Correspondence

EDITED BY KIRIAKOS XENITIDIS and COLIN CAMPBELL

War and psychological health

The study by Hacker Hughes *et al* (2005) is interesting but I wish to raise a few points. The end of pre-deployment mental health briefing was not the best time for assessment because the soldiers were aware that they would soon be going to war and hence their stress levels must have been high. One month after the return from the war, they must have felt relieved and their stress levels must have been reduced. Since the stress levels were high at the time of initial assessment, lack of increased morbidity at the final evaluation might not mean much. It would have been more appropriate to compare stress levels at the final evaluation with those measured during peacetime.

Although the soldiers were told that the commanders would be informed about only the pooled results, they were told that military mental health practitioners would contact them confidentially if results revealed cause for concern. This means that the answers were not anonymous and hence the soldiers may have hidden their psychopathology for fear of being considered weak and the consequences of being under treatment of the military mental health practitioner. These soldiers were in the war theatre for only 4 months and it has not been mentioned how much experience of combat they had but it is known that Basra was the scene of fewer hostilities than other areas. More combat experience may be associated with a higher prevalence of post-traumatic stress disorder (Hoge *et al*, 2004).

The figures do not add up. It is mentioned that 421 soldiers out of the original sample of 899 completed the questionnaires. Later it is mentioned that 35% (n=254) completed both sets of questionnaires. The number 254 is 35% of neither the original sample (n=899) nor the sample that completed the questionnaires at follow-up (n=421). The follow-up rate is very low and hence the advantage of the study being longitudinal is minimised. It is also not mentioned how many soldiers did not volunteer for the study before and after deployment although it is mentioned that participation was voluntary.

The conclusion of the study that 'participation in war fighting may sometimes not necessarily be as deleterious to psychological well-being as has previously been thought' is premature. The small sample size compared with studies with positive findings, the high drop-out rate and lack of baseline data do not allow us to draw any conclusions from this study.

Hacker Hughes, J., Cameron, F., Eldridge, R., *et al* (2005) Going to war does not have to hurt: preliminary findings from the British deployment to Iraq. *British Journal of Psychiatry*, **186**, 536–537.

Hoge, C. W., Castro, C. A., Messer, S. C., *et al* (2004) Combat duty in Iraq and Afghanistan, mental health problems and barriers to care. *New England Journal of Medicine*, **351**, 13–22.

H. P. Jhingan Leicestershire Partnership NHS Trust, Bradgate Mental Health Unit, Glenfield Hospital, Groby Road, Leicester LE3 9EJ, UK. E-mail: harsh.jhingan@leicspart.nhs.uk

Authors' reply: In response to Dr Jhingan's letter we should first point out that the rates of morbidity obtained pre-deployment were entirely compatible with those from other studies (Rona *et al*, 2004). Furthermore, it is illogical to argue that rates of pre-deployment stress must have been high in this group because of anticipatory anxiety. Not only is there no evidence for this assertion in this population but the converse probably applies. Troops in this elite formation would have probably been looking forward to the deployment, confident in the strong belief that they were going to win (Hacker Hughes *et al*, 2006).

The argument that post-deployment stress levels would be low because of relief to be home does not allow for the influence of any adverse events in theatre. In fact, 1 month after return is the earliest time to assess for possible post-traumatic stress using the screening questionnaire (Brewin *et al*, 2002).

For a brigade such as 16 Air Assault Brigade, there is no such thing as true 'peacetime'. This brigade has, to the best of our knowledge, been deployed more often than any other in the British Army since its formation and is constantly training for, or recovering from, deployments when not on operations.

With regard to responses not being anonymised, in fact the converse applies. Soldiers may use the questionnaires as a confidential means of signalling to command, via the mental health chain, that there is a problem. In addition, there are also data from the USA to suggest that when asked questions it is only information on banned activities (such as drug use) that is significantly affected by anonymity, rather than simple distress (Adler & Thomas, 2005).

