

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.

DAVID CUTTING

*St John's House Surgery,
28 Bromyard Road,
Worcester*

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.

LOTHAR B. KALINOWSKY

30 East 76th Street,
New York, N.Y. 10021

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.

GERALD SILVERMAN
MANGAL KATZ

St Bernard's Hospital,
Southall, Middlesex

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

Goodmayes we did not find a single example of the progressively deteriorating course which is the traditional stereotype of chronic schizophrenia. All the patients had either been maximally disabled at the time of first admission to hospital, or their deterioration had ceased to progress at least ten years previously; the end-state described by Bleuler (1972) in *Die schizophränen Geistesstörungen im Lichte langjähriger Kranken- und Familiengeschichte*.

This evidence that chronic schizophrenia tends to stabilize is supported by a number of long-term studies, including Bleuler's own personal follow-up of over 200 patients and Daum, Brooke and Albee's 20 year follow-up of 253 patients, and accords well with clinical experience.

This is not, of course, to suggest there will be no chronic schizophrenics in the community, but taken in conjunction with evidence that the most severe and crippling forms of the illness are less common than in the past (Hogarty, 1977, *Schizophrenia Bulletin*, 3, 587-99) it predicts a more hopeful future than the tenacious myth of inevitable, progressive deterioration.

DAVID ABRAHAMSON

Goodmayes Hospital,
Barley Lane, Ilford

London Borough of Southwark,
Social Services Department

DEBORAH BRENNER

Reference

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NO LUNG CANCER IN SCHIZOPHRENICS?

DEAR SIR,

I was prompted by the letter from Dr D. Rice (*Journal*, January 1979, 134, 128) and by the recent death of one of my chronic schizophrenic patients to look at post-mortem records at Rainhill Hospital—made available to me by Dr A. S. Woodcock, F.R.C.Path. In the past five years post-mortem examination has confirmed the presence of lung cancer in eight patients. Three with no previous psychiatric history had an acute psychotic episode of the type familiar in this condition; two had long-standing recurrent depressive illnesses; three were typical chronic schizophrenic patients of at least twenty years duration before the terminal illness. Two of them had been continuously in hospital (since 1953 in one case and 1956 in the other), while the third had been maintained at home, thanks partly to a supportive family. Histologically the tumours

were: oat cell, poorly differentiated squamous, and a well differentiated papillary adenocarcinoma.

D. V. COAKLEY

55 Rodney Street,
Liverpool 1

BRITISH POLICY ON OPIOID MISUSE

DEAR SIR,

Professor G. Edwards (January, 1979, 134, 1-13) refers to a paper of mine (1) by the wrong title, date and page, and misquotes some figures from it. He has made the mistake of combining results from my study with those of a previous one by Bewley *et al* (2), though he lacks the necessary data. The passage in his article should have read: 'of 112 opioid users whose deaths were reported in the United Kingdom, 24 were not known to the Home Office before they died'. These deaths deserve more attention than Edwards has given them because they represent some of the price paid for the present British policy.

The prescribing of NHS heroin or methadone—whether this is done by general practitioners or by specially licensed doctors—does not protect against the high morbidity, mortality and infectious nature of opioid misuse (1, 4). There is, therefore, an alternative option to the ones Edwards has proposed. This is to stop the prescribing of opioids for self-administration altogether, and for medical personnel to administer them to patients considered suitable for maintenance treatment. The advantages of this approach are that it would diminish the above risks, officially acknowledge that the medical risks are too great to justify using medical means (prescribing opioids) for social ends ('keeping the Mafia out') and enable different maintenance schedules to be tested. Certain problems would remain such as when to start maintenance treatment (5) for a 'new case' or for one who has relapsed, and when to stop because, say, a patient is misusing illicit drugs. The disadvantages would include the logistics of implementing this scheme and the possibility of stimulating a criminally organised black market.

Although it may have been justifiable in 1967 to be so fearful of what might happen, Edwards shows that there is less cause for alarm today and that the present policy should be reviewed.

R. GARDNER

Fulbourn Hospital,
Cambridge CB1 5EF

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'MEANING AND VOID'

DEAR SIR,

A review as muddled factually and conceptually as Dr Berrios' review of my book, *Meaning and Void: Inner Experience and the Incentives in People's Lives* (*Journal*, September 1978, 133, 270–1) compels a reply. Since space restrictions do not permit a reasoned point-by-point rebuttal to the review's lattice of misrepresentations, this letter can only indicate the nature of the principal discrepancies.

