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PLACEBO-CONTROLLED STUDIES OF ECT

DEAR SIR,

In his discussion of placebo-controlled studies of electroconvulsive therapy Professor Kendell (*Journal*, October 1981, **139**, 265–83) referred to the six studies reviewed by Barton (1977) and adds to this number the four double-blind studies published over the past four years.

In a recent review of placebo-controlled studies of ECT (Mendelson, 1981) I discussed two other studies which were only mentioned in passing by Barton and omitted by Kendell, namely the reports of Sainz (1959) and of Fahy *et al* (1963). Sainz reported on 20 patients with depressive illness, of which ten were treated with ECT while the remainder received 'mock' treatment. He found that in the electrotherapy group nine patients recovered and one improved; in the placebo group one patient improved, six were unchanged, and three became worse. Fahy and his colleagues compared groups of depressed patients treated with imipramine, electrotherapy, and 'thiopentone sleep'; there were 17 patients completing treatment in each of the three groups. Although ECT was more effective than 'thiopentone sleep', this difference did not reach statistical significance. This study was perhaps biased against ECT in that severely depressed patients who were considered high suicide risks were excluded from the trial, and it is a widely held clinical belief that these patients show the most striking response to electrotherapy.

Professor Kendell's comment about "conflicting results of recent comparisons of the effect of real and simulated ECT in the treatment of depressive illness" is ill-founded. Three of the four recent studies have shown ECT to be clearly superior, whereas the study by Lambourn and Gill (1978) used brief pulse stimuli, applied unilaterally with the rating of improvement made on the day following the last treatment, although it has been shown that unilateral ECT has a poorer therapeutic effect within the first week of therapy when compared with bilateral ECT (Heshe *et al*, 1978).

I would suggest that the results of placebo-controlled studies of ECT pose no serious challenge to the accepted clinical view that electrotherapy is a specific and effective treatment of depressive illness in the presence of indications as discussed by Kendell.

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BENZODIAZEPINES AND EFFECTIVENESS OF ECT

DEAR SIR,

I have read with much interest the magisterial review article "The Present Status of Electroconvulsive Therapy" by R. E. Kendell, (*Journal*, October 1981, **139**, 265–83).

I agree with him about the continued utility of ECT in psychiatric treatment and the necessity for accurate and conscientious routines in its use. I also agree with him about the necessity for further research in this form of treatment, still the most effective in some types of depressive states.

Professor Kendell seems quite concerned about the results of recent comparisons of the effect of real and simulated ECT in the treatment of depressive illness, particularly the results of the Northwick Park trial (Johnstone *et al*, 1980). This study seriously questions the generally assumed efficacy of ECT as anti-depressant method. However, when reading the paper by Johnstone *et al*, one finds on page 1318 the following: "Of the 62 patients who finished the course, 18 (8 on real ECT and 10 on simulated) were given benzodiazepines, mainly either as diazepam 5 mg regularly twice daily or as diazepam 10 mg in occasional doses to relieve distress. Improvement scores were similar in patients with and without diazepam. The only other psychotropic medication was a benzodiazepine hypnotic prescribed for all patients".

Clinical ECT experience and some research data indicate that benzodiazepines can be an effective means of decreasing the efficacy of ECT. In a retrospective study (Sand Strömgren *et al*, 1980), it was found that ECT-treated depressive patients, who received benzodiazepine, showed shorter seizure duration and a need for a significantly greater number of treatments.

Johnstone *et al* write that improvement scores were similar in patients with and without diazepines. It is however unclear what they mean. Benzodiazepine hypnotic was prescribed for all patients.

The administration of benzodiazepines to ECT-treated patients in the Northwick Park trial does not logically allow any conclusion about the efficacy of real ECT compared to simulated ECT.

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PROPRANOLOL IN SCHIZOPHRENIA

DEAR SIR,

Dr Carr (*Journal*, November 1981, **139**, 47) makes a relevant point in his criticism of our trial of propranolol in schizophrenia (*Journal*, August 1981, **139**, 105–11). It would be surprising if chronic schizophrenic in-patients showed a clear response to a new drug within three months. However, this was precisely the claim which was being made for propranolol by earlier workers, and our trial was specifically designed to test this claim, with a negative result. The lack of response to chlorpromazine was not surprising, as these patients were still in hospital with schizophrenic symptoms in spite of previous treatment with neuroleptics. Chlorpromazine is normally an effective treatment for schizophrenia, and, therefore, the term 'chlorpromazine-resistant' is appropriate for a group of schizophrenic patients who fail to respond to chlorpromazine. It is, of course, possible that our patients were also 'propranolol-resistant' and that propranolol might be effective in a different patient group, but our reasons for believing that we chose an appropriate group of patients for this trial are detailed in the discussion of our paper, to which Dr Carr does scant justice by quoting half-sentences out of context. Dr Carr appears to suggest that propranolol may be effective in the long-term maintenance treatment of chronic schizophrenic patients. This is not the principal claim which has previously been made for propranolol, and an entirely different trial design would be required to investigate it.

A number of psychiatrists have been encouraged by enthusiastic claims for propranolol to 'give it a try' in their chronic drug-resistant patients. They should appreciate that nobody has yet succeeded in demonstarting that propranolol as a sole agent is more effective than a placebo in the treatment of schizophrenia. On the other hand, there is good evidence that propranolol enhances the efficacy of neuroleptic drugs. The pharmacokinetic interaction which we demonstrated between propranolol and chlorpromazine, which leads to increased plasma levels of chlorpromazine, is sufficient to explain this effect which could probably be parallelled by using very large doses of neuroleptics rather than by co-prescribing propranolol. In my view the use of megadose propranolol in the treatment of schizophrenia is unjustified outside a research setting, in view of the lack of evidence of efficacy and the known risks of cardiovascular complications with such high doses.

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SEASONALITY OF SCHIZOPHRENIC BIRTHS: HARMFUL EFFECTS OR GENETIC MORPHISM?

DEAR SIR,

That schizophrenics tend to be born in the colder months of the year has been clearly established (Torrey, 1980). There are negative correlations