BJPsych | The British Journal of Psychiatry (2011) 198, 156–159

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Edited by Kiriakos Xenitidis and Colin Campbell

Contents

Continuing lack of evidence for the psychotic subtyping of PTSD

Gaudiano & Zimmerman[1] conclude that psychotic symptoms in post-traumatic stress disorder (PTSD) are associated with comorbid conditions, especially major depressive disorders, and that their results therefore do not support the existence of a psychotic subtype of PTSD. However, they did not evaluate certain factors that might be responsible for misinterpretation of their results. First, they did not report the severity of post-traumatic and depressive symptoms. It is possible that patients with PTSD without comorbid depressive disorder had a milder post-traumatic disorder and consequently less probability of presenting with psychotic symptoms. Second, in clinical practice the congruence of delusions and hallucinations with traumatic events seems to be distributed across a continuum: at one extreme there is complete congruence with trauma and at the other there are exuberant and bizarre symptoms similar to those described in schizophrenia. The elucidation of the possible existence of a psychotic subtype of PSTD must necessarily include the development of adequate instruments to measure severity and congruence of psychotic symptoms in 'non-psychotic' conditions (e.g. mood and anxiety disorders), as well as their biological correlates.

1 Gaudiano BA, Zimmerman M. Evaluation of evidence for the psychotic subtyping of post-traumatic stress disorder. *Br J Psychiatry* 2010; **197**: 326–7.

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doi: 10.1192/bjp.198.2.156

Authors' reply: We welcome the response by Brietzke and colleagues to our report. First, they suggest that differences in illness severity between those with *v.* those without the comorbidities we excluded might have accounted for reductions in the prevalence rate of psychotic symptoms in our sample. To help address this point, we reanalysed our data by conducting a logistic regression of data for PTSD patients with *v.* without the excluded comorbidities as a predictor of the likelihood of having psychotic symptoms while controlling for Global Assessment of Functioning scores. Comorbid status remained a significant predictor. It is important to clarify we did not exclude all comorbidities from the PTSD sample: we removed only those that also allow for the presence of psychotic symptoms (e.g. schizophrenia, bipolar disorder). Other comorbid diagnoses (e.g. anxiety disorders) remained in the refined PTSD sample showing the low prevalence of psychosis. Therefore, we do not believe that differences in illness severity can adequately explain our findings.

Second, Brietzke *et al* point out that psychotic symptoms in PTSD probably fall along a spectrum from congruent (e.g. hallucinations related to vivid re-experiencing of the trauma) to non-congruent (e.g. bizarre, non-trauma-related hallucinations). We agree with this observation in general, but disagree that congruence criteria are likely to clarify these issues from a diagnostic standpoint. The distinction between congruent *v.* non-congruent psychotic symptoms in primary mood disorders is known to lack prognostic value and patients frequently exhibit characteristics of both at the same time.[1]

We agree with Rosen & Lilienfeld[2] that continued conceptual confusion regarding the PTSD diagnosis suggests the need for greater caution, rather than a rush to expand the criteria to encompass larger groups of clinical presentations, until the validity of the core features of the PTSD diagnosis can be better established. We also disagree with Breitzke *et al* that investigations of biological correlates of PTSD are likely to shed much more light on these issues. Extensive previous research in this area has found a lack of evidence for biomarkers linked specifically to the PTSD diagnosis as opposed to those that cut broadly across diagnostic categories and clinical presentations.[2]

1 Gaudiano BA, Uebelacker LA, Miller IW. Course of illness in psychotic mania: is mood congruence important? *J Nerv Ment Dis* 2007; **195**: 226–32.

2 Rosen GM, Lilienfeld SO. Posttraumatic stress disorder: an empirical evaluation of core assumptions. *Clin Psych Rev* 2008; **837**: 837–68.

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doi: 10.1192/bjp.198.2.156a

Is transference-focused psychotherapy really efficacious for borderline personality disorder?

In their study of treatment for borderline personality disorder, Doering *et al* state that their results 'demonstrate the significant superiority of transference-focused psychotherapy with regard to the primary outcome criteria of drop-out rate and suicide attempts during the treatment year' when compared with treatment by experienced community psychotherapists.[1] They report that 'significantly fewer participants dropped out of the transference-focused psychotherapy group (38.5% *v.* 67.3%) and also significantly fewer attempted suicide ($d = 0.8$, $P = 0.009$)'.

In our view, this interpretation of primary outcome criteria might lead to misunderstandings. As regards suicidality, the authors suggest that the *P*-value of 0.009 would relate to absolute numbers of attempted suicides during the treatment year. However, the actual difference in suicide attempts during the treatment year (7/52 *v.* 11/52) is not significant ($P = 0.44$, continuity-corrected χ^2-test, LOCF analysis). The significant *P*-value reported by the authors seems to correspond to change scores (defined as 1/0/−1 by the authors), not to suicide attempts during the treatment year, which seems to be the outcome as defined in the study protocol (trial NCT00714311). The authors further suggest that the effect size of 0.8 would refer to the between-group comparison. However, the reported effect size seems to correspond to the within-group comparisons reported in Table DS2. The between-group effect size for suicide attempts during the treatment year would be small.