

AUTHORS' REPLY: We are grateful to Dr Shah for his remarks about our paper. With regard to statistical tests employed, the χ^2 test was the appropriate statistic as the sample size was greater than 20. A Yates correction to the χ^2 is the correct statistic when the expected frequency of at least one of the cells is less than 5. There is disagreement among statisticians as to whether such a correction is necessary (Everitt, 1977), and on statistical advice we did not apply such a correction. However, even after applying such a correction, the results remain significant for the association between left basal ganglia calcification and delusions, and just fail to reach significance ($P < 0.06$) for the right side. We admitted in the discussion that the number of patients involved is small, but this makes the trend of the association between delusions and basal ganglia calcification even more interesting.

With regard to Dr Shah's second point, his own unpublished figures are not comparable with ours since they are based on a hospital sample whereas our study included patients in a variety of settings. We agree fully that adequate drug trials, possibly using medication such as carbamazepine, are important for the treatment of aggression – a very troublesome and disruptive behaviour in Alzheimer's disease.

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EVERITT, B. (1977) *The Analysis of Contingency Tables*. London: Chapman & Hall.

Oestrogen therapy for the menopause

SIR: Ballinger in her paper on psychiatric aspects of the menopause (*Journal*, June 1990, 156, 773–787) comes to a clear conclusion that “anxiety and depression in such women do not respond to oestrogen therapy although some cases respond to antidepressants”. Few endocrinologists and gynaecologists with a large experience in treating such women will agree with this dismissal of the value of oestrogens and we are sadly aware that there is virtually no meeting point between our specialties on this issue. This review article, quite devoid of objectivity and omitting most relevant publications during the last ten years, will reinforce traditional psychiatric prejudice on this subject.

“The gynaecological view” described by Dr Ballinger is a travesty which gives a biased view of what gynaecologists are meant to think. As one of us (JS) is singled out for particular scorn for “uncontrolled” statements, may we be permitted to bring to the attention of your readership some of the controlled studies of the value of oestrogens performed in this department and elsewhere that have been omitted by your author.

We would all agree that there is an excess of depression in women compared with men and that this excess begins from puberty and occurs more commonly at times of hormonal flux. This appears as pre-menstrual depression, post-natal depression and climacteric depression. Certainly these may also be times of greater stress and we accept much of the psychiatric emphasis on the environmental contribution to these peaks of psychiatric pathology but not to the exclusion of a hormonal component.

Dr Ballinger uses the term ‘menopause’ to describe the years around the time of cessation of periods. Although this is more precisely called the climacteric, it is acceptable in this context because of common usage and because, as she has stated, the literature suggests that any alleged increase in depression in these women occurs in the few years before the menopause. There is no clear evidence of an increased incidence of depression in women who are truly post-menopausal.

Depression in the 40-year-old woman with regular menstruation is frequently cyclical with attendant cyclical symptoms of loss of energy, loss of libido, bloated feelings, mastalgia and menstrual headaches. This combination of debilitating symptoms, best called the ‘ovarian cycle syndrome’, is common and gets worse with age until it becomes indistinguishable from the most distressing symptoms of the climacteric. The symptoms frequently lose their cyclicity, becoming more constant, but the similarity to those of the climacteric supports the view that the psychiatric symptoms of the latter are a continuum of those of the pre-menstrual syndrome. Oestrogens are effective in treating both conditions. The problem is that depression (although it may be cyclical) in the presence of regular periods is not considered by psychiatrists to be responsive to oestrogens.

Montgomery *et al* (1987) in a placebo-controlled study reported the beneficial effect of oestradiol implants on depression in a group of women attending our menopause clinic. However, this improvement in depression and anxiety was only seen in the pre-menopausal patients and not in those in whom periods had ceased. Data yet to be published show that this significant improvement in scores on the self-rating scale of distress (SRD-30) persists over

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