Brit. J. Psychiat. (1970), **116**, 237–240

Correspondence

ATTEMPTED SUICIDE

DEAR SIR,

Dr. Kreitman and his associates, in a letter published in this column in June, 1969 (pp. 746–7), declared the term 'attempted suicide' to be highly unsatisfactory because the great majority of people so designated were not in fact attempting suicide but simulating or mimicking it. They proposed the term 'parasuicide' instead. My belated response to the letter is not due to lack of interest but to the desire to let others have their say first. The only comment that has appeared to date came from Dr. Merskey (*Journal*, October, 1969, p. 1227), who rejected the proposed term. He pointed to the risk of death people attempting suicide incur, small though it may be in many cases, and he rightly criticized the proposers for not doing justice to the complexities of motivation underlying suicidal acts.

The authors' proposals concern not only nomenclature but also the classification of acts of self-damage and their psychopathology. They raise the question whether the term 'attempted suicide' ought not to be retained for 'patients to whom the phrase really applies'. Let us see where this would lead to. We should have parasuicides, attempted suicides and suicides. As parasuicides will occasionally be fatal, they would have to be subdivided according to outcome. The differentiation between parasuicide and malingering on the one hand and attempted suicide on the other would have to be worked out. The fatal parasuicides would presumably be regarded as accidents. The authors probably envisage an escalation in some cases from parasuicide via attempted suicide to actual suicide.

Dr. Kreitman and his associates fail to define their new term or to delimit it from related concepts. Defined it must be before its usefulness can be assessed. What is parasuicide *para* to? Clearly, suicide has to be defined first before parasuicide can be considered. As the authors start off with the statement that the great majority of patients designated by the term 'attempted suicide' are not in fact attempting suicide, it is also incumbent on them to say what they mean by attempting suicide. This they fail to do, but it seems that to qualify for being classed by them as 'attempted suicide' the person has 'to address himself to the task of self-destruction', and it should be possible 'to construe his behaviour in any simple sense as oriented primarily towards death'. Whether or not this applies has presumably to be decided from what the person is able or willing to divulge. The unreliability of this criterion is notorious. Many people committing acts of self-damage of all degrees of dangerousness do not consciously address themselves clearly to anything, but this does not imply that their action has no meaning. It follows from the authors' requirements for 'attempted suicide' that only those fatal acts of self-damage in which these specifications are met can be classed as suicides. This would be in keeping with the practice of many coroners not to give a suicide verdict unless there is evidence for complete and undivided determination of self-destruction. Any sign of flirtation with life disqualifies. Considering that such signs can be discerned in most suicidal acts, including many fatal ones, it is not surprising that the validity of suicide rates based on such verdicts has been questioned.

While I do not regard 'parasuicide' as viable, the question of definition and classification of acts of self-damage is very important. We cannot leave it to the individuals committing acts of self-damage to classify themselves, and we have to accept the fact that they are often incompletely aware of the motivations and purposes of those acts. I have repeatedly pointed to the evidence (1) that most people who commit them do not want either to die or to live, but that they want to do both at the same time, usually the one more, or much more, than the other. This formulation includes those who are determined to kill themselves, yet take no reasonable safeguards against intervention from outside, as well as those who appear to think foremost of the immediate effects of their actions on others, yet take no safeguards against a fatal outcome. We have to adopt an operational definition of suicidal acts, such as the following: *A suicidal act is any act of deliberate self-damage which the person committing the act could not be sure of surviving.* This means that acts of self-damage which the person could not have felt to be potentially dangerous to life should not be classified as suicidal. The above definition takes into account the gamble character of suicidal acts, and makes it clear that the doctor's knowledge about the hazards incurred is not decisive for the classification of the act if it has been committed by a person not in possession of that knowledge. Whether or not a method of self-damage could be felt potentially dangerous to life by the average lay person will require some study. At a W.H.O. meeting of experts on suicide it was thought that in the case

of poisoning the use of a dose of a narcotic three times the medium therapeutic dose qualified for an act of self-poisoning being regarded as suicidal.

