

because almost all phobic patients seen at St. Thomas' are treated with anti-depressants; however, in spite of this, and the problems of a retrospective study, a great deal of new information was gained which went towards answering a number of questions we considered to be important, namely:

- (i) phobic patients improved on the treatment régime.
- (ii) panic attacks were reduced.
- (iii) there was no difference in the response of agoraphobic patients compared with those suffering from other phobias.
- (iv) significant improvement occurred in the first month.
- (v) the results appeared to be comparable and in certain respects superior to those obtained in a controlled retrospective study of behaviour therapy, although differences in patient populations limited the value of this comparison.

On the basis of this information it now seems justified to carry out a double-blind controlled trial of phenelzine versus placebo. We now know the type of patients we wish to study in such a trial, the appropriate dosage of medication, the importance of assessing panic attacks, and the duration of treatment which is likely to be necessary to get a partial response. A prospective study is at present being conducted conjointly at the Maudsley Hospital and at St. George's Hospital by one of us (D.K.). This trial should answer the question of whether phenelzine alone is superior to placebo in treating phobic patients, but it cannot be a substitute for the information gained by following as many as 196 adult patients over the course of a year's treatment, or for the unique opportunity of examining the effects of M.A.O.I.s on childhood phobias. The patients are being treated with either phenelzine or placebo for two months, but a crossover design is not being used because of the 'carry-over' effects of initial treatment and because it seems unjustified to substitute a placebo if a patient is improving on active medication and gaining confidence in overcoming phobias. Past experience in substituting placebo for phenelzine in patients who were becoming less phobic resulted in such a high relapse rate that the project was abandoned.

In clinical psychiatry, as in the whole of medicine, new treatment possibilities will continue to be discovered. In our view, it is not only ethically but also scientifically acceptable to establish the potential value of a treatment régime before embarking on a prospective trial in which a placebo is used, because of the many difficulties for patient and therapist

which it entails. Our study has done this to our satisfaction.

DESMOND KELLY.
WAGIH GUIRGUIS.
EVA FROMMER.
NITA MITCHELL-HEGGS.
WILLIAM SARGANT.

St. George's Hospital, S.W.1.

St. Thomas' Hospital, S.E.1.

Benham Hospital, Helwan, Cairo.

[In his last paragraph Dr. Mawson puts a question to the Editors to which the short answer is 'no'.

There are many ways of advancing our understanding of treatment in psychiatry; and scrupulously conducted double-blind trials, and other efficiently designed experiments, cover only part of our needs. No useful method of treatment was ever yet discovered in a strictly controlled trial, but such trials have their place when the exploratory work has been done. It is to be hoped that there will always be room in the *Journal* for the conscientious retrospective reporting of good pioneer work.

Dr. Mawson expects too much. There is, unfortunately, no work at all, published in this or any other psychiatric journal, which is not open to serious methodological criticisms. Even controlled drug trials contain a large make-believe element, since serum levels of the drug are not monitored over the trial periods.

No doubt the success claimed by authors in uncontrolled studies is generally greater than the success reported in controlled studies. Of course part of the difference will be due to self-deception—optimistic self-deception by the therapist, and also, at times, negativistic self-deception by the anti-therapist. But it seems likely that a large part of the difference in results is real. Though we cannot do without them, controlled studies are unfortunately very insensitive tests of therapeutic potentialities. It is not possible to get, by giving standardized doses at set intervals over a fixed length of time to an arbitrarily selected group of patients, the same results from a psychotropic drug as can be obtained by a clinical expert sensitively selecting his patients and dosages, individual by individual, on a basis of experience. The ethical dilemma cannot be escaped. One cannot both carry out a therapeutic experiment and do one's best for the patient who has placed himself in one's care.

Eds.]

DEPRESSION AND CARCINOMA

DEAR SIR,

In their article in the *Journal* for November, 1969, Kerr, Schapira and Roth report that 'deaths from

carcinoma among male patients with depression were significantly more frequent than expected'. Their study was conducted on patients admitted to a psychiatric hospital.

Furthermore, Brain and Henson (1958) include depression as one of the non-metastatic neuropsychiatric abnormalities that may antedate the appearance of carcinoma by at least three years.

A retrospective survey at this hospital did not confirm these findings.

Questionnaires were sent to the general practitioners of three groups of patients between the ages of 20 and 65 years, viz. 127 males with malignant disease seen at this hospital in 1968 or 1969, similarly 179 females, and 127 males with no malignant disease but with illnesses requiring hospitalization and matched for age with the cancer males.

The questionnaire requested information about any depression in the three years preceding the onset of symptoms due to the neoplasm or other illness; the treatment received and the response to this; whether psychiatric referral was necessary; and whether it was the first depressive episode.

For the purposes of this study patients were considered to have been depressed if their general practitioners had thought they required treatment for their affective disorder. No attempt was made to differentiate between endogenous and reactive illnesses.

