

Suicide has previously been reported (Bebbington, 1976), but we are not aware of a report of fire setting associated with this condition, thus indicating that this may be a rare occurrence. However, fumigation is often used by these patients and it seems essential to explore any attempts or intentions by them to use fire or smoke to combat the infestation, and to consider admission in order to protect both the patient and neighbours. Many such patients relapse when treatment is stopped, which raises legal and ethical difficulties when long-term treatment is attempted.

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group being at higher risk, because after three months the relapse rates are identical.

Finally, no firm conclusions can be drawn from a retrospective study, hence the title poses a question. Correspondence in the same issue (*Journal*, February 1987, 150, 264–265) has highlighted the fact that despite increasing use of lithium there has been an increasing readmission rate for mania at a number of different centres. There has been no satisfactory explanation for this. One possibility is that repeated lithium withdrawal increases the number of relapses, and I think this topic deserves further consideration.

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References

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Is there a Lithium Withdrawal Syndrome?

SIR: The letter from Hawkins & Shepherd (*Journal*, February 1987, 150, 273) contained a number of inaccuracies and misconceptions.

Firstly, they state that the words 'withdrawal' and 'relapse' are not interchangeable. I make it clear in my opening sentence that the paper is concerned with "the possibility of relapse being caused by drug withdrawal". Their criticism would seem to be an unnecessary exercise in semantics.

Secondly, they state that to show a withdrawal effect it is necessary for there to be a "fall in the relapse rate lower than expected for a period after the withdrawal syndrome has ended". This is clearly wrong. The question is whether there is an *increased* risk of relapse in the withdrawal period in addition to that which would be expected taking into consideration the natural history of the disease process. This is not the same thing.

Thirdly, they state that the theoretical relapse rate "can never be known", and ask "How can one distinguish a withdrawal state causing relapse and relapse alone?" Perhaps they are not aware that this is why control groups are used in order to estimate the theoretical relapse rate. In the control group only 8% of patients relapsed in the first three months, compared with 28% in the experimental group. This difference cannot be explained by the experimental

BITE: Self-rating Scale for Bulimia

SIR: The paper by Henderson & Freeman (*Journal*, January 1987, 150, 18–24) is a useful and timely development in the scientific study of eating disorders. However, four points require further consideration.

Firstly, the authors need to clarify whether they regard the BITE as a screening test or as a diagnostic instrument. The statement that "subjects achieving a high score have a high probability of meeting... criteria for bulimia" indicates clearly that it is a screening test. However, the observations that the BITE "can be used to *identify* binge-eaters in a given population" (our emphasis), and that it provides the information necessary to make a DSM-III diagnosis of bulimia, suggest that the authors also consider it to be a diagnostic instrument.

Secondly, the criterion for caseness is unclear. In study 1, the only criterion given for the patient group is that they were binge-eaters at "various stages of treatment". This is too imprecise – an operational definition is a central requirement for work of this nature. The absence of such a definition from study 1 is all the more mysterious since one was used in study 2. Where diagnostic criteria are considered, it is not always clear which are meant. DSM-III criteria were used in studies 2, 3, and 4, while the authors conclude, in their instructions for administration, that high scorers have a high probability of meeting "DSM-III criteria for bulimia *and* Russell's (1979) criteria for bulimia nervosa" (our emphasis). There is a crucial difference between the two: DSM-III criteria are relatively broad, in contrast to Russell's criteria which require evidence of a morbid fear of fatness. By which criteria does the BITE identify cases?