

in readmission rates in puerperal manics over controls. Our manic patients would have included patients who were diagnosed as schizoaffective, manic using the Research Diagnostic Criteria (RDC). There was an unequivocal increase of first-rank symptoms in our postpartum manics when compared with non-postpartum manics. The RDC would have called such patients schizoaffective, manic. Considerable data suggest that mania and schizoaffective, mania are manifestations of the same disease (Clayton, 1982).

Dr Platz and Professor Kendall reported twelve patients with manic disorder and six with schizoaffective disorder. I do not know how many of those schizoaffectives were schizoaffective, manic, although this is an important point. The simple fact that the readmission rate for schizoaffective patients was more like that of the manic patients than of the depressive patients suggests that most of them may have been manic. These 18 puerperal patients had 36 readmissions (2.0 per patient) and the controls 56 (3.1 per patient), a 55% increase. The duration for the combined puerperal patients was 16.2 weeks per patient, compared with 22.2 weeks per patient for controls (a 68% increase). It should be tested as to whether these differences are significant.

In the discussion, the authors suggest that childbirth may be a "uniquely potent" precipitant of psychotic illness, and that it is plausible "that it should be capable of precipitating episodes of illness in women with only a moderate genetic or constitutional predisposition to affective disorders". I do not think this follows. If one assumes that childbirth is a "uniquely potent" precipitant, all bipolar patients, including those that have episodes independent of childbirth, should have mania following childbirth. In fact, a significant proportion do not (Reich & Winokur, 1970). This suggests an alternative explanation: i.e. that puerperal psychoses in some proportion may have a different illness from that which is ordinarily seen as non-postpartum mania.

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Stuttering and Anxiety

SIR: I was interested in the paper by Drs Andrews and Craig on prediction and outcome after treatment for stuttering (*Journal*, August 1988, **153**, 236–240), but, as a psychiatrist who stutters, I feel competent to suggest a clarification of their premise that stuttering is not associated with anxiety or nervousness. The psychometric evidence the authors cite supports the view that stuttering does not indicate trait anxiety, but says nothing to counter the possibility that the phenomenon indicates situational anxiety. Indeed, a consideration of the suggested aetiology of the disorder indicates that it would not be surprising if a stutterer stuttered more when anxious. Drs Andrews & Craig tell us that stuttering is a consequence of inefficient sensory motor integration of speech at the cortical level, and that this is why various tactics, such as adopting a masking tone, are effective, at least temporarily, in reducing the frequency of stuttering. From this, then, it seems reasonable to suggest that conditions which affect cortical function generally may affect the frequency of stuttering. One such condition would be the alerting consequence of the adoption of the fight/flight posture, which is also characterised by the symptoms of situational anxiety. The intensity of the fight/flight position diminishes when the decision to flee or to fight is made, and this would explain why stutterers regain a degree of fluency after this decision. For example, when he or she is angered and goes on the offensive in an argument, or when a difficult interview over which he or she has no control comes to an end, the stutterer tends to become more fluent. An analogy with epilepsy may be relevant; the stutterer is more likely to stutter when tired, just as the epileptic is more likely to have a fit. This suggests that just as fatigue reduces the epileptic threshold, so too it reduces cortical efficiency in carrying out the sensory motor integration tasks required for fluent speech.

This model has implications for the interpretation of Drs Andrews & Craig's results. They identify learning skills to control stuttering as "probably the most important factor" in long-term successful outcome of treatment. This model would suggest that these skills are in fact specific anxiety management techniques, aimed at increasing cortical efficiency by adopting learned patterns of response to a fearful situation. In this context, therefore, it is not surprising that duration of treatment significantly correlates with improvement.

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