Brit. J. Psychiat. (1979), **134**, 647–52

Correspondence

RELIEF OF NIGHTMARES

DEAR SIR,

Dr Mark's article (*Journal*, November 1978, **133**, 461–5) on rehearsal relief of a nightmare concerns a rather involved case, but I would suggest the method should be more widely used. I am a trainee GP, and with the help of my trainer have been treating an 80-year-old man with a recurrent nightmare. Treatment consisted of rehearsal relief which has been dramatically successful.

The patient's dream was of his experience as a 17-year-old boy during the First World War. He remembered learning how to bayonet a dummy and how horrifying it was to perform this in real life on a man who shouted 'mother' as the bayonet plunged in and how it was necessary to stamp on his chest to extract the bayonet. He then had to continue running and passed a comrade who, mortally wounded, implored him to stop and help. The patient was unable to do so because of his infantry training and fear of court-martial or being shot in the back for cowardice. The guilt involved in this sequence had not left him for 63 years and he had apparently relived the nightmare most nights.

He had not talked out the nightmare with his wife and had only briefly mentioned it to one of his sons 5 years previously during a period of increased guilt and depression.

Consultation was conducted in his bedroom since the original contact came following a heart attack. I feel this setting encouraged him to talk, but he is a religious man and, fearing death, may have wanted to make peace with God. He repeated the nightmare in detail on four separate occasions and was exhausted after each session.

He also revealed that his father was an alcoholic who at times threatened his mother with a razor, and she had taught him to pray regularly and never to be a coward; also that one of his sons was killed in the Second World War and he felt this was God's punishment for the murder he had committed. Guilt was further increased since he could not tell his wife it was his fault.

Following these sessions the patient was relieved of the nightmare, although he still had vivid dreams. I suggest that this technique, although time consuming, could be used successfully in general practice, especially in the patient's home.

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SIMULATED AND REAL ECT

DEAR SIR,

The paper by Lambourn and Gill (*Journal*, December 1978, **133**, 514–19) was less disquieting to me than the two answers in the February 1979 Correspondence section (Barton and Snaith; Dowson (**134**, 220–1) which have difficulty in interpreting the results. In my opinion the actual explanation of the equal results in the two groups of the original article was the use of unilateral rather than bilateral ECT. Unilateral ECT was tried by most of us, but it was soon recognized as being inferior in the work of many clinicians and research workers who originally had been favourably impressed by the new method. My own experience was equally negative, and I often see ECT failures treated by others with unilateral ECT who respond immediately when bilateral treatment is instituted.

It should be remembered furthermore that barbiturate anaesthesia alone has a moderately favourable effect on various psychiatric conditions, and it is not surprising that the group treated with 'simulated ECT' showed some improvement. It was frequently my thought that whenever results are obtained with unilateral ECT they are partly due to the repeated barbiturate anaesthesia rather than to the convulsion. The paper by Lambourn and Gill supports this view.

Four decades of worldwide experience with ECT in the most severely psychotic patients is sufficient proof that it is not 'the mystique associated with an unusual form of treatment' that is effective. The authors of the original paper seemed to be aware of the problem of unilateral ECT. It is quite possible that if they had not limited themselves to 6 treatments but had given at least 12 treatments they would have achieved better results in their ECT group.

What their interesting investigation demonstrates, is again the poor therapeutic effectiveness of unilateral ECT.

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CHANGES IN SELF-RATING OF SYMPTOMS

DEAR SIR,

Bedford, Edington and Kellner (*Journal*, January 1979, **134,** 108–10) assume that response set 'is likely to make a test more stable, i.e. less sensitive and therefore less suitable for the measurement of changes related to treatment'. Though agreeing with the latter point, our own experimental work leads us to disagree totally concerning the assumption of greater stability. We have conducted a number of experiments to examine behaviour of response set with re-testing. A wide variety of subjects have been asked to rate photographs of faces for a number of items, some connected with psychiatric symptoms, especially mood and anxiety. Where *unipolar* item scales have been used (5 and 7 point and 100 mm line) they were perceived invariably though unwittingly as *bipolar* scales with an assumed opposite pole and mid-point. We have found that the sum of all scores lying above the mid-point initially falls dramatically on a subsequent occasion a week later. Similarly all scores below the mid-point move upwards.

In one experiment ten subjects were tested on four weekly occasions and the effect was seen even up to the fourth week. Calculations were made using both the *explicit* mid-point (i.e. 3 for 5, or 50 mm for the 100 mm line) and the *implicit* mid-point (grand mean of all scores). Some differences between the two methods are evident, but the picture overall is the same regardless, and changes in scores followed this way are significant beyond the 0.001 level. An implication arising is that rating scales containing items scaled for severity in the same direction, giving a simple total score, could show a drop in severity with re-testing alone (the photographs do not change).

We have conducted a *post hoc* test for this by extracting an eight item scale (from 18 items) equivalent to a depression/anxiety rating scale for two of our experiments. Where subjects initially rated high (one standard deviation or above), then on re-testing there was a fall significant beyond the 0.05 level thus confirming our prediction. Further research is being conducted with recorded speech and for the effect of drugs on change in response set with re-testing.

We do not suggest, of course, that the patients in the study by Bedford *et al* did not benefit from treatment but we think another explanation is available. Their sentence 'after affirming an item the patient then rates the intensity or frequency of occurrence of that item' is in our terms *those scores which initially fall above the mid-point*. We hope soon to publish our preliminary data in full and regard this 'Heracleitean Phenomenon' as an alternative explanation for the so-called placebo effect and a hitherto unrecognised serious source of error variance in treatment studies.

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CONVERSATIONS WITH SCHIZOPHRENICS

DEAR SIR,

Dr Morgan's account of his conversations with a group of chronic schizophrenic patients (*Journal*, February, 1979, **134,** 187–94) is of considerable interest to those working with similar patients, and compels admiration for his persistence, compassion and humour.

However, we have recently completed a study of 'old long-stay' patients which suggests it may be easy to form a misleadingly simple picture of their behaviour and overlook aspects which show it in a more complex light. Their shrewd understanding of what mattered to them day-to-day emerged clearly in our study, as indeed it does from Dr Morgan's conversations, and it is difficult to understand why he gives this little weight in comparison to interest in fields such as politics, from which they will have been excluded for most of their lives.

However, it is clear that his patients are severely disabled, having been selected by failure to respond to a sustained programme of social and occupational rehabilitation. Uncertainty about the precise effect of their disabilities is less important than doubts about the fundamental conclusions he draws from them about the course of schizophrenia. Dr Morgan assumes that the current levels of disability are due to continuing progression of schizophrenic illnesses, and that therefore 'the current community-orientated style of managing such illnesses will result in such chronic schizophrenic patients becoming no less disabled outside hospital after a similar length of illness'.

But he offers no evidence that his patients are undergoing a continuing process of deterioration. What he describes are intractable rather than progressive disabilities: a crucial distinction. Amongst a sample of the most disabled long-stay patients in