The following information was obtained:

	With Depression		Without Depression	
	No.	%	No.	%
Males with cancer ..	6	5.7	107	94.3
Females with cancer ..	39	24.7	119	75.3
Control males ..	12	14.6	71	85.4

The non-depressed patients include;

(i) Three females and 2 males with cancer whose general practitioners volunteered that they suffered from anxiety but no depression (many more may have been anxious, but this was not studied by the questionnaire), and

(ii) One female, 3 males with cancer, and 1 male control whose depressions were not thought to warrant any treatment (drugs, psychotherapy or ECT).

Depression was less common, to a statistically significant extent, among male patients with cancer than among male controls ($\chi^2=3.9$ 1 d.f. $p < 0.05$).

Furthermore, the sex difference noted by Kerr and his co-workers is not confirmed by the findings

here, as the ratio of the incidence of depression in the male and female cancer populations (4.3 : 1) is not very different to that found in a general practice survey by Porter (1970) where it was 4.7 : 1.

The incidence of depression is high in all groups compared to Porter (1970), who found that 0.65 per cent of males and 2.93 per cent of females in a general practice were depressed, and also to Shepherd *et al.* (1964) who found neuroses occurred in 5.7 per cent of males and 11.6 per cent of females in general practice.

Cancer males with depression

Name	Age	Site of lesion	Post psychiatric history	Treatment and response
GM	56	Bladder	No previous depression	No improvement with drugs from G.P.
OF	55	Mixed parotid	Alcoholic	Partial recovery with drug from G.P.
EF	62	Follicular lymphoma	No previous depression	Was depressed for six months before he developed haematuria. Following investigation and treatment here he required transfer to a psychiatric hospital where he recovered with drugs.
KL	62	Bronchus	Chronic anxiety state	Partial recovery with drugs from GP
BI	63	Renal pelvis	Recurrent depression	No improvement with drugs from G.P.

It seems likely that these authors were restrictive in their criteria of mental disturbance, and that mildly depressed subjects were excluded from their

studies. Furthermore, the discrepancy may be partially accounted for by difference in age distribution; in my series the majority of patients were over the age of 55 years. Although a few patients had been referred to psychiatrists in earlier years, none required this during the three year period except for M.E.F. (see Table above).

From these data there is no evidence that depression is a frequent precursor of neoplastic disease in men.

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F. A. JUDELSON.

*Department of Psychiatry,
St. Mary's Hospital,
London, W.2.*

ATTEMPTED SUICIDE: NOMENCLATURE

As a term, 'suicidal gesture', fails because it implies that the act was insincere or faked. 'Parasuicide' is preferable only because it has no precise meaning; but if it were adopted, it would soon assume the same connotation as 'suicidal gesture,' and other terms would have to be coined *ad infinitum*.

In dealing with suicidal patients there are two major pitfalls, viz.

(1) When it is considered acceptable for the patient to make a genuine attempted suicide, but not so if in retrospect the method used appears to have had no chance of success. (2) When the individual is assessed in terms of what the majority do.

It would be naïve to imagine that changing a diagnostic label would prevent mistakes. However, I agree with Dr. Kreitman and his associates that in this matter the nomenclature requires to be changed.

I suggest therefore that the term 'Threatened Suicide' be used for these cases. It would be used in the same way that obstetricians use the term threatened abortion, that is, in danger of aborting. Surely a patient in desperation, who, for example, falsely states that he has taken an overdose of pills, has threatened suicide. The following categories of suicidal behaviour would therefore be recognized:

(A) FATAL OUTCOME

(1) Accidental death from poisoning, injury etc., (not generally referred to as suicide).

(2) Suicide.

(intentional injury, poisoning etc.).

(3) Suicide following threatened suicide by injury, poisoning etc.,

(this implies that the doctor diagnosing is not sure if the patient really intended to kill himself, and implies the possibility of the patient having misjudged the harmful effect of the injury, poison etc.).

(B) SURVIVAL

(4) Accidental injury, poisoning etc.,

(5) Attempted suicide (failed).

(6) Threatened suicide by poisoning, injury, drug overdose etc.

ANGUS DODDS.

*Gartloch Hospital,
Gartcosh,
Glasgow.*

ATTEMPTED SUICIDE AS LANGUAGE

DEAR SIR,

In their paper (*Journal*, May 1970, pp. 465-73), Kreitman, Smith and Tan raise many interesting points, none of them related to language. They accept as a possibility the view that 'many so-called suicidal attempts function as a form of communication between the patient and the key figures in his environment, most often conveying an appeal for attention.' They hypothesise that 'the individual within the "attempted suicide subculture" can perform an act which carries a preformed meaning; all he requires to do is invoke it. The process is essentially similar to that whereby a person uses a word in spoken language, though certain important differences also exist (such as the relative lack of precision which often characterises behavioural as opposed to semantic communication.'

The hypothesis makes the all-too-easy assumption that communication is equivalent to language (though less precise along some undefined but presumably semantic dimension?) This is a low redefinition which denies language precisely its essential characteristic, i.e. that meaning is not performed but is generated by syntactic combinations, and confuses it with the pre-syntactic learning of a one to one link at the conceptual level of a meaningful sign or gesture.

Syntax is basic to language, and since non-verbal behaviour like self-poisoning is patently without it, attempted suicide as language is not a concept that empiricism could verify, any more than one could