted and each recommendation has been interwoven with the discussion of study findings. Please, see the Discussion section of the Revised version.

Some recommendations need more details. For example, what does “promoting autonomy in the workplace” look like?

Authors’ response: based on the Reviewer’s comment, the sentence has been rephrased as follows:

“...hospital administrations should improve nurses’ sense of control over their own scheduling and tasks by promoting their involvement in decision-making, particularly in demanding situations.” (Revised version, Discussion, page 11, lines 22-25).

Thank you for the opportunity to review this paper. Burnout in the healthcare workforce is indeed an ongoing concern and this paper adds valuable understanding about how healthcare workers experienced burnout in the early phases of the current global pandemic.

Reviewer 2
This is a single-center, cross sectional study with the aim to investigate burnout levels and associated factors among healthcare professionals working in a tertiary hospital in Veneto region. This is a well-designed and highly informative study.

Authors’ response: we thank Reviewer 2 for his positive feedback on our work.

Authors may consider commenting on a recent study which is also assessing psychological outcomes amongst healthcare workers during COVID-19 outbreak: (DOI: 10.1016/j.bbi.2020.04.049)

Authors’ response: Thank you for suggesting this interesting paper. As this paper specifically addressed physical symptoms, depression, anxiety, stress and post-traumatic symptoms rather than burnout, we referred to it in the Introduction as reference # 3.

Reviewer 3

This is a really interesting article.

Authors’ response: we are grateful to Reviewer 3 for his comments, which helped improve the manuscript. We have revised it accordingly.

However I have some concerns about this article. I think that the problem is that the sample is not representative. you only have 36% of answer rate. Usually is recommend to obtain 60% or at least 50% of answer rate.

Authors’ response: We think that the representativeness of the sample and the participation rate are not necessarily overlapping concepts and should be considered separately. A given survey may have low participation rate and nonetheless may be representative if the sample composition reflects the reference population in terms of key-characteristics. Of course the higher participation rate the more likely the representativeness may be.

The participation rate of our survey (36.9%), as we clearly acknowledged among the study limitations, is somewhat low in absolute terms. However, as we discussed in the paper, web-based surveys have generally lower response rates than face-to-face or telephone interviews or mail surveys: a meta-analysis (Cook et al., 2000) reported a mean response rate of 39.6%, very similar to our study. In addition, literature reports that surveys involving physicians have even lower overall response rates (35%) (Cunningham et al., 2005). Therefore our participation rate substantially overlaps with existing literature.

Regarding the representativeness of the sample, this was discussed in details in the Online supplement part 1 and briefly reported at the beginning of the Results section (original version of the manuscript, page 7). The representativeness of participants was assessed by comparing two key characteristics important for the aims of this study and for which official statistics from the Verona University Hospital were available (i.e., occupation and exposure to COVID-19 patients). The analyses reported in the Online supplement part 1, clearly show that the study sample overlaps with the Verona University Hospital staff both in terms of occupational profile and percentage of healthcare workers employed in units directly engaged with COVID-19 patients, thus indicating that the respondent selection did not produce a biased sample with respect to important characteristics for the aims of the study.

However, based on the Reviewer’s comment, in the revised version of the manuscript we clarified that:

“...The representativeness of participants was assessed by comparing the study sample with the overall eligible population on two key characteristics for the aims of this study and for which official statistics from the Verona University Hospital were available, i.e. occupation and exposure to COVID-19 patients. For details see on-line supplementary part 1. Overall, the study sample overlapped with the Verona University Hospital staff both in terms of occupational profile composition and percentage of healthcare workers employed in units directly engaged with COVID-19 patients, thus indicating that selection of participants did not produce a biased sample. The sample addressed here may be therefore considered representative of the overall hospital population...” (Revised version, Results, page 7, lines 29-34).

Probably you can adjust it, and center in one of the professional groups. For example just physicians or nurses or administration professionals. If not, I think that results are not representative of the population. You have less than 15% of the physicians and administration professionals.

Authors’ response: for the representativeness issue, please, see above. Here we specify that - as shown in the Table 1 of the online supplement part 1 - the percentage of physicians participating in our survey (13.9%) was the same of all physicians working in the Verona University Hospital (14%). Moreover, the percentage of administrative staff participating in our survey (9.7%) substantially overlaps with that of all administrative staff working in the Verona University Hospital (8.3%).
Therefore, the sample composition in terms of different occupational profiles reflects the composition of the overall hospital staff. The study sample is thus not biased and may be considered representative with this regards.

Moreover I am not sure if the stress or situation that can produce burnout is similar in all those professionals groups.

Authors’ response: we do agree with the Reviewer that working conditions (and job related stress) widely differed across hospital units and professional group. We therefore estimated adjusted ORs for potential risk factors, including place of work and occupation.

