evident in this paper due to the fact that all ratings were made only by the subjects themselves.

As the laterality of foci is not stated it is difficult to see how the findings can be regarded as support for an association between psychopathology and a left temporal lobe focus, though this is stated in the discussion.

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DEAR SIR,

We would like to reply briefly to Dr Roberts' 3 basic points. Firstly, complex seizures were a feature in all our patients with focal epilepsy, although, as we state in our paper, not all seizures were of temporal origin. Unlike Dr Roberts, we do not think that the available evidence, including the two review papers that he cites (Stevens, 1975; Stevens and Hermann, 1981), suggest significant differences in interictal psychopathology between patients with temporal and non-temporal seizures, if other factors, especially major cerebral pathology are excluded. Additional generalized seizures can be an aggravating factor, but as these were present in only 6 of the 47 patients with focal epilepsy, it is unlikely that they could have substantially affected the mean score of this subgroup of patients. Secondly, the study by Bear and Fedio (1977) is an interesting one, but based on a rather small number of patients (a total of 27) who were furthermore selected without regard to their psychiatric history—always a potential major complicating factor. Their finding of differences in psychopathology and response-style in temporal lobe epileptics related to laterality of the focus remains unconfirmed. Thirdly, we nowhere claim to have assessed laterality effects. The sentence in the discussion concerning the effect of a left temporal lobe focus merely refers to an additional finding in the study by Stores (1978), which we cited.

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AMITRIPTYLINE FOR DEPRESSED WOMEN WITH YOUNG CHILDREN IN GENERAL PRACTICE

DEAR SIR,

We feel that your readers would be interested to hear the results of a small controlled trial of amitriptyline in women who were identified as suffering from minor psychiatric illness in a general practice survey carried out in Harrogate, North Yorkshire. Symptoms of depression and anxiety which commonly affect women in the general population are related to adverse social factors (Moss and Plewis, 1977; Brown and Harris, 1978; Richman, 1978). Perhaps this is why it is often assumed that counselling of some sort is the most appropriate form of management in this sort of disturbance. However, we have some evidence that amitriptyline can reduce depressive symptoms under these circumstances and that improvement is still present after a year.

All the patients on the general practice list of one of us (J.H.) who were women with children aged two to 15 were approached and asked to complete the Leeds Scales (Snaith *et al,* 1976; Forrest and Berg, 1982) with a view to identifying those who had minor psychiatric illness. About 80 per cent responded. High scorers (scores of 7 or more) were interviewed a few weeks later and were offered treatment in a double-blind, randomly-allocated, placebo-controlled trial of a slow release amitriptyline preparation if the family doctor considered them sufficiently disturbed and there was no likelihood of pregnancy or severe physical illness. Twenty-five women, about a third of those interviewed, were included and successfully completed the trial. Progress was monitored using the Leeds Scales. The doctor made his own rating of symptoms and did not know what the questionnaire scores were. In retrospect it was found that his assessments of depression, apathy and diurnal vartiation of symptoms were significantly associated with Leeds D Scale scores ($P < .05$). The active drug group received 25 mg of amitriptyline for a week and then 50 mg in one evening dose. Blood levels of drug were estimated at four and

eight weeks after starting the trial. The means of two levels were: amitriptyline 18, 27, 18, 64, 35 and 30 respectively and nortriptyline 36, 28, 25, 35, 73 and 31 respectively in six of the cases. Two others who also had these estimations carried out produced positive results in only the first of the two tests: amitriptyline levels 13 and 7, nortriptyline levels 4 and 25. There was thus satisfactory compliance in three quarters of the women who had blood tests carried out.

The mean age of the women in the trial was 32.4 (SD = 6.7, range 20 to 46) without any significant difference between drug and placebo groups. On the first visit to the family doctor, the mean Leeds Scale D scores were: active drug group 6.3, SD = 3.5, n = 13, placebo group 4.2, SD = 2.5, n = 12. There was no significant difference. Two weeks later there was little change: active drug 5.7, SD = 4.4, n = 12; placebo group 4.5, SD = 2.6, n = 12. Mean scores of the active drug group continued to fall over the three months on medication and was 3.9, SD = 3.2, n = 12 after a year when the women were interviewed. This fall of 2.4 from initial visit to follow up was significant (t corr = 2.7, $P < .05$). Over the same period mean scores of the placebo group rose 1.1 which was not significant (t corr = 1.4). Comparison between active drug and placebo groups on the disparities between initial and review scores after one year, using analysis of variance showed significant differences ($P < .01$) between the groups.

Our survey, which has not yet been published, was comparable to other similar investigations in that about a third of women studied were identified as possibly disturbed, and adverse social factors were related to symptoms of anxiety and depression. An attempt was made to treat 30 of the disturbed women who could not be included in the trial, mainly because of possible pregnancy, by regular health visitor sessions, carefully planned and supervised by one of us (A.B.). Only eight of them accepted this approach. We believe that symptoms of depression affecting women with young children in the community should be treated initially with amitriptyline when there are no contraindications. This would seem to be acceptable to many of them and in keeping with the family doctors' usual way of dealing with troublesome symptoms.

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EEG MONITORING OF ECT

DEAR SIR,

There are several issues that must be raised regarding the recent article by Christensen and Koldbaek (*Journal*, July 1982, **141**, 19–23) describing electroencephalographic (EEG) monitoring of electroconvulsive therapy (ECT) using the MECTA instrument. We have had the opportunity to study 19-channel EEG tracings recorded during ECT on a Grass 8-18C EEG instrument using the International (10–20) system for recording electrode placement plus naso-pharyngeal leads (Staton *et al*, 1980; Staton *et al*, 1981; Brumback and Staton, 1982; Gerst *et al*, 1982). We have compared standard EEG tracings with simultaneous recordings produced on the MECTA instrument. We found that the MECTA recording of brain activity from bifrontal electrodes did not correlate with standard EEG tracings. Frontal electrodes are the most susceptible to muscle artefacts (from the frontalis muscle) and eye movement artefacts. What Christensen and Koldbaek labeled as a "supra-convulsion" (their Fig 1D) is the typical pattern of frontalis muscle artefact and their "threshold pattern" (their Fig 1A) is similar to eye movement artefact. The MECTA instrument displays a bipolar recording from the bifrontal electrodes. Bipolar recording measures only the *difference* in potential between electrodes. Since the bitemporal stimulus from the MECTA instrument produces a bilaterally symmetrical electrical seizure, there is little or no