Post-traumatic stress disorder's future

Rosen et al’s editorial\(^1\) 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\(^2\) proposed a preliminary taxonomy of distinct emotional modular 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.\(^3\) As such, some improvements to the DSM criteria can easily be accommodated such as differentiating the sleep disturbance associated with depression. In the DSM, a diagnosis of PTSD 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.


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 nomenclature. For the moment, however, it appears that the very literature spurred by the creation of PTSD has demonstrated somewhat, somewhat ironically, that the construct is flawed. It is in the context of these concerns that we acknowledge issues raised in the Journal’s letters and encourage continued discussion on the validity of the PTSD diagnosis.


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

To the uncritical mind, it appears as if Young & Hammond1 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.2 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 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.


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