Author’s response to reviews

Title: The prevalence of common mental disorders and PTSD in the UK military: using data from a clinical interview-based study

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Author’s response to reviews: see over
8th August 2009

Dear Dr Alam,

**Re: The prevalence of common mental disorders and PTSD in the UK Military: using data from a clinical interview-based study**

Thank you for you reviewers’ useful comments. I respond to each in turn below.

Reviewer 1 (Morton Hesse)

1. I have now included further details about the PHQ and I have provided a citation about its use over the telephone.
2. The tables were sent in a separate Excel file which the reviewer may have difficulty opening. I have now provided the tables in Word format.
3. Martial should indeed read marital and has now been amended.
4. I have removed UK.
5. No we intended to use uni-variable and multi-variable.
6. We agree that the PHQ alcohol questions are somewhat limited and also still serving military personnel might be especially unlikely to disclose alcohol abuse. This is now reflected in our limitations. It is true that this measure was not the same as that which was used in the Gulf veterans study that we cited. However the difference between the two prevalence rates was so large that is unlikely to be due to differences in the measurement alone. We also know from other studies that we and others have done, that the prevalence of alcohol misuse is increasing in the armed forces.

Reviewer 2 (Per-Olof Michel)

1. We agree that our study too is based on the use of questionnaires. These are likely to perform more accurately when administered by a trained researcher, as rapport and gentle probing for symptoms is likely to facilitate disclosure. We have changed the introduction slightly to reflect this important comment however.
2. The cut-off score of 50 on the PCL has been widely used in both UK and US military studies and is the cut-off used by our research group. I have made a note to explain this in the methods.
3. Figure 1 is now referred to in the method to explain to readers how the sample was derived.
4. I have now moved the 61% response rate into the description of the original sample.
5. I have changed the order of the method to make it clearer how the study was conducted.
6. I have included some further details about the PHQ. The PHQ is very widely used and has been used in military settings in the UK and US previously.
7. As far as I know, the PC-PTSD is not based on Breslau’s measure. The PHQ is based on DSM-IV criteria.
8. The tables are now represented as Word documents. We disagree that Table 2 has too much information and believe that the information presented is necessary to readers for interpretation. We have presented the tables as main tables and supplementary tables. The main tables are those that we would request are included in the main body of the article. The supplementary tables may be available if the journal wishes to present them in a separate web-link as suggested in their instructions for authors. If the journal prefers not to do this, we will simply describe these data (as we have already done) and include ‘data available for authors’.
9. We have now included details of how the pre-enlistment variables are derived.
10. I have amended the discussion to reflect the above points re the PHQ and also the 61% response rate.
11. I have described KCMHR in full text.
12. I have included that was the 12 item GHQ.
13. Those who scored above the threshold were also randomly selected. I have now stated this.
14. We feel that the description of PTSD symptoms is appropriate. PTSSR is not a diagnosis in DSM-IV and we are at least attempting to reach diagnoses with these measures (whilst acknowledging their limitations). Description as PTSD symptoms reaches a compromise between these two.
15. Neurotic disorders are defined in the footnote to Table 1.
16. I have now made a comment about the difference in the rates of neurotic disorder.
17. I have defined Era personnel.
18. I have elaborated on the difference between Gulf and Iraq rates of MUS.
19. The participants were not paid to take part; they were reimbursed for their time. Whilst this may seem like semantics, there is a difference. Paying people for studies introduces bias, as the reviewer suggestions. However, offering a small financial reimbursement to cover their time, is standard practice in the UK for research studies such as this, and is very unlikely to introduce bias. Our non-responder analysis did not reveal significant differences between those that took part and those who did not.
20. We disagree with the reviewer’s suggestion about describing the relation between two entities. We think both ways is fine, but we prefer to keep it as it is.
21. We have included a comment about pre-enlistment screening for panic disorder.

We look forward to hearing about the progress of the revised manuscript.

Yours sincerely,

Dr Amy Iversen