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PSYCHODYNAMIC CHANGES IN UNTREATED NEUROTICS

Dear Sir,

In their paper (*Journal*, May, 1968, p. 525) describing varieties of psychodynamically suspect patients, Milan, Bacal, Heath and Balfour appear to accept the following three propositions:

1. At follow-up, symptom improvement is no greater in psychodynamically treated than in untreated patients.
2. Symptoms are a response to identifiable stress, which the patient cannot handle because of personality disturbances.
3. Psychodynamic therapy relieves personality disturbances, so that the patient can handle the identifiable stress in a new way, without developing symptoms.

At follow-up a similar proportion of treated and untreated patients will be subject to the identifiable stress (either to its continuance or recurrence).

At least one of the propositions must be incorrect.

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MANDRAX AND DICHLORALPHENAZONE

Dear Sir,

In the April, 1968, issue of the *Journal* (p. 465), there is an article by Ijaz Haider: "A comparative trial of Mandrax and dichloralphenazone". I should like to comment on certain aspects of this evaluation.

The study was designed so as to generate a number of preferences for one or other drug on as many pairs of nights. In other words, each subject was tested for three pairs of nights, one night of each pair being either dichloralphenazone or Mandrax. Forty-eight subjects were said to have been tested. Preferences, both subjective (patients) and objective (staff) were gathered.

The trouble is that in the author's Table I the paired preferences add up to the specified N in no single instance! As an example, for patient preferences, for the first pair of nights there is a total of only 41 preferences, for the second pair of nights there is a total of only 35 preferences, and for the third pair of nights there is a total of only 34 preferences. Since Dr. Haider did not specify anywhere in the text what happened to the remaining preferences (that one would have expected from a sample of 48 subjects), it is difficult to make any sense out of the Table. Did the missing preferences indicate that the unheard-from subjects had no preference, or did it mean perhaps that they were dropped from the study, or did it mean that the records were lost?

Even if we accept the total N of 41 for the first pair of nights, 35 for the second pair of nights, and 34 for the third pair of nights, as to the patients' preference, and then turn to Table III for the statistical analysis of difference, we find serious errors. Chi squares for the successive pairs of nights are not as stated in Table III but are rather ·61, ·71, and ·47, respectively. The significance in all these instances is $\cdot 30 < p < \cdot 50$, a result totally different from the author's Table III.

There may be other errors in this paper; I have not bothered to check the statistics in all of the tables. However, the extent of the ambiguity and statistical error in just this instance is enough to cast doubt on the remainder of the study.

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TREATMENT OF PREMATURE EJACULATION

Dear Sir,

I refer to an article "The use of methohexitone sodium in the systematic desensitization of premature ejaculation" by Tom Kraft and Ihsan Al-Issa (*Journal*, March, 1968, p. 351). It seems to suggest that this drug has some special advantage over thiopentone sodium in premature ejaculation. It is certainly safer than thiopentone and recovery time is quicker. On the other hand, we find that during administration of 1 per cent. solution of methohexitone pain at the site along the vein is intense, and is enough to cause anxiety and distraction in the majority of cases. Our hospital anaesthetist (1) has tried using a more diluted form of methohexitone than recommended (i.e. 0·5 per cent.) but reports that pain is still experienced in no less than 35 per cent. of cases.

Premature ejaculation differs only in degree from the majority of impotence cases, if we exclude the few in which the causes are deep rooted in the process of psychosexual development (although Kinsey *et al.* (2) do not agree). Impotence and premature ejaculation are quite common in this part of the country; in most cases the causes lie in the personality, lack of sex education and rarely in the

practice of premarital intercourse. Such people seem to generate considerable anxiety in their initial sexual life and so certain patterns become established. In this unit the treatment consists in relaxation under thiopentone sodium, instruction in sexual techniques and temporary abstinence from intercourse. In fact most of my patients improve with 8 to 10 sessions directed towards crystallizing their concept of sex, explaining the physiological processes involved and helping them to regain their self confidence. Small doses of thioridazine are helpful. Psychogenic impotence needs intensive treatment and here again I prefer thiopentone sodium.

As to the question whether "a relationship might exist between decreased frequency of micturition and the increase in the duration of erection", the answer seems clear. Anxiety is the basic cause, producing both frequency of micturition and premature ejaculation or shorter duration of erection.

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PROGNOSIS OF SCHIZOPHRENIA BEFORE AND AFTER PHARMACOTHERAPY

Dear Sir,

I have read with interest Dr. Michael Pritchard's response to my comments on his paper (*Journal*, June, 1968, pp. 781–782). In all fairness it seems to me that he has circumvented the methodological issues which I had raised.

Implicitly, if not explicitly, I had referred solely to somatic treatments. Dr. Pritchard's reference to psychoneurosis and psychotherapy I must, therefore, regard as irrelevant.

I certainly agree that speed of recovery and duration of hospitalization may have a significant effect on outcome. But there remains the fact that Dr. Pritchard stated in his paper that "since the policy throughout has been to admit only non-compulsory patients for relatively short-term treatment, administrative changes in this hospital between the two periods are likely to have been minimal". Moreover, a comparison of the patients of Groups A and B did not reveal any significant differences in the length of stay in the hospital according to the study. This would seem to minimize, if not eliminate, the speed of resocialization as a factor influencing long-term outcome.

I therefore maintain that Dr. Pritchard has failed to explain on what basis drug treatment as reported could be expected to influence long-term outcome.

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CHEADLE ROYAL

Dear Sir,

In my review of Cheadle Royal Hospital's Bicentenary History (*Journal*, July, 1968, p. 891), I said it was a pity that the illustrations were all of Cheadle Royal today and that no historical illustrations had been included.

This is true in the sense that all the buildings shown are in existence today; but it has been pointed out to me that some of the photographs (those between pages 38 and 39) were taken round about 60 or 70 years ago, and some of the buildings have been modernized or put to new uses.

I still think it would have been appropriate to have reproduced some of the illustrations from Brockbank's previous history, now out of print.

I also implied that there are now only three former Registered Hospitals surviving; I should have said "surviving as independent institutions"—others have, of course, been absorbed into the National Health Service.

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Herts.

COUPS DE GRÂCE

Dear Sir,

Speaking personally, I no longer have any opinions whatever on evolution. The *coup de grâce* to my opinions was given by Dr. Ross Ashby's comments (*Journal*, May, 1968, p. 660) on the fact that the Piltdown skull had given the *coup de grâce* to his opinions on ESP.

Anita Gregory.

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AN INVITATION TO CALIFORNIA

Dear Sir,

Some eighteen months ago you very kindly published in the *Journal* a letter in which I issued to British and European psychiatrists who were planning a trip to North America an invitation to visit California to take part in the teaching programmes of the Californian State Hospital System.

I may say that the response to this letter was excellent and we were privileged to have some very