Brit. J. Psychiat. (1979), **135**, 481–487

Correspondence

ARE DISEASE ENTITIES IN THE MIND?

DEAR SIR,

With an increasing interest in the relevance of epistemology to psychiatry, we would like to comment on the debate between the Newcastle medical and London sociological classifiers of depression/anxiety.

Might not both sides gain by accepting that there are probably no natural taxa? Classes are for purposes. Work in Newcastle seems at least to have shown that we can predict the probable if temporary outcome of ECT. That does not in itself imply that groupings are the best for all treatments, not even that ECT is the 'best' treatment, nor that studies of aetiology will give the same clusters. The value of using factors other than mental state among the criteria for the purpose in hand is testable. Searching for *the* correct nosology, irrespective of its purpose or our motives, however, seems doomed to produce long statistical debates without rules to determine who has won them. There are not even in fact any 'non-existent clusters', just many useless and parochial ones, but utility depends on need, desire and purpose. Dimension versus category is also frequently a matter of choice; much depends on assumptions about linearity and about the use of arithmetic on the abscissa of graphs that can produce uni or bipolar curves.

The knower influences the known, especially in psychiatry. Perhaps we would be better advised to state our vision of what could be, as well as struggling to 'objectively classify disease entities'.

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DRUG OR MILIEU?

DEAR SIR,

De Maio and Levi-Minzi (*Journal*, July 1979, **135**, 73–76) compared three different dosage schedules in treating neurotic depressed patients with Amitriptyline. They find no difference whether the drug is given in the morning, at night, or three times a day. They suggest that, as all patients improved comparably, this was an effect of the drug. This is an unwarranted assumption, particularly as these were all inpatients. They should have had a fourth, drug-free, group: it being my view that if the inpatient milieu is a good treatment situation, this group may well have improved equally.

I am always surprised that organically-minded psychiatrists admit so many neurotic patients when they appear to hold the view that the important therapeutic tool is the drug which, of course, could be given, and at far less cost, to outpatients. (Neurotics are usually avid drug takers and do not need supervision). I submit that we all admit patients not so often because they are a danger to themselves or to others but because the different milieu has considerable healing effects. We are short of studies that evaluate which parts of the milieu are helpful and which parts are not.

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HARLOW ON CHILD DEPRIVATION

DEAR SIR,

Although F. H. Stone in his selection of readings relevant to child psychiatry (*Journal*, August 1979, **135**, 180–1) includes a reference to the WHO monograph in which there is a reassessment of Bowlby's original monograph he fails to do the same with respect to Harlow's work in the same area—deprivation.

Harlow's early work (1959) showed the importance of contact comfort for normal development in the infant monkey. This leads him to say that "the long period of maternal deprivation had evidently left them incapable of forming a lasting affectional tie".

However, the later work of himself and his colleagues has shown that, at least for rhesus monkeys, critical periods do not exist—i.e. that it is possible to habilitate infant monkeys deprived of maternal attachment to the extent that they acquire many normal, species-typical behaviours. These are not lost

as a result of deprivation of social contact but merely suppressed and can be learned under carefully selected and controlled conditions. One of these involves the use of younger, surrogate-peer-reared 'therapist' monkeys who have not yet learned those aggressive responses emitted by older monkeys which Harlow predicts have impeded attempts at habilitation.

I hope these additional references will enable readers to put Harlow's work into a different perspective from that one gets from reading his early work.

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TOXIC REACTIONS TO LITHIUM AND NEUROLEPTICS

DEAR SIR,

Toxic neurological reactions to combined lithium/haloperidol treatment was first reported by Cohen and Cohen, 1974 (*Journal of the American Medical Association*, **230**, 1283) and Loudon and Waring, 1976 (*Lancet*, *ii*, 1088), especially at high serum lithium levels and high doses of haloperidol. Thomas also reported (*Journal*, May 1979, **134**, 552) a further case differing in that the patient had experienced previous treatment with lithium/haloperidol combination without developing toxic side-effects. A similar syndrome was recorded by West, 1977 (*British Medical Journal*, *ii*, 642) after exposure to lithium/flupenthixol combination.

I would like to report a case of toxic reaction to combined treatment with lithium and fluphenazine:

A 25-year-old man with a manic episode and a seven-year history of manic-depressive psychosis was given fluphenazine, 75 mg in a single i.m. dose which was repeated one week later. In addition, patient was receiving chlorpromazine, 300 mg and trihexiphenidyl ('Artane'), 5 mg daily. Haloperidol drops were given eventually and for a few days in a maximum dose 2.5 mg/day. Lithium treatment was started 10 days after the second administration of fluphenazine and more than 10 days after the last administration of haloperidol, with 900 mg lithium carbonate daily, giving serum level 0.9 mEq/l. Four days after starting lithium treatment the patient developed tremulousness, rigidity, dysarthria, ataxia, tiredness, vomiting and confusion. Serum lithium level was 1.0 mEq/l. Lithium and chlorpromazine were stopped and 'Artane', 30 mg/day and 'Disipal', 12 mg/day, were given without any significant effect. The patient gradually improved and became functional after two months, with no clear evidence of organic brain damage.

One month later he became hypomanic and lithium treatment was attempted again starting with low doses (300 mg) and progressively increasing to 1800 mg/day, in addition to chlorpromazine, 300 mg/day. No side effects were noted, while serum level was 0.86 mEq/l. In previous episodes the patient had been treated with large doses of neuroleptics (chlorpromazine, 900 mg, haloperidol, 30 mg daily and fluphenazine, 75 mg/week) without exhibiting side-effects.

The case suggests that the toxic reaction was due to lithium-fluphenazine interaction, as previous treatment with neuroleptics and subsequent treatment with lithium and chlorpromazine, but without fluphenazine, did not produce adverse effects. Haloperidol, given in a small total dose long before lithium administration seems not to account for the side-effects observed.

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INFORMAL PATIENTS DETAINED

DEAR SIR,

The analysis of compulsory admissions by Elliott, Timbury and Walker (*Journal*, August 1979, **135**, 104–14) gives only a partial picture of the implementation of Section 31, the emergency Section. While the authors refer to "a three-fold increase in the use" of powers to detain informally admitted patients they omit the precise figures for these cases. A recent unpublished study by me at the Royal Edinburgh Hospital revealed that of 100 consecutive Section 31 applications, 38 were in respect of resident patients. If this use of the Act were included in the Gartnavel study, the mean annual figure of 71.6 Section 31s, which the authors reported, would undoubtedly be very much higher.

The use of 2nd, 3rd and 4th Section 31s in 10 per cent of the 1962–72 cohort is worrying and is clearly at variance with the intention of the lawmakers. The consequence is that patients are detained for 14, 21 or