Author’s response to reviews

Title: Cardiac patients' perceptions of neighboring patients' risk: Influence on psychological stress in the ED and subsequent posttraumatic stress

Authors:
Beatrice Konrad (bmk2145@cumc.columbia.edu)
David Hiti (dth2110@cumc.columbia.edu)
Bernard Chang (bpc2103@cumc.columbia.edu)
Jessica Retuerto (jr3448@cumc.columbia.edu)
Jacob Julian (jej2140@cumc.columbia.edu)
Donald Edmondson (dee2109@cumc.columbia.edu)

Version: 1 Date: 19 Jul 2017

Author’s response to reviews:

Dear Editor,

Thank you for your consideration of our manuscript EMMD-D-17-00036, entitled “Proximity to high acuity patients for patients evaluated for acute coronary syndrome in the Emergency Department is associated with increased patient stress and subsequent posttraumatic stress.” We have considered the insightful comments of the reviewers and submit these responses for your consideration.

Editor’s Comment:

An editor's comment that I would like to submit to you prior to revising the manuscript is the rather high number of self-citations in the bibliography (approximately 38%). Although BMC does not have a strict percentage over which this would pose a problem the practice is nevertheless subject to guidelines stating that "authors should not preferentially cite their own ... publications". Obviously in a field where one group is prolific this guideline may be difficult to follow. Nevertheless, in order to reduce their number would it be feasible to consider that some self-citations might do "double-duty", for instance, referencing several sections of the manuscript rather than multiple individual self-citations?

Response: We understand your concern, and have been able to delete 2 self-citations. The first was literally doing “double duty” (ref #18; Chang et al., EMJ was the same as ref #8), and the second (Edmondson and von Kanel; JAMA Psychiatry) is easily replaced with Vilchinsky et al,
an excellent review that covers much of the same ground and that one of the reviewers pointed out should be included.

Reviewer 1:

The present manuscript can be improved by correcting some minor errors or omissions. For example, on line 116 of the manuscript (in the section titled PTSD symptom score), the PCL is reported to use a 5-point Likert scale, ranging from "0 = not at all to 5 = extremely." This is incorrect, the PCL is scored 0 = not at all to 4 = extremely (with scores ranging from 0 to 80). With respect to the assessment of PTSD symptoms, the authors shifted from the DSM-IV-based version of the PCL to the new PCL-5 when DSM-5 was published because the symptom list for the diagnosis of PTSD was modified somewhat with the revised diagnostic manual. The authors explain the "we used only the PCL-5 items that correspond to the PCL-S (DSM-IV) items." It would be helpful to readers to explain which items from the PCL-5 were used (to be consistent with the information that was obtained when the DSM-IV version of this questionnaire was used in the study).

Response: We have updated the error in describing the reporting of the PCL (line 120). We now detail which DSM 5 items were included.

With respect to the description of the "Threat Perception" measure, the authors indicated that 6 questions comprised this measure. It appears that 4 of these questions are described in this section of the manuscript. Because the measure is brief, it would be helpful to the reader to present all 6 questions/items for this measure in the Method section. Related to this "Threat Perception" measure, participants rated each question on a 4-point Likert scale. However, the description of the anchors for the Likert scale range from 0 (not at all) to 4 (extremely). However, a 0 to 4 rating scale is really a 5-point Likert scale. One of the values for the anchors is mislabeled.

Response: Thank you for catching this error, and for pointing out the value of the extra two item descriptions. We now include them.

The authors should add some information for 2 variables listed in Table 1. For Hispanic ethnicity and Confirmed ACS, it is unclear whether the numbers for each variable represent the number of participants or the percentage of participants. It would be helpful to clarify what the numbers represent (which was done for the sex variable in this table).

Response: Thank you for catching this oversight. We now make clear that these are %.

In the presentation of the results in text related to the findings presented in Table 1 (manuscript lines 246 - 249), the authors stated that "participants who perceived a nearby patient as likely to die were younger, with lower GRACE cardiac risk and Charlson comorbidity scores and higher depression, and were treated during periods that the ED was more crowded." This statement is inconsistent with the results presented in Table 1, however, because there were no significant between-group differences on either the GRACE risk score or the Charlson comorbidity index.
The text should be revised to list only the variables that were found to be statistically significant (as presented in Table 1).

