The problem in testing his assertion that the response of delusions to ECT is longer-lasting than that of other features of depression is that the Hamilton scale which was used in our study (and most other recent studies) of ECT does not include an assessment of delusions. However, information on the time course of the effects of real vs. simulated ECT in patients with delusions is available in Tables II and III of our paper. The differences between the groups which are present at the end of the 4-week trial period are substantially diminished one month later and absent at 6 months. We find it difficult to see how conclusions other than those which we have drawn concerning the magnitude and duration of the effects of ECT, whether in patients with delusions or the sample as a whole, can be derived from these findings.

2. We have described (*Journal,* March 1984, p.228, column 2, para. 2) how the delusion score we used was derived from the P.S.E. ratings. Degree of conviction was assessed on the standard 0, 1, 2 scale. The reliabilities of P.S.E. ratings in general have been examined (Wing, Cooper, Sartorius 1974). No items in the P.S.E. are specifically concerned with "ability to verbalise illogical ideas, garrulousness, and an underlying predisposition to be convinced by one's own automatic thoughts" which Dr. Weeks supposes have contaminated our delusion score. We doubt whether verbal facility per se biasses the assessment of delusions when the P.S.E. is used in the recommended manner.

3. We remind readers who are interested in Dr. Weeks' suggestions concerning the nature of our trial sample that the patients were selected by (i) the Newcastle criteria for both endogenous depression and responsiveness to ECT, (ii) the criteria for depressive illness used in the MRC (1965) trial and (iii) the Feighner (St. Louis) criteria for primary depressive illness, and invite them to re-examine Table IV of our recent paper. This shows the mean score of our trial sample in relation to the predictive scales of Hobson, Roberts, Carney, Mendels, Kendell and Hamilton. We particularly draw attention to the findings in relation to the series of patients studied by Kendell at the Maudsley Hospital. On the scale he devised a higher score was associated with better response to ECT. In his sample those with a diagnosis of neurotic depression (ICD 314) had a mean score of −1.9, with manic-depressive illness depressed type (ICD 301) 9.5, and melancholic illness (ICD 302) 9.7. The mean score of our sample was 15.2 with a standard deviation of 10.9. We doubt Dr. Weeks' assertion that the spread of real and severe affective disorder across the country departs significantly from a random one but this observation (together with those of Table IV in our paper) does not support the view that the patient sample of the Northwick Park trial was less severely depressed than those treated with ECT elsewhere.

4. We view Dr. Weeks' assertion that "the entire Northwick Park ECT trial sample was atypical" as a personal conviction unrelated to the evidence we have presented and one which is appropriately placed in the context of his subsequent unreferenced declaration of faith ("I believe it undoubtedly was"). Clinical investigators can do no better than to present the findings from their studies in terms of the assessment and rating scales which are available and in as full and accurate a way as possible. This we have attempted to do.

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SPONTANEOUS SECOND FIT AFTER ECT

Dear Sir,

We would like to invite comment from your readers upon an unusual clinical phenomenon in the course of the treatment of a 17 year old girl suffering from a severe psychotic depressive illness. The girl became progressively depressed and deluded over a period of three to four months and eventually refused to eat or drink. She was unable to give informed consent and she was, therefore, treated with ECT twice weekly under Section 58 of the Mental Health Act, 1983. After the second treatment (right unilateral ECT Duo-plus mark 4 machine setting 2) the patient had a second bilateral tonic clonic fit with a divergent squint to the left lasting for one minute and occurring one minute after the electrically induced fit ceased. The patient made an uneventful recovery and she had no further episodes during her subsequent six treatments. The anaesthetic procedure was unremarkable (suxamethonium 50 mgs; Brietal 50 mgs; full preoxygenation). At the time she was on no medication and there was no history of cerebral insult or epilepsy. Neurological examination and EEG were normal and a CT Scan was reported as showing normal ventricles with an enlarged superior cerebellar cistern and cistern of the velum interpositum, but otherwise normal.

