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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

https://doi.org/10.1192/bjp.188.3.291-b Published online by Cambridge University Press

for psychiatric illness. Furthermore, our use of random-effects regression models controlled for further unmeasured individual characteristics that are stable over time. Our finding of a modest but robust effect is meaningful and therefore clinically important, especially when combined with other small effects. Further research into determinants of quality of life will provide other levers of change for improvement, which are unlikely to be staff-rated symptomatology (Lasalvia *et al*, 2002).

We agree that interventions to improve mental health will have an impact on patient-rated unmet need, which in turn (as we demonstrate) will improve quality of life. However, the advantage of identifying a modest but robust causal relationship is that it highlights the importance of a more comprehensive approach to meeting needs. Mental healthcare that focuses exclusively on treating psychiatric illness can risk neglecting the importance of other consequences of mental ill health, such as discrimination in travel (Driver and Vehicle Licensing Agency, 2005), insurance (Association of British Insurers, 2003) and debt (Meltzer *et al*, 2002). Mental health services that also address a wide range of health and social needs (as, for example, assessed in our study by the Camberwell Assessment of Need) are more likely to improve quality of life.

Declaration of interest

The Health Services Research Department, where this study was based, receives royalties from sales of the *Camberwell Assessment of Need* published by Gaskell.

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Abstinence-oriented treatment for opiate addiction

Smyth *et al* (2005) reported outcomes of abstinence-oriented in-patient treatment for opiate users at 2–3 years and found that 23% of patients were abstinent for the preceding 30 days according to self-report without methadone maintenance. At the start of the treatment 49% had injected heroin. There was, however, a group of patients who were truly abstinent: those who had died.

Of the 109 patients who had been located out of the original 149, 5 had died. The total expected number of deaths from the original sample would therefore be closer to 7, but would perhaps be even higher if we assume that those lost to follow-up led more 'chaotic' lifestyles. The authors rightly note that abstinence-oriented treatment is associated with accidental overdose (Strang *et al*, 2003).

In Glasgow, before the advent of supervised consumption, rates of methadone-related overdose were around 2.5 per 100 treatment-years. This rate fell to less than 0.5 per 100 treatment-years (Advisory Committee on the Misuse of Drugs, 2000) after the supervised consumption of methadone was introduced. Supervised methadone consumption is known to be effective in reducing the risk of overdose and there is a dose-related effect in reducing mortality, with doses over 75 mg being more effective than doses below 55 mg (van Ameijden *et al*, 1999). Methadone also reduces the risk of injecting; this in turn reduces viral transmission, which is the other significant risk of increased mortality among drug users (Dolan *et al*, 1998).

However, the attitude of treatment agencies towards extended maintenance is changing in the direction of delineated treatment episodes (National Treatment Agency for Substance Misuse, 2005). In these days of crack cocaine, the belief that methadone treatment works (Gossop *et al*, 2003) and saves money (Godfrey *et al*, 2004) has diminished. This is despite evidence for interventions such as contingency management and cognitive–behavioural therapy using substitute prescribing (Rowan-Szal *et al*, 2004).

Of course, abstinence should be a potential goal of drug treatment. Deciding those patients for whom abstinence-oriented treatment is appropriate, and the risk of such treatment, is more difficult. There is no reliable evidence for matching patients to optimal treatments in addiction. However, those who inject, isolated users and alcohol/benzodiazepine co-users are all over-represented in the morgue (Warner-Smith *et al*, 2001). Risk awareness might well be a reasonable first step and for many abstinence might be more dangerous than desirable.

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Author's reply: I agree with Critchlow & Nadeem that abstinence-based treatment may only be appropriate for a minority of opiate-dependent patients and that risk awareness is an essential first step for both patient and treatment provider. There is an increased risk of accidental overdose in the

https://doi.org/10.1192/bjp.188.3.291-b Published online by Cambridge University Press