
This is a well written manuscript about a subject that has not been enough research about; how to differentiate between grief disorder (PGD) and posttraumatic stress disorder (PTSD) after combined trauma and loss.

The gathered information in the project are comprehensive, thus there are many alternatives of how to present them. However, the authors have managed to define specific and important research questions and stayed strict to information related to the research question. They have used appropriate methods that are well described, and the data seems sound. They seem to have chosen, and clearly presented, proper statistical methods. The manuscript is well written, easily understood and present the data in a proper way. Most of the discussions and conclusions are well balanced and supported by the data (however, see comments below). The limitations are clearly stated and discussed in a balanced way. The references are relevant, and they acknowledge previous work. Both title and abstract convey the investigated subject and the abstract their findings (however, see comments below). The study is important and I recommend it to be published. However, I recommend some revisions and have some questions.

Major revisions:

1. The abstract states that the objective is to determine whether there are distinct classes of PTSD and PGD symptoms. However, the manuscript investigates both symptoms (for example Table 4) and precursors to the symptoms (for example Table 5). I suggest that the abstract and the last part of the introduction clearer state both perspectives.

2. The abstract state that “membership in the PTSD/PGD class was predicted by greater trauma exposure and traumatic loss...”. I am a bit unsure what is meant by trauma exposure, as trauma exposure is divided into three parts in the results. Table 5 may actually indicate the opposite, namely that “Exposure to conflict” was not related more to neither the PTSD/PGD nor PTSD class than the resilient class. I also miss a discussion in the discussion chapter of why this level of exposure is not related to level of distress. “Detention and abuse” was related to all three classes of membership of symptoms, thus did not distinguish between classes. The third type of exposure, “Traumatic Loss“ was related to combined
group (PTSD/PGD), but not to the PGD group. I miss a discussion of this lack of finding in the discussion chapter. If there were any part of the exposure one would imagine was highly related to grief, it would be traumatic loss. Is this lack of finding due to the dichotomizing of the exposure variables (see comment below)?

3. In the second chapter of the introduction, the earlier grouping of PTSD into pervasive, moderate and none are theorized to maybe “generalize to conflict and persecution-exposed groups...”. I suggest that it is made clearer that the widest generalization here is from only PTSD to bereaved groups and thus to both PTSD and grief.

4. It is a bit unclear how the Hopkins depression subscale was scored. Did you use a continuous sum score, cut-off score or caseness by DSM-IV derived algorithm? I have problems interpreting the OR figures as compared to the frequencies of depression in the last chapter in the results. Is the odds ratios really as high as 340 etc?

5. Why was the Post migration living difficulties checklist dichotomized? I can understand that it is easier to present the descriptive data when dichotomized, but I am unsure if this is a good choice for the multiple regression model (Table 5). You remove variability and thus power. I am also uncertain if the living difficulties variables in Table 5 are normally distributed after you have dichotomized the sub variables. For example the variables of Residency determination difficulties and Threat to family are constructed of three sub variables and thus only have three possible response alternatives after dichotomizing the sub variables. Would it not be better statistic to keep the full variability of the sub variables and make sum variables to enter the multiple regression?

6. The method chapter also indicates that the exposure and PTSD symptoms were dichotomized in a similar manner. The Harvard trauma questionnaire includes several response alternatives, and some of the presented subscales are made out of few sub variables. Thus, similar problems as mentioned above for the Post migration living difficulties checklist apply for exposure and PTSD symptoms.

7. I suggest that the response alternatives and range of possible results for each subscale is presented in the method chapter.

8. In the fourth paragraph of the discussion, the importance of culture for grief reactions is discussed. In the seventh chapter, the relation between “Loss of culture and support” and PTSD are discussed. I miss a link between these two discussions, and a discussion of why “Loss of culture and support” were not found to be related to grief (PGD group).

Minor issues not for publication:

1. The beginning of the second chapter of the introduction seems to have a smaller font size than the rest of the manuscript.
2. In the second to last paragraph before the conclusions, a sentence miss “are” or “were”: “…so were unable to examine the extent to which these XXX related to class membership”.

3. The last paragraph before the conclusion, a sentence miss “than”: “…interventions tailored to the patient’s specific needs may be more effective XXX that universal treatments…”.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:
I declare that I have no competing interests.