Brit. J. Psychiat. (1980), **136**, 408–412

Correspondence

DEPOT INJECTIONS FOR AFFECTIVE DISORDERS

Dear Sir,

I was interested to read the letter published in this *Journal* (January 1980, **136,** 105) from G. J. Naylor and C. R. Scott. I have been using fluphenazine and flupenthixol for manic-depressive disease for the last four years, having introduced them mainly because of the unreliability of patients with oral medication, and also because the drugs are major tranquillizers suitable for manic and hypomanic states. I have also been aware of lithium toxicity since I saw a case of status epilepticus with blood lithium within the therapeutic range. The relapse rate of most of my patients has decreased since the introduction of these two drugs.

I have also used the injectable form of flupenthixol for recurring depressive illness; 20 or 40 mg weekly or fortnightly, with good results. I wonder, therefore, if a university psychiatric department would be interested in studying the use of these drugs in affective disorders more systematically?

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ON MATCHING

Dear Sir,

I have read with interest the article 'Validity and Uses of a Screening Questionnaire (GHQ) in the Community' (*Journal*, May 1979, **134,** 508–15) in which "Each subject with a high GHQ score was matched with a low-score respondent, according to five criteria . . . The matched sample consisted of 118 persons. Refusals lowered this number to 105 (50 high and 55 low-scorers) left for the analysis". After the standardized interview was administered by psychiatrists to all participants, it was concluded that the difference did not reach 5 per cent level of significance for any of the six sociodemographic attributes observed (five criteria used for matching as well as education).

The conclusion may well be valid. However, the result might have been biased through taking into account five low-score respondents for whom matched pairmates were missing, and one has to assume that their presence did not confound the relationship and conceal a possible difference.

Therefore, whenever one uses the described procedure (i.e. individual matching) it is strongly advisable to base the analysis of data only on matched pairs. It is basically the technique used for case-control (retrospective) studies as described in some textbooks of epidemiology (MacMahon and Pugh, 1970; Mausner and Bahn, 1974).

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SOCIAL EFFECTS OF PERSONALITY

Dear Sir,

Huxley, Goldberg, Maguire and Kincey (*Journal*, December 1979, **135,** 535–43) demonstrate how poor the relationship is between the clinical features of minor psychiatric disturbances and prognosis. They have excluded major depressive disorders, but it is of interest that the 'second unrotated component' consists largely of items which are very common in depressive illness—lack of concentration, reported depression, fatigue, and somatic symptoms. It seems likely that this finding is an expression of the fact that depressive disorders do improve more than do personality problems.

The authors do not point out that many social factors may be an expression of personality. It seems to me naïve not to recognize this. Dissatisfaction with social contacts, dissatisfaction with leisure activities, housing, income inadequacy, extent of social contact—and many others which could have been chosen—are largely the result of having a certain type of personality. In any given street of identical houses, all occupied by people of the same social class, there will be some young men who cycle off to play football a mile away, and others who lack the drive to do so, and claim that the opportunities to

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play football locally are inadequate. It is the same for almost every other activity and interest that human beings are involved in.

To use the Eysenck Personality Inventory N scale and a modified California Q sort as the only assessments of personality is to miss most of the features of personality which distinguish those who cope well with life from those who do not. Unfortunately clinical assessments by experienced assessors do not produce information which can be easily dealt with statistically. This difficulty should not prevent us from viewing the interplay of personal and social factors realistically.

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CUSHING'S SYNDROME

DEAR SIR,

Dr Cohen's conclusion (*Journal*, February 1980, 13;, 120–24) is not justified. He writes "since the depressive symptoms are removed by removal of the adrenal glands, a substance produced by the adrenals must be responsible for them". But other major changes occur, including the psychological one that the patient is finally given the certain cure of his long distressing illness. Moreover, the patients do not all recover promptly: "twelve had bilateral adrenalectomy: in some of these the psychiatric symptoms began to abate within a few days, usually within a few weeks, but in some it was as long as a year before they had cleared completely". The factors must include hormones, possibly susceptibility to affective disorders as mentioned by the author, and how the situation seems to the patient.

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DEAR SIR,

Dr Cohen (*Journal*, 1980, **136**, 120–24) has presented an informative account of the psychiatric concomitants of Cushing's syndrome. However, his conclusions outreach his data on the question of the association between depression in patients with adrenal hyperplasia as opposed to those with tumours. He states categorically that if there are no psychiatric symptoms in a patient with Cushing's syndrome there is a three in four chance that they have a tumour. This is open to challenge on two counts. Firstly, it is not certain from Dr Cohen's account whether all of his psychiatric diagnoses were made blind to the results of endocrine investigation and surgery. Even the most careful and scrupulous investigator is not immune from observer bias and studies with a small sample size are particularly vulnerable to producing spurious results from this source. Secondly, the contingency table of patients with/without psychiatric symptoms X with/without tumour, contains two cells where the expected frequency is below five. Hence it is inappropriate to use the chi square test as he has done (Siegal, 1956). Re-analysing the table using Fisher's exact test gives a probability of obtaining these or more extreme results, of 0.0524 and therefore just fails to attain the conventional level of statistical significance.

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Reference

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MEDITATION

DEAR SIR,

Attempting to isolate transcendental meditation or any other form of meditation from its proper setting is to produce an artificial situation (see Review Article by M. West (*Journal*, November 1979, **135**, 457–67). One should note that meditation is to be practised in the context of a way of life extending far beyond the bounds of the 'sitting' period. Wherever it is practised there is likely to be some effect, whether attributable to placebo effect, relaxation or something else. What is important is to be wary of the effects, especially when meditating in isolation; in fact meditation alone is not likely to achieve very much, but it can lead to immense changes and it is the nature of such change with which we should be concerned. On the one hand it may be generally welcome, but on the other it could be catastrophic.

We in the West know next to nothing of meditation and related practices, and we are not likely to change this, except in a sadly naive way, by our objective attitudes. Our modern western mind is tuned in to a different wavelength from that of those who properly practise meditation. If we are seriously interested we should be prepared to undergo considerable changes ourselves which perhaps means leaving the security of the 'scientific-cum-logical' approach, with all its tenacious cultural accretions, if only for a while, as does the anthropologist who ventures out to live in a situation quite new to him. Meditation comes from

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