In addition, I think that the Burnout scale is not used correctly. You have used the MBI general survey, but for health workers there is an specific one, the MBI-HSS. Of course, all people can answer the general one, but if you want to publish results of burnout in health workers you should use the specific test. If not, you should state it in the limitations section.

Authors’ response: we agree with the Reviewer that the most commonly used instrument for the measuring burnout among people working in human service professions, like healthcare workers, is the 22-item MBI–Human Services Survey (MBI-HSS). We however disagree that MBI-GS was “not used correctly” in our study.

The 16-item MBI-General Survey (MBI-GS) is a modified version of the MBI that was designed to assess burnout in any occupational setting (i.e. in both professions that involve working closely with people as well as in professions without direct contact with service recipients) (Leiter & Schaufeli, 1996). The development of a scale to assess burnout also outside the human service professions was fostered by evidence that the process of burnout does occur and takes the same form in all occupational sectors (Leiter & Harvie, 1998; Leiter, Harvie & Frizzell, 1998; Leiter & Schaufeli, 1996). The MBI-GS is based on the conceptual assumption that burnout is related to the general performance of work, rather than on relationships at work (eg, with patients) – i.e. burnout can occur anytime and anywhere there is a major imbalance, or mismatch, between demands in the work environment and the individual’s available resources (Maslach, Schaufeli & Leiter, 2001). The MBI-GS contains three scales that parallel those of the original MBI and it has been found to be reliable and valid across multiple cultural settings and occupations, including healthcare professionals (Leiter & Schaufeli, 1996; Bakker et al., 2002). The authors who developed the MIB-GS found that the group of healthcare/social services workers did not differ from other occupational groups in the level of burnout, thus confirming the assumption that burnout does not result from the specific nature of working with people (Maslach & Leiter, 1996). A number of studies conducted by independent research groups, confirmed that MBI-GS represents a reliable measure to assess burnout across different occupational groups, including healthcare professionals (Langballe et al., 2006; Setti & Argentero, 2011; Chirkowska-Smolak & Kleka, 2011). The fact that MBI-GS is actually used also on health care professionals is confirmed by recent research. A systematic review on the use of the MBI as a tool for “diagnosing” burnout on healthcare professionals (ie, doctors, nurses and medical residents) found that 50 studies had used either the MBI–HSS (46 studies) or MBI-GS (4 studies) (Doulougeri et al, 2016). In a systematic review and meta-analysis of prevalence and determinants of burnout in mental health professionals (O’Connor et al., 2018), the authors fund that the MBI-HSS was the most commonly used measure (50 studies); however, the MBI-GS was also used (4 studies). A systematic reviews conducted on studies addressing burnout among physicians (Rotenstein et al., 2018) reported that most studies (57.8% [108/182]) used the MBI-HSS; however, 4.8% of studies (9/182) also used the MBI-GS. Finally, in a recent systematic review of burnout among healthcare professionals in sub-Saharan Africa (Dubale et al., 2109), among 20 studies employing the MBI 3 used the MBI-GS.

Incidentally, the MBI-GS was also used by Reviewer 1 in a recent paper aiming to identify the sensitivity, specificity, and concurrent validity of a self-defined burnout measure compared to established MBI measure in a sample of clinicians and staff working in primary care clinics (Knox M, Willard-Grace R, Huang B, Grumbach K. Maslach Burnout Inventory and a Self-Defined, Single-Item Burnout Measure Produce Different Clinician and Staff Burnout Estimates. J Gen Intern Med 2018;33:1344-1351).

Below, relevant references cited above:


Doulougeri K, Georganta K, Montgomery A. “Diagnosing” burnout among healthcare professionals: Can we find consensus? *Cogent Medicine* 2016; 3:1


Therefore, in order to address the Reviewer comment, we specified in the Methods section (“Outcomes and covariates” paragraph) the rationale for choosing the MBI-GS:

“The MBI-GS is a modified version of the MBI that was specifically designed to assess burnout in any occupational setting. We decided to use the MBI-GS as this scale is based on the assumption that burnout takes the same form in all occupational sectors and it is related to the general performance of work, rather than on relationships at work (eg, with patients) - i.e. burnout can occur anytime and anywhere there is a major imbalance, or mismatch, between demands in the work environment and the individual’s available resources (Maslach, Schaufeli & Leiter, 2001). The MBI-GS was found to be reliable and valid across multiple cultural settings and a wide range of occupations, including healthcare professionals (Leiter & Schaufeli, 1996; Bakker et al., 2002; Rotenstein et al, 2018; O'Connor et al., 2018). It consists of 16 items constituting three subscales that parallel those of the original MBI…” (Revised version, Methods, page 5, lines 30-34 and page 6, lines 1-4).