With regard to the figures, they add up perfectly. There was a population of 899 with 733 initial responses (giving a response rate of 82%); 421 completed the follow-up questionnaires and, in total, 254 of the initial 733 (35%) completed both sets.

On this basis, it is totally reasonable to have stated that, for highly trained professional soldiers involved in brief, focused operations with positive outcomes, participation in active war fighting may not be necessarily bad for mental health, at least in the short term.

Declaration of interest

J.G.H.H., F.C., R.E., M.D. and N.G. are or were employed by Defence Medical Services. S.W. is honorary Civilian Adviser in Psychiatry (unpaid) to the British Army Medical Services.

Adler, A. & Thomas, J. L. (2005) Measuring up: comparing self reports with unit records for assessing soldier performance. *Military Psychology*, **17**, 3–24.

Brewin, C. R., Rose, S., Andrews, B., *et al* (2002) Brief screening instrument for post-traumatic stress disorder. *British Journal of Psychiatry*, **181**, 158–162.

Hacker Hughes, J. G. H., Campion, B., Cameron, F., *et al* (2006) Psychological morbidity in soldiers following an emergency operational deployment. *Military Psychology*, in press.

https://doi.org/10.1192/bjp.188.3.290-a Published online by Cambridge University Press

Rona, R. J., Jones, M., French, C., *et al* (2004) Screening for physical and psychological illness in the British Armed Forces. III: The value of a questionnaire to assist a Medical Officer to decide who needs help. *Journal of Medical Screening*, **11**, 148–153.

J. G. H. Hacker Hughes Academic Centre for Defence Mental Health, King's Centre for Military Health Research, Institute of Psychiatry, London SE5 9RJ, UK. E-mail: j.hacker-hughes@iop.kcl.ac.uk

F. Cameron, R. Eldridge, M. Devon Defence Medical Services Department, Colchester, UK

N. Greenberg, S. Wessely King's Centre for Military Health Research, London, UK

Post-traumatic stress after non-traumatic events

Authors' reply: We thank Ben-Ezra & Aluf (2005) for their letter, in which they broadly support our findings (Mol *et al*, 2005) that life events may cause as many symptoms of post-traumatic stress disorder (PTSD) as traumatic events classified according to the A1 criterion of the DSM–IV. However, they also have some criticisms. Ben-Ezra & Aluf argue that 'serious illness (self)' – classified as a life event in our study – can be considered a traumatic event. We decided against this classification as many respondents had experienced an illness that was chronic but not life-threatening in the short term. However, when we re-analysed the data with 'serious illness (self)' as a traumatic event the PTSD scores of the traumatic and life events groups still did not differ (total log PTSD score 0.68 in both groups).

As suggested by Ben-Ezra & Aluf we have also excluded accidents and sudden deaths from the trauma events group, since this might be a heterogeneous group regarding the magnitude of the event. This resulted in a mean total log PTSD score of 0.76 (*v.* 0.71), which is not an essential change compared with the original difference.

Ben-Ezra & Aluf argue that the magnitude (severity) of an event is related to the likelihood of developing PTSD, and that we should have allotted events to either of our two groups on the basis of their severity. We agree that symptoms are related to severity but we found a striking overlap in PTSD symptomatology after life events and traumatic events (Tables 2 and 4) and similar mean symptom levels (Table 3).

The severity of an event can be assessed objectively and subjectively. Ben-Ezra & Aluf allude to the objective assessment but the subjective appraisal of an experience also plays an important role (McNally *et al*, 2003). It is likely that objective and subjective severity are associated with PTSD symptoms after both traumatic and life events.

Declaration of interest

The Achmea Foundation for Victim Support in Society paid the salary of S.S.L.M. but had no influence on the methodology or analyses of the study.

Ben-Ezra, M. & Aluf, D. (2005) Traumatic events *v.* life events: does it really matter? *British Journal of Psychiatry*, **188**, 83–84.

