Correspondence

Edited by Kiriakos Xenitidis and Colin Campbell

Post-traumatic stress disorder’s future

Rosen et al’s editorial raised problems associated with criteria that creep into the diagnosis of post-traumatic stress disorder (PTSD). Conditions including grief, relationship problems, dental care, abortion, traumatic television and humiliating events have entered the arena of PTSD. I support their appeal to psychiatrists to adopt a narrower definition, but beg to go further.

The DSM series has been invaluable for taking the science of psychiatry from its infancy to its adolescence of today. However, we now need to look towards maturity when we will use conceptualisations that involve true entities instead of symptom collections. What do we currently mean by PTSD? Both ‘stress’ and ‘traumatic’ are so non-specific they are now virtually meaningless – not to mention the ‘P’ and ‘D’. According to the authors’ concerns, the broadened concept of PTSD might euphemistically be described as ‘Post Something Really Horrible Disorder (PSRHD)’.

Panksepp proposed a preliminary taxonomy of distinct emotional modal systems (i.e. core emotions), supported by neuroscientific findings complemented by an evolution-based approach. I suggest that for the high-prevalence conditions comprising most of psychiatry, neuroscience without consideration of evolutionary adaptiveness is plain stupidity, as many of the relevant genes would not have persisted without adaptiveness.

Much of the PTSD bracket relates to the multiple forms of depression (a loss phenomenon) already catered for in the DSM. I have proposed that PTSD should be viewed as a disorder of defence involving extreme fear as the core emotion. As such, some improvements to the DSM criteria can easily be accommodated such as differentiating the sleep disturbance associated with depression from other disorders. The criterion C7 (sense of foreshortened future) should be suppressed from other commonsense improvements (see Cantor, pp. 124–26).

The notion of ‘Post Terrible Scare Disorder’ might be a more scientifically valid concept, if lacking in elegance as a term.


Rosen et al observed that the clinical presentation of PSTD is not restricted to those who have experienced severe trauma, that patients who are traumatised do not necessarily develop PTSD and that PTSD is often misdiagnosed. We would add that there is almost no evidence that PTSD is reliably diagnosed in ordinary clinical settings. In our naturalistic study of expert reports about psychological injury after motor vehicle accidents, we found that the agreement about the presence of PTSD by experts engaged by the same side in the court case was little better than by chance. Most of the disagreement seemed to be due to selective use of the diagnostic criteria, although there was also difference in opinion about the severity of the patients’ experiences and hence whether they met the ‘A’ criteria.

A search of PubMed, PsychLit and CINHAL did not locate any studies to show that PTSD can be reliably diagnosed without the use of a structured or semi-structured interview. The DSM–III and ICD–10 field trials did not report the interrater reliability of PTSD and the DSM–IV trials restricted the examination of the reliability to the rating of audiotapes of 25 patients’ responses to the PTSD module of the Structured Clinical Interview for DSM. Furthermore, we have not been able to ascertain whether the very high kappa scores reported in the DSM–IV trials (κ=0.85) included a correction for the loss of degrees of freedom arising from the use of the same ratings in multiple-rating pairs.

Although there are numerous studies confirming the interrater reliability of various diagnostic instruments, many of the instruments are only administered when the patient is suspected of having the disorder, and their ability to reliably distinguish PTSD from other disorders is not well established. Despite their limitations, we support the call of Miller for the routine use of diagnostic interviews, as there is no evidence the disorder can be reliably diagnosed in any other way.

Rosen, Spitzer & McGough call for DSM–V criteria that reflect research findings and limit the potential for misuse of the diagnosis. We believe that the logical step would be the complete removal of the A criteria. This would separate the clinical assessment of the patients’ psychological state from issues of causation and minimise pre-emptive decisions about the cause and nature of the patient’s distress. This new disorder, which could be called ‘phobic memory disorder’ or another name that does not imply a particular cause, could then be diagnosed in the usual way. As there are likely to be few objective features of the disorder, the diagnosis should be made using a semi-structured interview for the new criteria. Causative factors, including the role of trauma, premorbid conditions and litigation, would be considered in the same way as in other disorders.