Finally, I haven’t seen the Ethics Committe statement in the manuscript.

Authors’ response: in the submitted version of the manuscript, the Ethics statement was reported in the Methods section, “Study design” paragraph [“The survey was approved by the Ethics Committee of the Provinces of Verona and Rovigo (approval No. 22002; April 17, 2020)"], on page 5, lines 17-18. However, based on the Reviewers’ comment and the Editorial requirements, we moved the Ethics statement in a specific paragraph of footnotes in the revised version of the manuscript:

“Ethics approval: This study complies with ethical standards. The study was approved by the Ethics Committee of the Provinces of Verona and Rovigo (approval No. 22002; April 17, 2020). All participants provided informed written consent” (Revised version, page 15).
VERSION 2 – REVIEW

REVIEWER | Margae Knox  
| University of California, Berkeley, USA

REVIEW RETURNED | 22-Dec-2020

GENERAL COMMENTS
Thank you for addressing my concerns. It was interesting to learn about more recent cut-off recommendations and applying the MBI internationally. Recommendations in the discussion also read nicely.

I have only a few minor follow-up comments:
1. From the reader perspective, it may be more intuitive to provide row percentages for completed responses (x% complete and x% non-complete physicians adds to 100%; x% complete and x% non-complete residents adds to 100%; etc.). Also, I didn’t see the response rate table for an online appendix. Perhaps I missed it?
2. Please include the coefficient and 95% confidence interval in your results section so that readers don’t have to look back and forth between the text and the table to understand the magnitude of the association.
3. You may also want to temper the conclusions about higher burnout among intensive covid care vs. sub-intensive or front line covid care as a ”trend toward higher burnout..." since the confidence intervals include the null value.

Thanks.

REVIEWER | Oriol Yuguero  
| IRBLLEIDA, Spain

REVIEW RETURNED | 29-Dec-2020

GENERAL COMMENTS
I have to congratulate the authors of this article. They have addressed all the reviewers comments in a really good form. All my comments have been reviewed and included in the article.

VERSION 2 – AUTHOR RESPONSE

Reviewer 1
I have only a few minor follow-up comments:

1. From the reader perspective, it may be more intuitive to provide row percentages for completed responses (x% complete and x% non-complete physicians adds to 100%; x% complete and x% non-complete residents adds to 100%; etc.).

Authors’ response: Following the Reviewer’s request, in the new revised version of the manuscript we gave the row percentages for completed responses as follows:

“... Specifically, physicians (93.5% completers vs. 6.5%, non-completers) and residents (92.8% completers vs. 7.2%, non-completers) were more likely to complete the MBI-GS compared to nurses (87.7% completers vs. 12.3%, non-completers), other health care staff (84.7% completers vs. 12.6% non-completers) and administrative staff (88.2% completers vs. 11.8% non-completers) (Chi-square test, p=0.003). Moreover, staff not exposed to COVID-related traumatic events were more likely to complete the MBI-GS (96.0% completers vs. 4.0% non-completers) compared to exposed staff (88.3% completers vs. 11.7% non-completers) (Fisher’s exact test, p<0.001)...” (Revised version, page 8, lines 7-13).
Also, I didn't see the response rate table for an online appendix. Perhaps I missed it?

The response rate for MBI-GS completion for each profession was not given in the supplementary material as it was fully provided within the text of the manuscript (see above). In the Online Supplementary part 1 we gave response rates for participating to the overall survey.

2. Please include the coefficient and 95% confidence interval in your results section so that readers don't have to look back and forth between the text and the table to understand the magnitude of the association.

Authors’ response: Based on the Reviewer’s comment, in the new revised version of the manuscript we provided where necessary in the results section the coefficient and 95% confidence interval. Please see page 10 of the revised version.

3. You may also want to temper the conclusions about higher burnout among intensive covid care vs. sub-intensive or front line covid care as a "trend toward higher burnout..." since the confidence intervals include the null value.

Authors’ response: Following this comment, in the revised version of the manuscript we modified the sentence as follows:

“...staff working in services dealing on daily basis with great numbers of COVID-19 patients (such as radiology and emergency department) tend to display a lower risk of burnout than staff working in sub-intensive or ICUs.” (Revised version, page 11, lines 24-26).

VERSION 3 – REVIEW

<table>
<thead>
<tr>
<th>REVIEWER</th>
<th>Margae Knox</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>University of California</td>
</tr>
<tr>
<td>REVIEW RETURNED</td>
<td>05-Jan-2021</td>
</tr>
<tr>
<td>GENERAL COMMENTS</td>
<td>Congratulations to the authors for a well-done, timely manuscript.</td>
</tr>
</tbody>
</table>