References

1. Granville-Grossman, K. L., and Turner, P. (1966). "The effect of propranolol on anxiety." *Lancet, i,* 788–790.
2. Peterfy, M., Pinter, E. J., and Cleghorn, J. M. (1967). "The adrenergic receptors and anxiety." *Amer. J. Psychiatry,* **124,** 157–158.

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