Dr Berrios misrepresents me as equating 'meaning' with 'incentive' and of setting incentives up as 'a kind of *primum mobile*'. In fact, on p. 24 I wrote "The idea that incentives control behaviour . . . manages to hide as much as it reveals", and I go on to point out its circularity. Most of the book from that point on is devoted to nailing down what this 'pedestrian truth' (Berrios) may mean in terms of specific functional relationships among psychological processes and conditions—the conditions that govern attraction to objects and that determine the rise and fall of value, the role of affect in this process, the effects of frustration, and the clinical implications. The incentive-related *systems* involved are certainly regarded as pivotal features of human life, but this is very different from representing incentives as prime causes.

The review wonders about the relevance of '138 American students talking about the importance of meaning in their lives'. In fact, that isn't what they talked about, and that paragraph further misrepresents the function, number, and diversity of the samples involved in that four-page section of the book.

Contrary to Berrios, the book never refers to lack of meaning as a cause of depression or as a cause of anything else, other than to reflect a motivational basis for attempts to alter one's state of consciousness.

The review misrepresents several chapters as unoriginal rehashes of stale material. The reviewer

noted the 'expected' references but ignored the rest, as well as the original integrations. For example, are expectancy-value formulations of suicide really as customary as all that? How many books have systematically formulated principles of value change, or have traced the role of affect and habituation in value, drawing on the experimental as well as clinical literature? Above all, this book develops original current-concerns and incentive-disengagement approaches to motivation.

The review misrepresents the book as espousing a 'view of depression based on learning', a view that much of Chapter 5 is specifically devoted to rejecting.

There is much more to be said. Berrios's review simply does not fairly represent the book. I urge you to consult it yourself.

ERIC KLINGER

*University of Minnesota,
Division of Social Sciences,
Morris, Minnesota 56267,
USA*

ELECTROSLEEP

DEAR SIR,

I was interested to read your recently published study of methadone withdrawal with electrosleep by Professors Gomez and Mikhail (Gomez and Mikhail, 1979), and to learn that they had found electrosleep successful under controlled conditions, but was disappointed by the brevity of their discussion which made no mention of possible mechanisms and only mentioned four previous studies. I am not sure whether, by this, they were implying that electrosleep is so well accepted that discussion is unnecessary, or so peculiar that discussion is impossible . . . Neither of these situations apply, and I suspect that many of your other readers would also welcome the authors' fuller discussion of the results of their otherwise admirable paper.

At my own review some years ago (Hall, 1973) over a hundred previous articles on the subject were brought to my attention, and there had even then been two international symposia held at Graz in 1966 and 1969, a controlled trial carried out by American workers (Rosenthal and Wolfson, 1970) and the subject had been reviewed in several of the foreign science bulletins put out by the United States Library of Congress (Ivanovsky, 1967, 1968 and 1969) since electrosleep had been introduced by Livenstev in 1949. Despite one's inevitable scepticism about a treatment which is pleasant, quick, economical and without side effects, and which several eminent neurophysiologists have quite properly explained to me is scientifically far from respectable, an admittedly

small study (Hall and Smith *et al*, 1974) suggested that electrosleep was as effective in the relief of neurotic anxiety as tranquillizers and/or hypnosis, and I have continued using it in my own Unit in the treatment of anxiety states (some extremely intractable and 'placebo-proof') despite my own and my staff's basic scepticism.

As Gomez and Mikhail indicate, the actual technique of electrosleep is almost ludicrously simple to administer (Hall, 1973). The possibilities seem to be that the current may simply act on the scalp hair follicle receptors, producing an electronic 'cradle rocking' effect, that the monotonous repetitive stimuli may produce light hypnosis, that an induced alpha EEG rhythm may be produced (much as in biofeedback techniques), or that the whole technique is simply electronic mumbo-jumbo.

Nevertheless, there seem to be recurrent suggestions that the presence or absence of 'alpha type' ten hertz electrical microcurrents in the head are related to the presence or absence of subjective anxiety (e.g. Smith, 1973), and Lippold and Redfearn (1964) found under double blind controlled conditions that resistant depression may respond to treatment with microcurrents. Since Victorian times there have been numerous swings of fashion's pendulum regarding 'electrotherapy' of various types in psychiatry, and particularly as electroplexy, hitherto considered 'respectable' appears to be under increasing attack. I feel that some rigorous research studies of electrosleep are overdue.

