British Journal of Psychiatry (1986), **149**, 244–247

Correspondence

Mania Following Bereavement

SIR: The assumption is not uncommonly made in papers that since there have been few reports of a particular event, relationship, etc., in the literature, this means that the phenomenon is rare or uncommon. A case in point is Rosenman and Taylor's case report of mania following bereavement (*Journal*, April, 1986, **148**, 468–470). Surely such assumptions are quite erroneous. How many clinical psychiatrists take the trouble to report such cases in the literature? I wonder if there is the slightest connection between cases reported and cases actually occurring.

J. A. G. WATT

Gartnavel Royal Hospital
1055 Great Western Road
Glasgow G12 0XH

SIR: The case report by Dr Rosenman and Ms Taylor (*Journal*, April 1986, **148**, 468–70) is further evidence of the continuing difficulty there is in ridding modern psychiatry of Freud's erotic fantasies. As Oscar Wilde might have said, the only absurdities tenaciously believed are the really unbelievable ones.

In their discussion, the authors refer to the "paradoxical nature of the response". The false paradox is in their use of the words "grief" and "mania" as opposites. Grief is a normal experience and the opposite might be happiness. The patient was not just happy but described as profligate, grandiose, talking constantly, overbearing and irritable. In a word, manic—an illness, the opposite of which is a state of endogenous depression which itself is not merely grief. Does anyone really expect mania to occur only after weddings?

While one can accept the seemingly jolly clown entertaining everyone while his heart is broken because of his lost love, it is going a bit too far to see the same 'manic defence' in the patient described whose clear bipolar affective vulnerability was shown by the puerperal depression preceeding the manic episode. A non-specific stress reveals a vulnerability (the nub of the matter is that this is metabolic and not primarily psychological) specific to the patient which might, in different individuals, be anything from skin disturbances to cardiac problems. Or was Dr Rosenman tempted to treat the lady with antidepressants? Freud was very bad on the functional psychoses, unbelievably so.

PAUL BRIDGES

The Geoffrey Knight Psychosurgical Unit
Brook General Hospital
London SE18 4LW

Gluten Free/Gluten–Load Trial

SIR: I read the paper by Dr Vlissides *et al* (*Journal*, April 1986, **148**, 447–452) with interest, and have a few points to make.

The pathophysiological processes in gluten sensitive enteropathy or coeliac disease are totally independent of the pathophysiological processes in schizophrenia. I have raised this because constantly in the literature coeliac disease and schizophrenia are brought nearer to each other during discussion. In gluten sensitive enteropathy improvement in most patients is seen in one month, and one should not expect such dramatic changes in the form of improvement in schizophrenia in a short time, as the pathophysiological processes in causation of their symptoms are not the same.

Removal of glycoprotein gluten from the diet may help to improve symptoms of schizophrenia, but addition of gluten would not necessarily make symptoms worse. If one selects severely ill psychotics, whose symptoms are at a peak, further deterioration in clinical symptoms by addition of gluten to the diet cannot be adequately measured by existing clinical rating scales. One may expect only no change in scores.

Having used gluten free diet and essential fatty acids in the treatment of schizophrenia with some success, I recommend a minimum period of eight to ten months must elapse before the gluten free diet is considered not beneficial. Nutritional therapies generally take a long time to cause any changes in the body. The total period in the above papers was extremely short.

K. S. VADDADI

Crawley Hospital
Crawley, West Sussex, RH11 7DH

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