withdrawal of lithium with intensive supervision during the period of reduction and discontinuation may be a more adequate approach to preventing relapses.

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LITHIUM THERAPY AND THE RISK FOR LEUKEMIA

DEAR SIR,

There are now several case reports raising the question of a relationship between lithium and leukemia, Orr and McKernan (1979), Hammond and Appelbaum (1980), Nielsen (1980). Levitt and Quesenberry (1980) have shown that lithium *in vivo* increases granulocyte production, and this mechanism might explain the mild leukocytosis noted in many patients on lithium maintenance therapy (Gallagher and Gleaves, 1979).

One hypothesis is that chronic marrow stimulation by lithium may induce leukemia, especially of the myelogenous subtype. In order to examine this question we decided to search for any overlap in the diagnoses of bipolar affective disorder and leukemia among patients in our institution. Over the past 10 years our hospital has treated 710 in-patients with bipolar affective disorder and 571 patients with leukemia. Data obtained from the State Tumor Registry indicates that our hospital treats approximately half of all leukemia patients in Iowa. Our experience is that patients treated in this hospital for a major medical or psychiatric problem usually receive treatment here for other illnesses that develop. Our anticipated incidence of leukemia in the bipolar sample was 1–2 cases (Gallagher and Gleaves, 1979) and we therefore would expect to identify 0–1 case by our survey. Another assumption was that almost all of the bipolar patients would have been exposed to lithium at one time or another, and many would be on chronic therapy. Support for the hypothesis would come by finding a significantly increased incidence of leukemia in the lithium-treated patients.

We found no cases of leukemia in the group of bipolar patients. This finding speaks against an association of lithium treatment with leukemia. However, a well-designed, prospective study that specifically follows up on each subject would put the hypothesis to its proper test.

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IDENTIFICATION OF DISEASE ENTITIES

DEAR SIR,

We have been surprised at the lack of correspondence relating to Kendell and Brockington's paper on the identification of disease entities (*Journal*, 1980, **137**, 324–31). Apart from one letter which is critical on broadly philosophical issues, there has been little discussion of the statistical basis of the proposed technique, which, since the authors invite its use by others, has considerable importance.

It seems to us that the authors oversimplify by implication the interpretation which would be made had even a clear nonlinearity been observed. The situation is complicated by the fact that the scales used in psychiatric research are seldom natural interval scales analogous to those found in the physical sciences (e.g. weight). The intervals between points on the scales cannot be guaranteed to be of equal size—any given interval can be arbitrarily stretched or squashed, making a nonsense of any assessment of linearity of the resulting plot. Admittedly some of the scales referred to are weighted averages (the discriminant functions) and the averaging process

will tend to alleviate the difficulty. On the other hand, one of the scales (the first outcome scale: time in hospital) is a proportion—which saturates at 0 and 1, producing a characteristic S-shaped curve!

In any case, even if one was happy about the interval nature of the scales it is improbable that, *a priori*, the relationship between symptomatology and outcome would be linear. As Guttman puts it: "In the social sciences, at least, linearity should be regarded as a departure from non-linearity and not vice-versa".

In view of these difficulties we would like to suggest that even if the method gave clear indications of a non-linear relationship this should at best be regarded as merely indicative and in no way conclusive.

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INTER-RATER RELIABILITY IN MULTI-CENTRE TRIALS

DEAR SIR,

In interpreting the results of the study by Fisch *et al* (*Journal*, February 1981, **138**, 100–9), it should be appreciated that they used a drastically reduced version of the Hamilton Depression Rating Scale (HDRS) of only eight items instead of the original eighteen. Furthermore, the HDRS is an instrument for rating change in depressive illness and is not designed for diagnostic procedures. Fisch *et al* used written descriptions of mythical cases for their physicians to rate, but video-tape interviews with patients may be a more useful procedure for increasing inter-rater reliability. Thus, in studies involving members of our group, Tiplady and Louden (1980) found that during video-tape sessions lasting one day, there was a significant improvement in inter-rater reliability ($P < 0.01$) from first to last rating.

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OILY INJECTIONS THAT OOZE

DEAR SIR,

Catherine Finlay Kinnes states (*Journal*, February 1981, **138**, 178) that she strongly suspects that leakage of fluphenazine decanoate from the site of injection is the explanation for the comment frequently heard in psychiatric clinics: "the injection never touched me", and she recommends "Z" injections to prevent leakage.

This possibility occurred to me and my associates several years ago. We selected three patients in whom oozing appeared to be substantial, and measured the amount of drug lost by absorbing it on to filter paper (*Lancet*, *i*, 364, 1976). Two patients were given 25 mg IM, and lost 0.4 mg (1.6 per cent) and 2.3 mg (9.2 per cent) respectively. The third patient was given 37.5 mg IM and lost 1.4 mg (3.7 per cent).

It seems that a small amount of oil, containing a clinically insignificant amount of drug, can appear to be a great deal. Using the "Z" technique may still be worthwhile in those patients who ooze, but I hope our data alleviate the concern that such losses are clinically important.

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ANOREXIA NERVOSA AND PSEUDO-ATROPHY OF THE BRAIN

DEAR SIR,

We wish to report a case of anorexia nervosa who was found to have the appearance of generalized cerebral and cerebellar atrophy on EMI scan: the 'atrophy' reverting to normal when the patient's general condition improved.

We were asked to see a 14-year-old prepubertal girl, who presented with severe weight loss, intolerance to cold, headaches and abdominal pain. Her weight on admission to the general paediatric medical ward was 10 kg below the 3rd centile and her height was above the 10th centile. Neurological examination revealed no abnormalities and examination of the fundi showed no optic atrophy. Investigations, including CXR, skull XR, again were normal.

No space occupying lesion was found in the EMI scan, though there were moderate degrees of generalized cerebral and cerebellar atrophy (see Figures 1 and 2). EEG done at the time shows occasional transient theta discharges occurring in both temporal regions.

When her weight rose above the 10th centile, 3 months after she was admitted to our unit, repeat EEG recording showed no definite abnormality. EMI scan showed that the atrophy had disappeared. These changes in the EMI scan have been reported by Heinz *et al* (1977) and Enzmann and Lane (1977).