I take the last point concerning the age-distribution of the patient groups in the study by Dr. Griffiths and myself. Gregory (1958) pointed out that because of improved mortality rates younger individuals nowadays are much less liable to experience the death of a parent. In our series there may be a slight tendency to underestimate the significance of parent-loss among the schizophrenics, who are probably younger on average than the control individuals. This would not apply to the affective disorders, in which the age distribution would be relatively similar to that of the controls.

This field is bedevilled by conflicting results, failure to make adequate definitions, and a tendency to rush into hasty conclusions, of which we are all guilty. Many of our difficulties are semantic, and I regret that, in my opinion, Dr. Birtchnell's letter has increased rather than decreased such difficulties.

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UNILATERAL AND BILATERAL ECT

DEAR SIR,

My apologies to Drs. Sutherland *et al.*, for my inexcusable error in reading their paper (*Journal*, September 1969, pages 1059 to 1064). Unfortunately their letter (*Journal*, January 1970, p. 126) does not answer the points which I raised. Perhaps I could elaborate upon these.

1. One cannot be satisfied that they were in a position to make any statements about the relief of depression, since this was not assessed in their trial. The number of ECTs given is surely not a reliable indication of response to treatment, particularly as several different psychiatrists were involved in deciding what this would be for any particular patient. We all differ in our ways of deciding when a patient has had enough ECT and what constitutes 'an adequate course of treatment'. A therapeutic trial should attempt to minimize this personal and idiosyncratic judgement.

2. They do not tell us how double-blind assessments of such variables as 'time taken to breathe spontaneously' were made. I take this to mean that the observer was not in the room at the time when the shock was given, and that he was informed of the exact time when this had occurred. Since the time intervals involved were relatively short, fairly elaborate arrangements would be needed to avoid any bias on the part of the person administering treatment. One can think of various ways in which this could be done, but the paper does not describe the method adopted. It is also extremely difficult to make a very definite decision about the beginning of spontaneous respiration, since many patients start off with small and almost imperceptible inspirations.

3. I wonder what led the writers to conclude that the EEG assessor was able to guess correctly the method of treatment any more frequently than would be accounted for by chance? Table III shows that the allocation was correct in only 10 of 19 bilateral cases and 11 out of 18 unilateral non-dominant cases. Admittedly the assessor did rather better on the dominant cases (14 out of 22), but I find it difficult to see how these figures could yield a value of $p = \cdot 00003$. Could the writer enlighten us on the statistical procedure employed?

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AMPHETAMINE TAKING AMONG YOUNG OFFENDERS

DEAR SIR,

We were interested to read Drs. Cockett and Marks' article (*Journal*, October 1969, pp. 1203–4). Our interest in this subject was also aroused by Scott and Wilcox' study (1965), and for the past twelve months we have been screening the urine of all boys aged 14–16 admitted to Rose Hill Remand Home, Manchester. Rose Hill receives boys mainly from the Cities and County Boroughs in Lancashire, including Manchester, Salford, Bury, Bolton, Blackburn, Oldham, Preston and Warrington. Many of these places have the sort of clubs which are associated with drug-taking.

Method. Urine was collected from each boy as soon as possible after admission to the remand home. Younger boys in whom drug taking was suspected were also tested. Samples were screened by the method of Mellon and Stiven (1967). Those showing spots in the area Rf 0·70–0·95 were further investigated, in duplicate, by the method of Beckett *et al.* (1967), one extract being run in butanol/acetic acid/water (5 : 4 : 1), the other in isopropanol/5 per cent ammonia (10 : 1). Spots were developed with 0·5 per cent methanolic bromo-cresol green. Coincidence of spots on each system with those of control urines con-

taining dexamphetamine and methylamphetamine were presumed positive. In all 55 'screened' specimens were further investigated by the method of Beckett *et al.*, of which 13 were 'confirmed'. Control experiments showed the sensitivity of the Beckett method to be twice as great as that of Mellon and Stiven. It was considered reasonable, therefore, to regard as 'positive' only those samples confirmed by the Beckett system.

Results: 640 specimens were tested in the 52 weeks from October 1968 to September 1969, inclusive. The results, according to age, are shown in the table below:

Age (yrs.) ..	13	14	15	16	Totals
No. of tests ..	2	202	179	257	640
No. of positives	0	0	3	10	13
% positive ..	0	0	1·7	3·9	2·0

These results would appear to show that drug abuse in the North West is no significant problem under the age of 15 years, and the incidence is lower than in the London area.

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CONTROLLED EVALUATION OF CHLORDIAZEPOXIDE

Dear Sir,

In the paper by Kelly *et al.* (*Journal*, December 1969), p. 1387–92) the last sentence of the summary states: 'The Clyde Mood Scale and Semantic Differential are valuable for quantifying subjective changes, and deserve wider use.'

This sentence is rather vague and uninformative, and contrasts strongly with the fact that in Table I of this paper (p. 1398) the data are given for 6 scales of the Clyde Mood Scale, and only one shows significant differences between drug and placebo. Of the 7 variables of the Semantic Differential Scale none shows significant difference.

It is unfortunate but true that all too often the summary of a paper does not accurately reflect the contents. It would be helpful to readers and to those looking up references if such anomalies could be eliminated.

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MENTAL RETARDATION

Dear Sir,

May I comment upon Dr. Spencer's letter in the *Journal*, January 1970, p. 127.

I agree with Dr. Spencer that it is essential that there should be a consistent international nomenclature. Unfortunately, the term 'mental retardation' which has been adopted by the W.H.O. classification, is a bad one. This is because in clinical practice it is used to describe cases where bad environmental conditions have produced a retardation of development, in patients who have normal potential. I share Dr. Spencer's dislike of the term 'subnormality' both because it is a confusing term, as he points out, and also because it is inaccurate, as the majority of cases are abnormal, rather than subnormal.

It is a great pity that the term 'mental deficiency' was discarded, especially as this was done, not for scientific, but for emotional reasons. I feel that there is quite a case for urging the W.H.O. to go back to it, particularly since, as Dr. Spencer points out, it is still used in Scotland.

If, however, people are determined to have a new term, may I suggest that 'mental handicap' is one which is most acceptable, as it cannot be confused with clinical terminology and descriptions.

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Dear Sir,

I am glad to have Dr. D. A. Spencer's support in the campaign to introduce the term 'mental retardation'. I suggested this in the correspondence columns of the *British Medical Journal* on 9 November 1963 and again on 6 September 1969, pointing out that the use of the term 'subnormal' conveyed abuse, degradation, hopelessness, inaccuracy and confusion. I got little support for my first letter, and apparently the Department of Health and Social Security now prefers to