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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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 logic because of the confounding variable of active or inactive treatment.

Perhaps I have overlooked some strength in the methodology chosen; I would be glad to be corrected if this is the case.

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ECT in neuroleptic malignant syndrome

SIR: In his otherwise impressive update on neuroleptic malignant syndrome (NMS), Kellam (*Journal*, August 1990, **157**, 169–173) seems to have included some factual inaccuracies while discussing the treatment of continuing or recurring psychosis after the successful treatment of NMS. Dr Kellam quotes Lazarus (1986) to the effect that electroconvulsive therapy (ECT) is safe after the syndrome has subsided. Lazarus, in his case report, suggested that ECT may offer a safe and rapidly effective intervention in cases of NMS unresponsive to supportive medical therapy.

Similarly, Hermesh *et al* (1987) recommended ECT for the treatment of NMS episodes rather than for continuing or recurring psychosis after the successful treatment of NMS. In my rejoinder to Hermesh *et al* (1987), I did not argue about the safety of further neuroleptic treatment for psychotic illness as Kellam seems to imply. I suggested that the drug treatment (i.e. bromocriptine or dantrolene) should be tried for the management of NMS before resorting to ECT (Adityanjee, 1987). I did mention having used ECT for the management of recurring psychosis after NMS had subsided (Adityanjee, 1987; Adityanjee & Chawla, 1989). Similar practice was adopted by Aizenburg *et al* (1985) who used ECT for the management of psychotic illness after NMS had resolved successfully with supportive treatment only.

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Was Hitler a Christian?

SIR: There are a number of dubious assumptions and implications in Samuel's (*Journal*, July 1990, **157**, 151) argument that Hitler, Mussolini, Stalin, Franco and Hoess were all Christians. Firstly, there is the assumption that anyone who professes to be a Christian is one by definition. Arguably, such a profession does fit the dictionary definition given by Philip Timms, although I would feel that in this context the use of the word 'profess' should more meaningfully be taken to include observance and practice of, rather than simply confession of, faith. Secondly, there is an implication that the failure of the Roman Catholic Church to proscribe *Mein Kampf* amounts to an acceptance of its contents not only by the Catholic Church (which is itself highly debatable) but also by the wider Christian Church as a whole.

The most serious implication of Dr Samuel's argument, which is both unjustified and offensive, is that there is some causal connection between the professed Christian faith of the individuals named and the acts of atrocity and inhumanity that they perpetrated during their lives. The lives that they led, as recorded in history, clearly betray the insincerity of any profession of Christian faith that they made. The teachings of Christ made clear that it was not those who 'professed' righteousness, but those who were truly repentant, who found favour in God's sight.

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This correspondence is now concluded.

Anorexia nervosa and OCD

SIR: I read with interest the paper by Holden (*Journal*, July 1990, **157**, 1–5) on the evidence for a

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