Reviewer's report

Title: Late-life coronary heart disease mortality of Finnish war veterans in the TAMRISK study, a 28-year follow-up

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Reviewer: Maria Inês Azambuja

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1) The title suggests that the paper just wanted to establish if an association existed between being wounded in the war (1943-45) and dying from CHD after the age 55 (black-box approach). The conclusion, on the other hand, suggests that the true question was if an association could be postulated between traumatic events at early adulthood and CHD development late in life. To me the questions are not equivalent.

Being wounded in the war may result in life-long physical disability, which would add additional potential CHD `risk factors´ (inactivity, obesity, depression, medications…) to the selected exposure: “early adulthood traumatic event”.

On the other hand, traumatic events are not only physical. And if the hypothesis is one of association between stressful early-adulthood events and late CHD mortality, the exposed should possibly be all the military that were at the front (527) X men that were not (72+68).

Major Compulsory Revision – to clarify the study question

2) The study used a black-box approach. There was no attempt to model a causal hypothesis. This introduced some inconsistencies 1) regarding the question to be answered (see item 1); 2) regarding the modeling of the CHD mortality risk - depression and BMI would possibly be intermediary variables between trauma/stress and a CHD death (especially if chronic disability ensued). In this case, they should not be controlled for in the risk model (Major Compulsory Revision). 3) also, it seems to me that there is difference between previous reported CHD events (p=0.12) and diabetes (p=0.19) between the study groups – again, expectedly intermediate variables between early adulthood exposure (trauma/stress) and the outcome (CHD death).

3) The study population is small (compared to the studies referred by the authors), the choice of study groups is unconvincing if the underlying association to be investigated is one between stress and late CHD, and the variables are too loosely defined. Which criteria were used to classify exposed and “non-exposed” to trauma according to “smoking” for example, an important risk factor to CHD? (Major Compulsory Revision)

4) Participation rate was 667/843. Does the Kaplan-Meyer curves - with significant losses in the exposed group in the first 1-2 years of the study followed by a recovery towards years 5-10 - suggest bias towards severely ill individuals
during the recruiting of cases in 1980? Doesn’t that trend deserve a comment? (Major Compulsory Revision)

5) Why the distribution of the main causes of deaths in each group was not presented? (role of competing causes of deaths) Discretionary Revision

6) Why the CHD deaths were not described according to year of occurrence? I believe that these descriptive data would add interest to the paper. Discretionary Revision, strongly recommended

PS – Review the phrase “this study is an important contribution to the literature because most studies to date have looked at the effects of exposure to combat-related PTSD rather than exposure to combat itself” This is not the case here. Exposure was not to combat but to wound. (Major Compulsory Revision)

Level of interest: An article of insufficient interest to warrant publication in a scientific/medical journal

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'