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Correspondence

Letters for publication in the Correspondence columns should be addressed to:
The Editor, British Journal of Psychiatry, Chandos House, 2 Queen Anne Street, London, W1M 9LE

DEPRESSIVE ILLNESSES IN LATER LIFE

DEAR SIR,

We note that the frequency distribution of scores on the Newcastle Scale in the letter from Drs. Kendell and Post (*Journal*, May 1973, **122**, p. 615) agrees with previous work (Carney, Roth and Garside, 1965) in showing a cleft at +5 despite the investigators' presumably not being of the 'dichotomous' persuasion. It is not altogether surprising that the score distributions of the four smaller series of patients on which their larger distribution is based did not differ significantly from normal. The combination of small samples and large standard errors favours the null hypothesis (Costello, Bolton, Abra and Dunn, 1970). In minimizing the possibility of very real differences between groups of depressed patients thus shown up, Drs. Kendell and Post are reminiscent of the counsel who seeks to defend his client by discrediting the other side's witnesses.

There is evidence that neurotic and endogenous depressives do not differ with respect to severity of depression (Carney and Sheffield, 1972; 1973); and Eysenck has shown (1970) that the statistical evidence so far fails to support a unitary hypothesis but indicates separate dimensions of depression, one endogenous and another neurotic, these two axes not being logically compressible into a single continuum. Thus, all the arguments based on these single-axis frequency distributions of depressed patients' scores are probably fundamentally unsound. In addition, the shape of the curves derived from these scores will be largely determined by two other important considerations—the selection of the patients and the population sampled. Inclusion of patients with anxiety and other neurotic features will skew the curve and possibly introduce a third mode, as suggested by Roth and Garside (*Journal*, September 1973, p. 373), referring to Kendell and Post's distribution. The selection of patients depends very much upon the practices of individual clinicians. The population sampled may vary tremendously from one study to another, i.e. in-patient or out-patient, treatment with ECT or otherwise treated, hospital or general practice, etc. The only valid way of overcoming this difficulty is to sample the population at large—in the way that psychological tests are validated. We are not aware that this has yet been done.

However, these are largely theoretical considerations. We believe that the practising psychiatrist is interested in some independent validation of what may appear to be a rather sterile academic dispute, and in the implications of distinguishing between one kind of depressive and another for treatment and prognosis. We therefore decided to work backwards from outcome to diagnosis score, rather than the reverse as has hitherto been the case. We did a blind assessment of 165 depressed patients (Carney and Sheffield, 1972; 1973), rated on the Newcastle and Hamilton's Scales before ECT, on a four-point scale (A+B, socially recovered; C+D, incomplete social recovery), one month after the last electroplexy. There were 101 'good' (A+B) and 64 'poor' (C+D) outcome patients, the two groups not differing significantly on mean age (53·6, S.D. 16·1 and 51·4, S.D. 18·4 respectively), sex ratio and pre-ECT mean Hamilton's score (21·3, S.D. 6·2 and 21·8, S.D. 5·3 respectively). The 'good' outcome patients had a strikingly higher mean pre-ECT Newcastle score (7·5, S.D. 3·3) than the 'poor' outcome patients (4·4, S.D. 4·1) ($t = 4·35$; $P < 0·001$). In other words, social recovery and the absence of social recovery after ECT were clearly linked with pre-ECT Newcastle scores indicating endogenous-neurotic depression respectively. In the absence of a generally agreed physical basis for depression, responsiveness to treatment is probably the most useful independent criterion of depressive classification we have. Thus, making the distinction between endogenous and neurotic depression in terms of the Newcastle Scale was evidently a valid as well as a practically useful clinical exercise.

M. W. P. CARNEY.
B. F. SHEFFIELD.

Lancaster Moor Hospital,
Lancaster.

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PSYCHOTHERAPY GROUPS FOR COUPLES

DEAR SIR,

May I add a few observations on psychotherapy groups for couples to those expressed in Mr. Cochrane's interesting recent paper in your *Journal* (October 1973, 395), for it would be unfortunate to leave the impression that such groups are ineffective. My own experience with them has been extremely favourable, and as they are conducted in exactly the manner he finally advocates, his conclusions are fully supported from another point of view. Though I subscribe to most basic psychoanalytic *concepts*, the *techniques* appropriate for individual therapy, and even 'stranger' groups, are indeed quite inappropriate for 'natural' groups, whether families or couples. In natural groups the projections and 'parataxic distortions' which come to make up the 'transference' are already fully developed between the members, and clarification of these *where they are* not only avoids delay but ensures that working through continues actively between the sessions, no doubt one explanation for the surprising rapidity of change compared with other psychotherapy, using this technique. Transference does become evident where ambivalence cannot be contained within the marital system, and then of course it must be interpreted, but to seek to encourage it in relation to the therapist is pointless.

However, these marital tensions are extremely powerful and felt by the couples as highly dangerous if unleashed. Whatever else the therapist does he must provide very clear structure and boundaries, giving a sense of control and safety, if the couples are to venture outside the ambivalent bickering in which their hostility is normally bound. I think Mr. Cochrane is absolutely right to see the role of referee as an appropriate one. I (or my co-therapists) often have to shout, to bang the table, and to wave admonishing fingers when couples go too far into a destructive spiral. For one domineering woman, endlessly blocking her spouse and the group by demanding interpretations of why she had to talk too much, the most helpful comment I gave (for which she was later grateful) was, 'why not just *try* shutting up for a while?' Such control is valued by the couples, as by naughty children, as evidence of real care and concern for them, especially by those showing deficient inner controls and low ego strength, and capacity to provide it is rapidly internalized and taken over by the group members themselves. Passivity and neutrality by contrast is regarded as indifference and it is scarcely surprising if patients who observe it in the therapist fail to become involved with, and care for, each other. When adequate structure is provided, the couples are able to give up their ambivalent symbiosis, where clinging rage provides a compromise between excessive closeness (threatening loss of identity) and rejection (threatening abandonment).

This control can only be accepted, as Mr. Cochrane suggests, in a context of openness, warmth and support from the therapist(s). I find our marital groups enjoyable, often deeply moving and despite the explosive moments a psychoanalyst in the next room often complains that he cannot hear his analysand's associations because of the laughter.

Our experience also supports his suggestion regarding the value of co-therapy. Two therapists feel far more secure than one, enable support and control to be provided simultaneously, and offer a model of a relationship, as well as both sexual roles, for identification. After many years of work with professional co-therapists I now also work with my wife, which adds additional aspects to the model, and it is interesting that many family therapists in the United States have taken this step in recent years. However, co-therapy, like marriage, brings new problems and challenges as well as mutual help, and a careful choice of partner, as well as some period of work together is necessary if the collaboration is to bring more aid than difficulty to the situation.

The groups described are taken in private practice, where motivation and intelligence are likely to be higher than average, but have included a schizophrenic as well as several borderline characters and savagely destructive relationships. The most vicious interaction we have encountered, in a couple who had each received up to nine years of psychoanalysis from highly skilled practitioners, was satisfactorily resolved in fifteen months altogether of couple followed by couples-group therapy, and progress in general is a good deal more rapid that in the 'stranger' groups I take with similar patients.

At a teaching hospital where I supervise the marital and family therapy on the adult side, the trainees have not yet undertaken couples groups, but their work with couples of all social and personality levels, including psychotics, often shows surprisingly good results in cases intractable to other approaches. I find the main need is to help the trainees to over-