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Electroconvulsive Therapy in the Elderly

SIR: O'Shea *et al* (*Journal*, February 1987, **150**, 255–257) present the case of a 91-year-old patient who responded well to ECT. The oldest documented patient to receive ECT was a 94-year-old woman (Bernstein, 1972). We describe the successful use of ECT in a woman aged 103 years.

Case Report: The patient, who was born on 6 December 1883, was referred to us by her GP with a history of worsening depression over the preceding weeks. On admission she appeared very depressed and expressed the idea that God no longer wanted her. She felt that she was evil and that she belonged to the devil. She also said that she felt "dirty and rotten" inside. She was preoccupied with religious ideas and spoke of little else. Her family reported that her interest in outside events had diminished prior to admission and her appetite had deteriorated. She was diagnosed as suffering from a psychotic depression.

She told us that her first episode of depression had been 40 years previously, when she lived in England. This had recurred from time to time over the years and her first admission to this hospital was in 1977. That episode was similar in its presentation to the current one. At that time she was treated with ECT but relapsed soon after. Altogether, in 1977 she received three courses of ECT, each of approximately four treatments. There was no definite family history of affective illness. Physical examination during this recent admission revealed that she was suffering from mild congestive cardiac failure and atrial fibrillation. She was receiving digoxin and frusemide for this. She was treated initially with a course of mianserin and later of doxepin. These produced no change, and after eight weeks of in-patient care she was still depressed and miserable. We discussed ECT with her and her family and she agreed to receive a course. The anaesthetist had some reservations, but in view of her persistent depression and previous response to ECT he agreed that it was an acceptable risk to give her a general anaesthetic. She received two treatments in all and was remarkably improved after the second. She was somewhat confused after the treatment, but this disappeared within 24 hours. As she was by now well, it was decided not to proceed to a full course of ECT. It is now two months since the patient's discharge and she remains well.

We think that this report demonstrates the effectiveness of ECT as a treatment for psychotic depression even in the very elderly age-group. We agree with Weiner (1982) that ECT is a relatively safe treatment in the elderly if performed with due precautions.

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Delusional Parasitosis

SIR: Macaskill's report (*Journal*, February 1987, **150**, 261–263) of a patient's delusional infestation which responded to non-pharmacological treatment was optimistic, but probably describes a mild form of disorder – as might be expected when clear precipitants are present. However, the potential dangerousness of these patients must be emphasised.

Case report: After the death of his wife and 22 years of police service in Antigua this 70-year-old man emigrated to England, where he has lived alone and worked as a security officer for 18 years (until his retirement in 1982). His complaints about insects started in 1980 when he was rehoused in council property following compulsory purchase of his flat: since then his life has been dominated by delusions of infestation. He has persuaded the council to rehouse him three times, despite having set one flat on fire and been convicted of criminal damage for flooding another. He has also persuaded Environmental Health officers to fumigate his property eight times, although they have never seen any insects in his flats. He was detained under Section 3 of the Mental Health Act in 1983, following a fire, but he was discharged by a tribunal.

In November 1986 he was readmitted under Section 136 following two fires. Mental state on admission revealed a dishevelled, smoke-stained, angry man who shouted loudly and aggressively about his detention and complained that "insects swarm all over me". He was cognitively intact and had a normal computerised tomography scan. During the month of observation while detained under Section 2 this mental state persisted. A Section 3 order was made, and treatment with haloperidol syrup (Andrews *et al*, 1986), up to 30 mg/day, was started. There was a general improvement: he was able to conduct a normal conversation and he became calmer and even friendly. However, his delusions of infestation have persisted and he continues to buy large quantities of fly sprays. He has recently been treated with fluphenazine decanoate as a long-term treatment and because compliance was doubtful at times.

Suicide has previously been reported (Bebbington, 1976), but we are not aware of a report of fire setting associated with this condition, thus indicating that this may be a rare occurrence. However, fumigation is often used by these patients and it seems essential to explore any attempts or intentions by them to use fire or smoke to combat the infestation, and to consider admission in order to protect both the patient and neighbours. Many such patients relapse when treatment is stopped, which raises legal and ethical difficulties when long-term treatment is attempted.

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Is there a Lithium Withdrawal Syndrome?

SIR: The letter from Hawkins & Shepherd (*Journal*, February 1987, **150**, 273) contained a number of inaccuracies and misconceptions.

Firstly, they state that the words 'withdrawal' and 'relapse' are not interchangeable. I make it clear in my opening sentence that the paper is concerned with "the possibility of relapse being caused by drug withdrawal". Their criticism would seem to be an unnecessary exercise in semantics.

Secondly, they state that to show a withdrawal effect it is necessary for there to be a "fall in the relapse rate lower than expected for a period after the withdrawal syndrome has ended". This is clearly wrong. The question is whether there is an *increased* risk of relapse in the withdrawal period in addition to that which would be expected taking into consideration the natural history of the disease process. This is not the same thing.

Thirdly, they state that the theoretical relapse rate "can never be known", and ask "How can one distinguish a withdrawal state causing relapse and relapse alone?" Perhaps they are not aware that this is why control groups are used in order to estimate the theoretical relapse rate. In the control group only 8% of patients relapsed in the first three months, compared with 28% in the experimental group. This difference cannot be explained by the experimental group being at higher risk, because after three months the relapse rates are identical.

Finally, no firm conclusions can be drawn from a retrospective study, hence the title poses a question. Correspondence in the same issue (*Journal*, February 1987, **150**, 264–265) has highlighted the fact that despite increasing use of lithium there has been an increasing readmission rate for mania at a number of different centres. There has been no satisfactory explanation for this. One possibility is that repeated lithium withdrawal increases the number of relapses, and I think this topic deserves further consideration.

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BITE: Self-rating Scale for Bulimia

SIR: The paper by Henderson & Freeman (*Journal*, January 1987, **150**, 18–24) is a useful and timely development in the scientific study of eating disorders. However, four points require further consideration.

Firstly, the authors need to clarify whether they regard the BITE as a screening test or as a diagnostic instrument. The statement that "subjects achieving a high score have a high probability of meeting... criteria for bulimia" indicates clearly that it is a screening test. However, the observations that the BITE "can be used to *identify* binge-eaters in a given population" (our emphasis), and that it provides the information necessary to make a DSM–III diagnosis of bulimia, suggest that the authors also consider it to be a diagnostic instrument.

Secondly, the criterion for caseness is unclear. In study 1, the only criterion given for the patient group is that they were binge-eaters at "various stages of treatment". This is too imprecise – an operational definition is a central requirement for work of this nature. The absence of such a definition from study 1 is all the more mysterious since one was used in study 2. Where diagnostic criteria are considered, it is not always clear which are meant. DSM–III criteria were used in studies 2, 3, and 4, while the authors conclude, in their instructions for administration, that high scorers have a high probability of meeting "DSM–III criteria for bulimia *and* Russell's (1979) criteria for bulimia nervosa" (our emphasis). There is a crucial difference between the two: DSM–III criteria are relatively broad, in contrast to Russell's criteria which require evidence of a morbid fear of fatness. By which criteria does the BITE identify cases?