Recent evidence (Christensen & Koldbaek, 1982) with EEG monitored ECT has shown that disturbed electrical activity continues after the visible tonic clonic phase ceases. Blumenthal (1955) reports in a series of 51 cases the occurrence of spontaneous fits within a few hours of administering ECT. The questions that concern us are how unusual is it for a second fit to occur within a minute of ECT and whether this can be explained in terms of a spontaneous second fit or continuation of the original electrically induced

fit with an unusual pattern or rate of spread of electrical activity within the cortex.

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RATING TARDIVE DYSKINESIA

DEAR SIR,

The paper by Bergen *et al* (*Journal*, May 1984, **144**, 498–502) found marked intra-rater variability in the assessment of tardive dyskinesia in four patients, using the Abnormal Involuntary Movements Scale (AIMS). This finding casts further doubt on the usefulness of this, and similar scales, for the measurement of tardive dyskinesia. the AIMS was originally developed to quantify the severity of spontaneous, involuntary movements, and consists of a check-list of movements at various body sites. Brief descriptions of the movements to be rated are provided, but these are too limited to allow clear distinction between abnormal (dyskinetic) movements and normal, fidgety movements, the motor phenomena of akathisia, tics, and schizophrenic stereotypies and mannerisms. No criteria for severity are provided, and, as Bergen *et al* comment, the rater arrives at a global judgement of severity based on the character, amplitude and frequency of a movement. Although satisfactory inter-rater reliability has been demonstrated (Smith *et al*, 1979a, b), whether AIMS scores are a valid reflection of tardive dyskinesia has not been demonstrated. In research, the use of the scale as a diagnostic instrument, with arbitrary cutoff scores, can yield a patient sample that is markedly heterogeneous with regard to type and distribution of movements.

Bergen and his colleagues stated that they were not aware of any other study which had presented re-rating data. This may be true for the AIMS, but my own study (Barnes & Trauer, 1982) analysed ratings on 94 patients, by six raters using a videotape assessment procedure, and found high inter-rater and re-rating reliability. Like the AIMS, this scale involves the rating of movement at a number of body sites, and it suffers from some of the same drawbacks. However, the measure of severity was specified as the proportion of time a movement was present while the patient was being observed, a criterion that seemed a reasonable reflection of clinical morbidity. Validity was tested by comparison of the ratings with the diagnostic opinion of independent, experienced clinicians. Ratings for oro-facial movements showed close agreement with the clinicians' evaluation of the severity of tardive dyskinesia, and a diagnostic threshold score was set for ratings of oro-facial movement. Ratings for trunk and limb movements did not show good agreement with the clinicians' diagnostic assessment. For such movements, simple, quantitative measures of frequency, amplitude or duration of movement are inadequate, as they fail to discriminate between tardive dyskinesia of the trunk and limbs and the variety of other movements of the extremities that may be present in psychotic patients on long-term treatment with antipsychotic drugs.

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IN THE BEGINNING . . .

DEAR SIR,

I was interested to read Philippe Ployé's article (*Journal*, July 1984, **145**, 55–8) on the possible antecedents of projective identification and idealisation in the last three months of pregnancy.

Why three months? Are not the basic elements of the personality present from conception? The pointed, motile, energetic sperm and the round, passive, receptive egg form the creative zygote which has a desire to reproduce itself, to put out, to take in, a need to relate and a drive to grow, to mature and become a person.

May I ask another question? What might be the psychological sequelae of freezing embryos? Would we predict a deep seated fear of stagnant dependent immortality, alongside the fear of death?

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CORRECTION

Please note the following is the correct key to Figure 3 in the paper by David Sturgeon *et al* published in the *Journal*, July 1984, **145**, 62–9.

High EE experimental (n = 9)
High EE control (n = 4)
Low EE experimental (n = 4)
Low EE control (n = 2)