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**Authors’ reply:** We welcome the responses by Cantor and Nielsen & Large to our editorial. On the lighter side, we observe that yet more proposals for post-traumatic conditions are proposed by Cantor (e.g. PSRHD), thereby demonstrating an ever increasing incidence of ‘acronymitis’. This disorder, characterised by a seeming compulsion to develop acronyms, was to the best of our knowledge first labeled by Isaac Marks (personal communication, 2005).

On a more serious note, we would like to use our limited space to highlight several observations that we have taken from an extensive review of the PTSD construct. This review proposes that PTSD’s defined clinical syndrome might best be conceptualised as encompassing a broad range of reactions to adverse events that are in turn influenced by multiple dimensionally distributed factors (e.g. pre- and post-incident risk variables, peri-traumatic appraisals and real-life consequences). The long history of general stress studies, and more recent research on PTSD, has demonstrated that these multiple factors and their complex interrelations yield a wide range of outcomes after adverse events. Within this framework, it remains an open question whether any attempt to define a distinct post-traumatic syndrome can lead to a true disorder in nature that is specific to a subset of stressors. Perhaps such a disorder exists, and PTSD or some other acronym should remain in the psychiatric literature spurred by the creation of PTSD or some other acronym should remain in the psychiatric nosology. Perhaps such a disorder exists, and PTSD or some other acronym should remain in the psychiatric nosology. Perhaps such a disorder exists, and PTSD or some other acronym should remain in the psychiatric nosology. Perhaps such a disorder exists, and PTSD or some other acronym should remain in the psychiatric nosology. Perhaps such a disorder exists, and PTSD or some other acronym should remain in the psychiatric nosology. Perhaps such a disorder exists, and PTSD or some other acronym should remain in the psychiatric nosology. Perhaps such a disorder exists, and PTSD or some other acronym should remain in the psychiatric nosology. Perhaps such a disorder exists, and PTSD or some other acronym should remain in the psychiatric nosology.

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Even in terms of efficacy, the choice of lithium remains arguable. For example, in the meta-analysis by Smith et al., when relapses due to depression were analysed, there were more relapses with lithium compared with placebo. For example, in the meta-analysis by Smith et al., when relapses due to depression were analysed, there were more relapses with lithium compared with placebo. For example, in the meta-analysis by Smith et al., when relapses due to depression were analysed, there were more relapses with lithium compared with placebo. For example, in the meta-analysis by Smith et al., when relapses due to depression were analysed, there were more relapses with lithium compared with placebo. For example, in the meta-analysis by Smith et al., when relapses due to depression were analysed, there were more relapses with lithium compared with placebo.

**Lithium in mood disorders: a one-sided re-appraisal**

To the uncritical mind, it appears as if Young & Hammond have made a case for more use of lithium in mood disorders than is currently the trend. They partly based their argument on the meta-analysis by Smith et al. A close perusal of the meta-analysis, however, revealed that the case made by Young & Hammond for lithium is one-sided, unbalanced and may be misleading. Even though the study by Smith et al. stated that lithium remains the medication with the strongest evidence base, we believe that its declining use may be due to incontrovertible evidence of adverse effects. For example, in the meta-analysis by Smith et al., when withdrawals for any reason and withdrawals for adverse events were analysed, there were more withdrawals with lithium compared with lamotrigine, valproate semisodium and olanzapine. Even in terms of efficacy, the choice of lithium remains arguable. For example, when relapses due to depression were analysed, Smith et al. found that there were more relapses with lithium than with lamotrigine and valproate semisodium. In terms of manic episode, there were more relapses with lithium compared with olanzapine, and in terms of any mood episode, there were more relapses with lithium than with lamotrigine and valproate semisodium and olanzapine.

We do not advocate for any particular medication but we strongly feel that for this type of medication advocacy, authors should attempt to provide a balanced rather than one-sided argument. It is also patronising to partly ascribe the declining use of lithium to poor training of psychiatrists rather than acknowledge the fact that psychiatrists may actually base their choices on individual patient criteria as well as the profile of medications within the wide array of available agents.

Lithium in mood disorders: a one-sided re-appraisal
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