attempt to standardise the diagnosis of post-natal depression – cases were divided into mild, moderate and severe pending on how they were treated by their various general practitioners.

The fact that the authors were encouraged to use progesterone as a sole therapy for puerperal mania by their experiences with two patients quite frankly astounds me. The first patient reported a 'subjective calming effect' when progesterone was given (50 mg intramuscularly) before and after neuroleptic therapy was commenced – nothing particularly encouraging about that. The second women's improvement was most likely due to the fact that she was given haloperidol (40 mg intramuscularly – a high dosage) and chlorpromazine (50 mg intramuscularly) during the 48 hours before recovery.

Clearly, hormonal changes in the puerperium may be one of the factors that precipitate a psychotic illness in susceptible individuals but to expect that progesterone might be successful as a therapy for puerperal mania is in my view being rather simplistic.

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Benzodiazepine withdrawal

SIR: With reference to Ashton *et al*'s paper on buspirone in diazepam withdrawal (*Journal*, August 1990, **157**, 232–238) we would like to make the following points.

We question the clinical relevance of this study. It is now generally accepted that gradual dose reduction with attention to appropriate psychological treatment is the best way to manage benzodiazepine withdrawal (Edwards *et al*, 1990). If this is done at the patient's own rate, pharmacological treatment of symptoms, which may complicate the withdrawal process, as indeed occurred in this study, should be unnecessary.

We think the study was unethical for two reasons: firstly, withdrawal was rapid and took no account of either the starting dose or the patient's response to withdrawal; and secondly, a blind study deprives the patients of the right to determine their own rate of withdrawal and the opportunity to learn from this experience. Did informed consent include telling patients that this was not the best way to come off benzodiazepines, was likely to create unnecessary distress and that buspirone was unlikely to help?

The design of the study is unsuited to small numbers. Unmatched groups, a failure to control for attending a support group or concurrent prescribing, and the high drop-out rate in the buspirone group make it difficult to draw any useful conclusions.

Finally, the study ignores important psychological factors which are crucial to maintaining abstinence. The importance of patients being in control of their withdrawal, learning non-drug alternatives and improving their quality of life makes a psychological approach more appropriate than a pharmacological one.

This study only perpetuates the search down a blind alley for pharmacological short-cuts which fail to respect the patient's right to participate in the decisions, manage the withdrawal and be offered alternative ways of coping.

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EDWARDS, J. G., CANTOPHER, T. & OLIVIERI, S. (1990) Benzodiazepine dependence and the problems of withdrawal. *Postgraduate Medical Journal*, **66** (suppl. 2), S27–S35.

SIR: I would like to congratulate Cantofer *et al* (*Journal*, March 1990, **156**, 406–411) on their study. Benzodiazepine dependence is a difficult condition to treat and an attrition rate as low as they obtained must indicate considerable enthusiasm. However, I am slightly surprised at the design of the study which appears confounded by having two variables, in that patients were allocated either to abrupt withdrawal and active treatment with propranolol or gradual withdrawal and placebo propranolol. From the study design one could draw the spurious conclusion that propranolol is no benefit for the patient withdrawing from benzodiazepines. I believe there is fairly good evidence that propranolol is of benefit in benzodiazepine withdrawal, at least as far as somatic symptoms are concerned (Halstrom *et al*, 1988). The other main finding of the study, that gradual withdrawal is easier than abrupt withdrawal, is already well supported in existing literature. However, to make such a deduction from the study is an error in