mediately followed three nights on the potent drug. Such an effect would, of course, be diluted by ensuring that half the patients got drug B first.

I am not suggesting that either phenobarbitone or nitrazepam is inert, but just hope more notice may be taken of modern knowledge about sleep and drugs.

IAN OSWALD.

Department of Psychiatry,
University of Edinburgh,
Royal Edinburgh Hospital,
Morningside Park,
Edinburgh EH10 5HF

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DEAR SIR,

A short presentation of a clinical trial does not permit, unfortunately, a detailed discussion on all problems encountered during the study. Actually, we were aware of the more important points discussed by Dr. Oswald, even if this does not appear clearly from our paper. Although some of his objections seem to us rather irrelevant, we welcome this opportunity to comment on his letter.

We agree with Dr. Oswald that phenobarbitone is no ideal hypnotic, at least not for prolonged use. It is not used in the daily routine at our clinic (in fact, we use very little barbiturates). We would also like to point out that in our paper we did not recommend the use of this drug—we only pointed out some differences in its clinical action versus nitrazepam, which was the main subject of our study. We are quite confident that the readers of this journal are aware that other factors than those discussed in our paper must be taken into consideration when choosing among the many available hypnotics. However, we found that phenobarbitone could be used in our particular study, in which it was given in the low dosage of 100 mg, for three successive nights only, and with a 'wash-out' period of two nights in between the active drugs. Dr. Oswald points out the well-known fact that the blood level of phenobarbitone decreases very slowly. There is, however, no simple correlation between blood level, or even total body level, of barbiturates, and their effect on sleep (see Goodman and Gilman 1965). In our study there were several indications that phenobarbitone did not have as long-lasting effects as would have been expected from the slow elimination from blood: (1) The frequency of 'hangover' was exactly the same after phenobarbitone and placebo, and only slightly (not significantly) less after nitrazepam. (2) The average time-profile of sleep during the night (objectively measured) was almost exactly the same for phenobarbitone as for nitrazepam. (3) Sleep on phenobarbitone, whether objectively or subjectively assessed, did not differ significantly between the first, second or third night on this drug.

If phenobarbitone in our study had a shorter duration of action than has been found in experiments on 'normal' subjects, the reason may have been that most of our patients had previously received drugs which are known to increase the rate of metabolic degradation of barbiturate in the liver.

Still, there is the possibility that the patients' judgements of their sleep when using phenobarbitone were in part influenced by some subtle persistent effect of the drug, as suggested by Dr. Oswald. However, if the patients were generally 'unrealistically self-satisfied' in the mornings after the phenobarbitone nights, one would have expected a higher over-all ratio of subjective : objective assessment of sleep on phenobarbitone than on nitrazepam. In actual fact, these ratios were exactly the same (as can be seen from Table I in our paper). But in one subgroup of patients there was a discrepancy: patients who had difficulties in going to sleep because of disturbing thoughts were rated relatively higher on the subjective than on the objective scale when using phenobarbitone. In our paper we do not give any definite explanation of this finding, but offer some discussion on it. It could be that 'unrealistic self-satisfaction' should also be taken into consideration, although it is difficult to see why this should be important in this type of patient only.

Dr. Oswald states that we 'make the usual error in assuming that one night is independent of the next'. As a matter of fact, we were not quite unaware of this—it was, for example, the main reason why we used placebo between the active drugs. The possibility of carry-over effects in a cross-over study is certainly a disadvantage, but in our opinion it must be weighed against the greater advantage of using each patient as his own control, in a study like ours. We do not think that carry-over effects between the

active drugs made any significant difference on the results in our trials. Thus, there was no significant difference in results between the groups of patients who received phenobarbitone first or nitrazepam first. As to the possibility of rebound effects interfering with sleep on placebo nights, re-examination of our data does not lend support to the assumption that such an effect should be more marked after phenobarbitone than after nitrazepam.

Dr. Oswald comments at some length on the presumed non-validity of the comparison between active drugs and placebo. Although he makes the erroneous assumption that the night nurse, who made the objective rating, also gave the sleeping pills and knew the different appearance of active tablets and placebo tablets, we agree that the comparison between placebo and active drugs was not quite valid, and we believe we made this point quite clear in our paper. We had no intention of making a valid comparison between active drugs and placebo; our primary aim was to compare phenobarbitone and nitrazepam. We consider it amply proven by numerous previous trials that both drugs are better than placebo. As already stated, our main reason for using placebo was to have a wash-out period in between the drugs; the first placebo night also served as a crude control of the patient's sleeping pattern.

Dr. Oswald objects to our use of the term 'quality' of sleep. However, we think it appears from the context in our paper that this term was only used as a reference to those aspects of sleep, rather than the mere length of the sleeping period, which may have some influence on the patient's own judgement of whether he has slept well or badly. We think common clinical experience indicates that such a term, even if not precisely defined, is warranted.

Our main conclusions were, firstly, that the active drugs did not differ significantly in their over-all effect on sleep, and secondly, that there was a highly significant tendency for patients complaining of early insomnia because of disturbing thoughts to report better sleep after phenobarbitone than after nitrazepam. We do not see that Dr. Oswald's comments make these conclusions invalid.

O. LINGJAERDE.
T. ANDERSEN.

Oslo University Psychiatric Clinic,
Vinderen,
Oslo 3, Norway

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'PARASUICIDE'

DEAR SIR,

So far the score on 'parasuicide' as an alternative term to 'attempted suicide' (proposed by us in your columns, June 1969) is two-nil against us. Dr. Merskey (October 1969, p. 1227) and Professor Stengel (February 1970, pp. 237–8) both vote for continuing the status quo. Both seem to us to have missed our main contention, which is that 'attempted suicide' continues to cause untold mischief and confusion, especially among general practitioners and other non-psychiatrists. Not unreasonably these colleagues assume that the term means what it appears to mean, namely an attempt at self-destruction. Consequently we still hear of patients not receiving psychiatric assessment because they were 'only making a gesture' or the like. The importance of this issue is not that our terminological sensibilities are outraged, but that patients suffer. The matter is a serious one, and we submit that some alternative to existing nomenclature *must* be found. Our critics contribute nothing to this task.

An awareness of the urgency of the problem led Professor Kessel to propose 'deliberate self-poisoning or self-injury'. While we endorse some of the reasoning behind his suggestion, the choice of term seems to us unfortunate for the reasons set out in our previous letter. In particular, dropping all reference to suicide seems regrettable; hence our own proposal. It is ironic that we are then attacked for being insufficiently alert to the overlap between suicide and parasuicide, even though we are proposing the former as the model which the latter simulates. It becomes bewildering when we are also accused of failing to recognize the complexity of motivations underlying suicidal behaviour, of denying the element of gamble and of relying exclusively on the patient's stated intention. Nothing we have written justifies such comments.

It is difficult to comment briefly on the numerous other issues raised by Professor Stengel. Of course we accept that the usual legalistic concept of suicide is too narrow, and that self-inflicted deaths often bear elements of a desire to survive. Among parasuicides, however, the ambivalence is not, in our experience, usually related to living or dying so much as to a mixture of other seemingly incompatible motivations such as hostility against, and appeal to, a spouse. Often the patient has little wish, overt or covert, to die, but may want others to consider him as one who has been driven to desperate measures. We note Professor Stengel's *new* definition of suicidal behaviour, but doubt if it can be used operationally, for all his and Dr. Merskey's concern for the difficulties of the epidemiologist.