Response: We have removed the GRACE cardiac risk and Charlson comorbidity scores from that sentence (line 253).

The Discussion of the study's findings is clear and concise. However, the authors may want to consider adding a couple brief points to expand on the ideas offered in the discussion. For example, in manuscript lines 301 - 307, it is noted that subjective fear and threat during the ED experience may have an important impact on subsequent psychological outcomes. This idea was embodied in the DSM-IV definition/description of a traumatic event. This notion that a traumatic event includes both the actual potentially traumatic event along with the experience of intense fear or horror was deleted from DSM-5. The present findings suggest that the individual's personal reaction of intense fear (in this case of possible death) may be important with acute coronary syndrome.

Response: Thank you for this excellent comment. We have added several sentences in our discussion section on this point, including the implications of our findings for PTSD in acute coronary syndrome particularly. (lines 304-309)

In this same paragraph (manuscript lines 301 - 307), the authors note that "hospital leadership and ED clinicians can create environments that heighten or reduce stress during the ED evaluation." It is clear that this research team has much experience in this area, and it would be beneficial to readers to provide some brief suggestions on how to reduce the stress that patients experience during the ED evaluation.

Response: We have added several suggestions of future work to potentially reduce subjective stress/threat perception in CVD patients in the ED (lines 312-318)

Reviewer 2:

Interestingly, the final finding, if I understood correctly, was that personal threat perception is the explanatory factor and not perceived risk of other's death. Thus, I wonder if the title shouldn't be changed in order to better capture the actual results (the title is also very long and quite vague).

Response: Thank you for this comment. We think we may see the results very slightly differently. Specifically, if I am an ED patient being evaluated for an ACS, and I see another patient nearby who appears to be at high risk for death, that perception increases my own sense of vulnerability and heightens my own awareness of my mortality risk in that moment, thereby increasing my risk for PTSD symptoms after I am discharged. Even so, we agree that this explanatory variable is a key part of the story, and therefore we have now changed the title to reflect that.
Also, the discussion should follow these results. Since personal threat was measured at the same time as perceived other's risk, the authors cannot infer causality. Indeed, it seems that a personality characteristic which makes the patient both vigilant in regard to the goings-on in the ED and at the same time more susceptible to PTSD (such as insecure attachment, trait anxiety) is a better explanation here, as the results in fact bear out.

Response: We appreciate this comment. Although we had considered the temporal issue previously, your points caused us to wrestle with it more vigorously. We now include an additional paragraph in the Limitations, and have slightly rewritten and restructured the Discussion.

The introduction would do well to elaborate more on the theoretical background for the study's assumptions as well as its importance. I suggest bringing in Edmondson's somatic threat model in the introduction, or even as soon as in the abstract.

Response: We sincerely appreciate this opportunity to expand on the theoretical basis for hypotheses we tested. We now discuss the enduring somatic threat model in the Introduction, and its proposition concerning the role of mortality fear in the development of PTSD after cardiovascular events.

The authors should provide hypotheses.

Response: We now explicitly state our hypothesis at the end of the introduction.

The authors should provide the reference for the PCL-S specific for ACS.

Response: We now note that the psychometric properties of the PCL-S (the PCL, but with directions and item wording that is keyed to a single event for a single study) were reported by Blanchard et al in BRAT. We considered citing our prior work on the factor structure of the scale in ACS patients, but the editor has suggested that we include no more self-citations. We are happy to follow editorial guidance either way.

Line 301. The authors should provide a reference for the sentence: "like most prior studies of PTSD in cardiac patients...). They may use Vilchinsky, Ginzburg, Fait & Foa (2017).

Response: This is an excellent suggestion, and one of the best works to date on the topic. We have highlighted it there.

A spare t appears in line 71.

Response: We have removed this typo.

Thank you again for your consideration of our manuscript. We look forward to hearing your feedback.