PETER HALL

Worcester Royal Infirmary,
Newtown Road,
Worcester WR5 1JG

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SCHIZOPHRENICS WHO WEAR EARPLUGS

DEAR SIR,

Dr Frith's paper (*Journal*, March 1979, **134**, 225) is an interesting addition to the recent literature on the possible relationship between primary cognitive abnormalities and the secondary development of schizophrenic symptoms. It is refreshing in a speculative paper of this sort to have the predictions deriving from the theory spelt out so explicitly. However, I wonder if, in formulating his first prediction (that reducing the ambiguity of stimuli should reduce hallucinations), he has too readily dismissed the idea that schizophrenics may experience some temporary beneficial effects from a reduction in sensory input. Faced with the problem of information overload (Hemsley, 1977) it has been postulated that schizophrenics may utilize one of a number of different strategies in their attempts to cope. They may for example minimize their contact with arousal-heightening stimuli by total social withdrawal (Venables and Wing, 1962). Alternatively they may employ less drastic methods to lessen perceptual input. One such case I have seen recently.

Case Report

A 20-year-old single man of West Indian origin was undergoing a day hospital rehabilitation programme following recovery from an acute psychotic illness. His breakdown had been of sudden onset, had had certain affective features and there were no residual signs of a definite schizophrenic process. His phenothiazines were therefore cautiously stopped. He fared well initially, but after a few weeks became unsociable, refused to attend groups and began to neglect his personal appearance and hygiene. His mother complained that when she visited him in his flat she found that he was no longer coping with his day-to-day household chores. In some respects his behaviour became decidedly odd. For example, he would attempt to memorize pages in the Bible and then tear them out and throw them away. Shortly after this he began to wear cotton-wool earplugs. When interviewed he denied hearing voices but said that extraneous sounds bothered him and made him feel ill-at-ease in a way which was difficult for him to describe. These sounds included the ticking of clocks and 'certain notes' in the music which was played almost continuously over the radio in the day hospital workrooms. Wearing earplugs, however, improved his sense of general well-being and helped him to 'hear

things clear'. He refused to consider restarting anti-psychotic drugs and over the course of the next three weeks, with earplugs in situ, he improved socially in his work and in his relationship with his parents. This amelioration proved but temporary, however. His decline was heralded by the announcement 'Half of my brain is linked to the Moon'. Following this he developed bizarre quasi-religious ideas and delusions of control by outside influences. It was necessary to readmit him to an in-patient ward where he eventually improved again on phenothiazines.

In addition to this man I have personally seen two other male schizophrenics who have worn earplugs during exacerbation of their illnesses and who claimed they felt better for doing so. Both of these differed from the case I have described in that auditory hallucinations formed a prominent part of their complaints. Conversations with colleagues who mention similar cases suggest that the phenomenon is not rare.

It may well be that the wearing of earplugs is a product of the disordered logic of the schizophrenic. Auditory hallucinations are perceived as arising from outside the head and cotton-wool plugs are used to keep them out. The reason for a non-hallucinated patient wearing earplugs comes less readily, but it could perhaps be explained away as simply another example of a pointless irrational schizophrenic affectation. The phenomenon, however, bears an alternative explanation which would make the exercise seem less futile. It may be that the schizophrenic, during the early stages of his illness or during

an exacerbation, finds himself overwhelmed by sensory input from his surroundings. He seeks ways to reduce this and may temporarily succeed in relieving his subjective distress and in improving his performance. It is tempting to suggest that this was true of the patient I have described as well as others like him who adopt the seemingly eccentric habit of pluggings up their ears.

P. MCGUFFIN

*The Bethlem Royal Hospital,
Monks Orchard Road,
Beckenham, Kent*

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OMISSION

Unfortunately, in the paper by Drs Bond, Cundall and Falloon entitled 'Monoamine Oxidase (MAO) of Platelets, Plasma, Lymphocytes and Granulocytes in Schizophrenia' (*Journal*, April 1979, **134**, 360-5) both the address for reprints and the authors' address were omitted from the end of the paper. Drs Bond and Cundall are working at St John's Hospital, Stone, Aylesbury, Buckinghamshire and this is the address for reprint requests.