two years, and Brincat *et al* (1984) have shown a recurrence of depression and other related symptoms when patients well controlled with implants for many years are then given a placebo. The same percutaneous implantation of 100 mg oestradiol every six months was used to treat depression in younger women with pre-menstrual syndrome and was found to be more effective than placebo in every Moos cluster of symptoms including negative affect (Magos *et al*, 1986). This is not transient as a sustained improvement has been reported after five years of such therapy (Watson *et al*, 1990).

Oestradiol implants have a prolonged duration of action which may be undesirable in some patients. Transcutaneous oestradiol patches in high doses (Estraderm, 200 μg) which do not have this long-term characteristic have also been studied with equally impressive effects on depression when compared with placebo (Watson *et al*, 1989).

These findings are not unique to our clinic but the space and number of references permitted in this letter do not allow me to give details of data from Montreal, Cardiff and London which support this view.

All doctors who treat depressed peri-menopausal women, regardless of the finer definitions of depression, should be aware of the potential that oestrogens have in relieving the suffering of some (or many) of these women. Nobody makes any claims that oestrogen therapy is a panacea for all the psychiatric problems of middle age, but patients deserve that the place of this therapy is evaluated carefully and not dismissed in such an unscholarly review.

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AUTHOR'S REPLY: Dr Studd and his colleagues express some dissatisfaction about the choice of references in my review, but I can only reiterate that the majority of general population studies do not support the view that the menopause or climacteric is associated with a significantly increased risk of psychiatric disturbance in women. In addition, the majority of treatment studies do not support the view that oestrogen has a specific antidepressant effect, but many women feel better if their symptoms of flushing and sweating are effectively relieved and this complicates the interpretation of many studies.

The 'ovarian cycle syndrome' described by Dr Studd and his colleagues and said to be common is difficult to evaluate in that the physical symptoms described may well make a woman feel depressed and, conversely, a depressed woman may be much less tolerant of, and more disturbed by, these cyclical changes in sensation. Symptoms such as loss of energy and loss of libido are certainly very common in straightforward depressive illness.

Several studies have shown that women attending gynaecology out-patient clinics have higher levels of psychiatric morbidity than matched controls from the general population (Munro, 1969; Worsley *et al*, 1977; Byrne, 1984). Dr Studd and his colleagues will see many depressed women who benefit from their clinic attendance. However, I would question the view that oestrogen implants are the main therapeutic agents as far as psychiatric symptoms are concerned and would suggest that it is the very obvious concern and enthusiasm of the staff that is of prime importance, as suggested in the study of Strickler *et al* (1977).

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The 'new cross-cultural psychiatry'

SIR: Littlewood (*Journal*, November 1990, **157**, 775–776) has misidentified a 'conventional error' in my editorial. I did not suggest that culture should be held

constant. I was concerned with the extent to which folk concepts of mental illness need to be ascertained before embarking on any comparative study involving psychiatric symptoms and/or diagnosis. While it may be an ideal counsel to recommend this groundwork before any comparative psychiatric study is undertaken, it is simply not practical and will not be followed. I introduced my example to suggest that differences in folk concepts of mental illness between native whites of Salford and London are unlikely to be of a magnitude to invalidate the use of the same criteria for psychiatric symptoms and diagnosis across the populations. This strategy does not of course exclude the study of other cultural differences between the two populations.

On another issue, it is Dr Littlewood's perception that I have 'put down' local Yoruba knowledge about smallpox. This is a clear illustration of the different value systems held by anthropologists and medical practitioners (although Dr Littlewood is both, in this instance he is taking an anthropologist's stance). As discussed in my previous letter (*Journal*, August 1990, **157**, 296), an anthropologist is neutral as to whether or not people die of smallpox. His or her professional concern is with the local meaning of rituals and their value to the society that practices them. By contrast, the primary concern of the doctor is the prevention of disease and death, and he or she needs to determine whether a practice/ritual is effective in this respect. This by no means always involves 'putting down' non-Western practices. For over 2000 years Ayurvedic practitioners in India were using an effective antipsychotic agent, Rauwolfia, while doctors in the West were beating psychotic patients, chaining them and, even into the present century, immersing them in cold water. The meaning of these practices/rituals would be of interest to a social anthropologist, whereas the primary concern of a psychiatrist is that these ineffective methods of treatment have been replaced by effective ones, initially Rauwolfia.

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Ethnic minorities and the psychiatric system

SIR: Although we are somewhat uncertain how to acknowledge Littlewood's assessment of our paper (*Journal*, March 1990, **156**, 373–378) as "not unuseful", we are grateful to him for his less ambiguous critical comments (*Journal*, September 1990, **157**, 451–452). We agree that our study does not address the question of subtle or unconscious racism, but this was not our purpose. One of the starting points for our investigation of police admissions to psychiatric hospitals was to find out how much agreement there was between the policeman and psychiatrist in diagnosing mental illness in emergency referrals. Our results revealed a very high degree of concordance. One of the reasons that we undertook this investigation was in response to an assertion, supported by Dr Littlewood, that "the police are *overtly* racist and selectively pick out non-mentally ill black people in the streets and take them to a psychiatric hospital under Section 136 of the Mental Health Act as an alternative to arrest" (Littlewood, 1986). We believe that our study, carried out in an area with one of the highest Section 136 admission rates in the UK, challenges this assertion, and raises the concern that *overt* racism is more likely to result in mentally ill subjects from the ethnic minorities being channelled into the criminal justice system instead of the psychiatric services.

We appreciate the value of case vignette studies of the type advocated by Dr Littlewood and used in the study by Lewis *et al* (*Journal*, March 1990, **157**, 410–415). However, we are concerned that such an enlightened approach should not preclude descriptive studies which detail the progress of the ethnic minorities through the psychiatric services and the criminal justice system. It could even be argued that the results of such an approach (e.g. Harrison *et al*, 1988), which undoubtedly appear naive to the anthropologist, have brought transcultural psychiatry research to life in this country.

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Benzodiazepine withdrawal

SIR: We thank Hawley for his kind comments on our paper (*Journal*, November 1990, **157**, 777–778) and welcome the opportunity of replying.