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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9th September 2009

Dear Dr Alam,

MS: 1986982071273374: The prevalence of common mental disorders and PTSD in the UK military: using data from a clinical interview-based study

Thank you for your letter dated 3rd September 2009. We are delighted that you anticipate acceptance of our revised manuscript.

We have amended the manuscript with the addition of a title for the authors’ contributions.

We would be happy for you to issue a press release of this paper. I am away in the USA from 9th-28th September, but should your team need to correspond with us during this time regarding the press release, please contact either of my co-authors Dr Neil Greenberg (sososanta@aol.com) or Dr Nicola Fear (Nicola.t.fear@kcl.ac.uk).

We take note of your Editorial board member’s comments, but have decided that we do not wish to amend the manuscript on this occasion.

We address each of the reviewer’s comments in turn:

1) (a)The response rate was 76%. The sample was a subsample (phase 1 of the
KCMHR military health study) from an earlier investigation with response rates of 61%, i.e., overall there was a dropout over 50%. (No information is given, how many actually consented to this follow-up, but this will have lowered the response rate even further). The dropout is too high to provide reasonable prevalence estimates, given that a bias is not unlikely. For instance, those with high avoidance symptoms or those with guilt feelings may have refused to participate, as may have those who are very have successfully integrated in society and are too busy.

Over 90% of the original cohort consented to be followed up, and the response rate for both the phase 1 study and this clinical study are well within the expected parameters of large epidemiological surveys such as this. We have written previously about non-response drawing on experience from a number of our studies (http://www.bmj.com/cgi/content/full/bmj;332/7534/165). We have also shown that non-response in our main study was not influenced by health status (see Tate R, Jones M, Fear N, Hull L, Rona R, Wessely S, Hotopf M, How many mailouts? Could attempts to increase the response rate in the Iraq war cohort study be counter productive? BMC Medical Research Methodology 2007, 7:51), and in our preliminary analyses of the forthcoming phase 2 follow-up data we can confirm that having symptoms of PTSD at Stage 1 does not influence response at Stage 2.

(b) A four item questionnaire, asked via a telephone survey may reveal certain aspects of PTSD-related symptoms, but will definitively not allow to estimate PTSD prevalence. A diagnosis of depression requires a full clinical interview and a behavioral observation by an experienced expert. I don't think that the study provides valid information in terms of diagnosis.

It is not possible to perform 'behavioural observation' in large epidemiological studies with more than 800 participants, and we are not aware of any similar studies employing this methodology, nor is behavioural observation part of any structured interview of which we have knowledge. When we chose the measures that we used in this study to look for symptoms of mental disorders, we did choose carefully. We chose the Patient Health Questionnaire because it is probably the most widely used and validated short screen in the general population, it covers the specific common disorders of interest (depression, anxiety, alcohol), it is quick and acceptable to participants, can be used on the phone, and it has been used in the US military. It was also designed for use in primary care to identify individuals who are likely to benefit from onward referral and treatment.

(c) The respondents would not conceive the study as independent from the military, a fact that clearly constitutes another serious bias.

Our participants do perceive us to be independent of MoD. It is inconceivable that they would disclose alcohol dependence, self harm, etc to us if they did not believe that this information was treated confidentially and independently. Our paper in the Journal of Medical Screening explored this issue in more detail and demonstrated that people were quite happy to fill in our questionnaires about
their health, but they were reluctant to go on to consult within the military for their symptoms (see http://www.jms.rsmjournals.com/cgi/reprint/11/3/148). We have also just completed a randomised trial of members of the Armed Forces serving in Iraq comparing anonymous with non-anonymous data collection extending this observation further. We do not believe that there is bias, let alone serious bias, on this score.

(d) The authors seem to be aware of the observation that with increasingly cumulative exposure to traumatic stress the likelihood of trauma-spectrum disorders increases (e.g. Dohrenwend et al., 2006, Neuner et al., 2004) but fail to consider the implications of this effect in their study and their conclusions.

We are certainly aware of this. However cross sectional methodology is an imperfect way of measuring cumulative trauma – we have published two papers looking at recall first of general military hazards and second of recall of exposure to vaccines making this point. However, this will indeed be addressed prospectively in our follow-up study of all those who took part in Stage 1.

We look forward to hearing from you about the submission.

Yours sincerely,

Amy Iversen, Simon Wessely, and Nicola Fear