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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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.

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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 empirically verify whether an attempted suicide had been committed in the passive voice or the future tense.

However, in pointing out that non-verbal behaviour is not syntactical one is not thereby denying that attempted suicide may for some people be a means of drawing attention to their plight. Rather is one suggesting that the suicidal behaviour is not a form of 'See *what* I mean' but 'See *that* I mean (what I say)'.

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ACTUARIAL V. CLINICAL PREDICTION

Dear Sir,

I have recently pleaded (McConaghy, 1969), that editorial responsibility should include seeing that if the conclusions reached in articles are not the only ones which are consistent with the relevant data, the alternative ones are at least presented. Otherwise the majority of readers who do not have time to read articles carefully will accept the only conclusions put forward as established. Can I extend this responsibility to requiring, at least of review-type articles, that when the research work of others is quoted at some length this is so done that it is not necessary to refer to the original reports to ensure they support the reviewer's conclusions.

I am confident Professor Sines (*Journal*, February, 1970, p. 129–44) is aware that it would grossly favour actuarial v. clinical prediction if the data were analysed after a study was completed, the items selected which best accorded with the feature to be predicted, and the correlation of these items with the feature accepted as a measure of actuarial prediction. I therefore presume that all the studies he quoted applied predictive relationships determined prior to the study being carried out, not only those few where he stated this was so. Without consulting the original article it is difficult to see how this could be so from his description of Lindzey's study, which suggests that the 85 per cent actuarial prediction was made with a formula developed in the course of the study. As Professor Sines points out the disfavour done to actuarial prediction when a predictive formula is tested on a different population from that from which it was derived, it would seem not unreasonable that he should warn against this opposite and unfortunately still too common error of accepting unpredicted relationships found after a study is completed, before they are tested on a similar population.

Basically I am in agreement with his in fact extremely modest claims, pointing out as he does the limited relevance of actuarial predictions, at least at present. On *a priori* grounds one would expect clinical impressions to be most valuable when they are based directly on the interview. In this situation the clinician is able to form hypotheses as he goes along, and obtains data to support or refute these by suitable direction. When, as in the majority of studies he quotes, this flexibility is lost by limiting the clinician to making his judgements from the protocol of an MMPI or a TAT, most psychiatrists would, I think, expect that better predictions would result from actuarial methods.

This point that clinical impressions may be more valuable in some situations, actuarial in others does, I think, need stressing. The previously widely promulgated belief that improvement in patients in clinical trials should be measured by rating scales rather than by a clinical estimation of global improvements is rarely refuted, despite the evidence that the clinical estimate is as good as any rating scale measure and considerably better than many. (Lipman *et al.*, 1965; Rickels *et al.*, 1965). I consider the abandonment of the clinician's judgement and the complete reliance on rating scales in this situation probably responsible for such bizarre findings as that thioridazine is as effective an anti-depressant as imipramine (Overall *et al.*, 1964); as well as negative findings as to the value of the anti-depressants themselves (Ashby and Collins, 1961; Hare *et al.*, 1964). These results, so much at variance with clinical experience, seem to have produced a loss of interest in carrying out controlled trials, so that for some years now such important questions have been left unanswered as whether some depressed patients would respond better to tricyclic anti-depressants, others to MAO-inhibitors; and whether those who fail to respond to drugs from one of these groups would respond to one from the other.

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