I still think that for the time being a non-fatal act of self-damage falling into the above definition should be described as attempted suicide and a fatal one as suicide. Attempted suicides should be sub-classified or graded according to their 'lethality' (2) and other criteria. In the proposed operational definition, suicidal acts of various degrees of dangerousness are viewed as parts of a continuum in which the relatively harmless to the almost certainly fatal can be placed. Terms like 'parasuicide', 'pseudocide' (3) or 'suicidal gesture' tend to deny or at least to obscure this fundamental fact for which there is ample clinical evidence.

ERWIN STENGEL.

7 Montrose Court,
Hill Turrets Close,
Sheffield, S11 9RF

REFERENCES

(1) STENGEL, E. (1969). *Suicide and Attempted Suicide*, 2nd revised edition. London: Penguin Books.
(2) FARBEROW, N. L., and SHEIDMAN, E. S. (1961). *The Cry for Help*. London: McGraw-Hill.
(3) LENNARD-JONES, J. E., and ASHER, R. (1959). 'Why do they do it?' *Lancet*, *i*, 1138.

CIRCADIAN RHYTHMS IN MANIC-DEPRESSIVE PSYCHOSIS

DEAR SIR,

The interesting survey of circadian rhythms in 12 depressive patients by Drs. Moody and Allsopp (*Journal*, August, 1969, pp. 923–28) seems to suggest that a disturbance of man's central timing device may lie at the root of this malady.

It is about ten years since Dr. Janet Harker first described the isolation and successful transplant of a central physiological clock. Although the animal concerned was only the humble cockroach, this work does seem to have great theoretical importance. Somehow it carries the implication that in higher creatures as well this internal clock is likely to be an anatomical entity, probably placed in close relation to the hypothalamus.

That normal man is subject to an inner, 'circadian' rhythm, at variance with that of the solar day, has been thoroughly proved by isolation experiments on healthy volunteers, such as those by Professor Aschoff. It has been argued therefore that in health the central physiological 'clock' is highly responsive to external (or solar) time, and synchronizes with it. However, in depression this internal clock tends to re-establish its 'primitive' dominance. That this change-over to an intrinsic circadian timing is likely to be only partial is due to some residual influence being retained by the solar time-scale, at least during the active hours of the day. It is such an uneasy equilibrium which may account for the variations in the shift of water and electrolyte excretion, such as the authors noted in their twelve patients.

Certainly, the classical disturbance in the sleep pattern of this illness could be seen to fit into this schema of 'time out of joint'. In these circumstances, the central or intrinsic clock may be said to exact its own pathological tribute by triggering the arousal mechanism at inappropriate times.

As for the seasonal increase in depressive illness in spring and autumn, this could be explained by the changes in background illumination acting as a stress on the smooth running of the solar rhythm. This may enable the intrinsic rhythm to break through, with subsequent release of the depressive reaction.

This model lends itself quite well to testing by experiment, where isolation facilities are available. A suitable depressed subject may be monitored under conditions of complete deprivation of external (solar) time. One might predict that this would eliminate the stress which stems from the postulated conflict between external and inner time, and may in this way lead to clinical amelioration. Alternatively, a fit volunteer could, in a similar setting, be administered depression-inducing drugs in order to discover if changes such as the 'functional shift' in hypothalamic function, described by Dr. Pollitt, will still ensue.

Also, such a model could provide a gratuitous bonus for those interested in the physiology of sleep. The paradoxical and non-paradoxical alternating phases of the sleep cycle could then be considered as a dynamic equilibrium between the intrinsic rhythm emanating from the central clock, and the solar-conditioned rhythm. It is the REM phase of sleep which would need to be equated with the 'primitive' inner rhythm control.

However, experimental EEG findings, such as those obtained by Dr. Oswald in depressed subjects, remain inconclusive in that direction.

KLAUS HEYMANN.

21 Ladbroke Grove,
London, W.11.

REFERENCES

HEYMANN, K. G. (1967). 'Depression and the physiological clock.' *Practitioner*, **199**, pp. 224–7.
HARKER, J. E. (1960). 'Endocrine and nervous factors in insect circadian rhythms.' *Cold. Spr. Har. Symp. quant. Biol.*, **25**, pp. 279–87.
ASCHOFF, J. (1963). 'Gesetzmässigkeiten der biologischen Tagesperiodik.' *Dtsch. Med. Wschr.*, **88**, pp. 1930–7.