McNally, R. J., Bryant, R. A. & Ehlers, A. (2003) Does early psychological intervention promote recovery from posttraumatic stress? *Psychological Science in the Public Interest*, **4**, 45–79.

Mol, S. S. L., Arntz, A., Metsemakers, J. F. M., *et al* (2005) Symptoms of post-traumatic stress disorder after non-traumatic events: evidence from an open population study. *British Journal of Psychiatry*, **186**, 494–499.

S. S. L. Mol Department of General Practice, Julius Centre, Stratenum 6.108, UMC Utrecht, The Netherlands. E-mail: s.s.l.mol@umcutrecht.nl

A. Arntz Department of Medical, Clinical and Experimental Psychology, Maastricht University, The Netherlands

J. F. M. Metsemakers, G.-J. Dinant, P. A. P. Vilters-van Monfort, J. A. Knottnerus Department of General Practice, Maastricht University, The Netherlands

Patient-rated unmet needs and quality of life improvement

Slade *et al* (2005) have published a potentially important study of the relationship between patient-rated unmet needs, quality of life and the effect of meeting those needs. They draw the conclusion that 'meeting patient-rated unmet needs should be the starting point for mental healthcare'. Although much psychiatric care is indeed directed towards reducing unmet need, we believe that this research shows (over the time scale of the study) that reducing unmet need is actually largely ineffective. A longer study might confirm continuing incremental improvement but this would need to be demonstrated.

In the descriptive part of the study the authors show that low quality of life is associated with high unmet need. Figure 1 shows a clear gradient which can be estimated to be −0.2 by inspection (no summary statistics are given). By contrast, in the second part of the study, which looks at the effect of reducing unmet needs, Fig. 2 shows almost no relationship between change in unmet need and change in quality of life (summary statistics: $B=-0.04$, s.d.=1). Although B indicates high statistical significance it seems to be clinically irrelevant: one would have to meet 25 unmet needs to improve quality of life by one point; B is very small compared with the standard deviation and importantly is only one-fifth of the gradient in Fig. 1.

Thus quality of life and unmet need are associated (gradient=−0.2) but meeting unmet needs has a negligible effect (gradient $B=-0.04$) on quality of life. This suggests that unmet needs do not cause low quality of life and that the relationship between the two may be mediated by some third factor, such as psychiatric illness, that causes both. If this were the case, treating psychiatric illness should be the starting point for mental healthcare and not 'meeting patient-rated unmet needs'.

Furthermore, if the justification for meeting unmet needs of psychiatric patients is to improve quality of life *per se*, then this research shows that in terms of size of effect (and over the period of the study), reducing unmet need is largely ineffective, and is therefore a questionable use of resources.

Slade, M., Leese, M., Cahill, S., *et al* (2005) Patient-rated mental health needs and quality of life improvement. *British Journal of Psychiatry*, **187**, 256–261.

D. McQueen Northgate Clinic, Edgware Community Hospital, West London Mental Health Trust, London W10 4JD, UK. E-mail: daniel.mcqueen@virgin.net

P. St John-Smith Barnet, Enfield and Haringey NHS Mental Health Trust, London, UK

Authors' reply: We are grateful to Drs McQueen & St John-Smith for their response, which highlights that our study raises the question of the purpose of mental healthcare.

We agree that the effect we showed is small but we believe it is more meaningful than that shown by other study designs. Our data comprised repeated measures at monthly intervals over 7 months, and we demonstrated temporal precedence in the relationship between patient-rated unmet need and quality of life – reduction in the former precedes improvement in the latter. Cross-sectional studies more easily demonstrate apparent associations, which prove on further investigation to be spurious.

The analysis controlled for baseline symptomatology (assessed using the Brief Psychiatric Rating Scale) and diagnosis, and found no evidence of a mediating role

https://doi.org/10.1192/bjp.188.3.290-a Published online by Cambridge University Press