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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.

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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.

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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

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