Alzheimer's disease is not worth £800 per year (rivastigmine), even when it saves on institutional fees; a saving for the local authority but not for the NHS.

One accepts that the drugs budget has limitations, but when compared with the cost of other less effective interventions the price is relatively small. If we do not stand up for our patients the case will go by default as, for example, is typified by the lukewarm approach of the Alzheimer's Disease Society.

GOODKIN, D. E. (1998) Interferon beta therapy for multiple sclerosis. *Lancet*, **352**, 1486–1487.

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Rationale behind psychiatrists' choice of drugs

Sir: I read Johnson Dickson-Mulinga's survey (*Psychiatric Bulletin*, November 1998, **22**, 677–680) with considerable interest. However, why is the author so worried at half of his respondents quoting "personal experience" as the most important influence on their prescribing habit and why does he assume that such prescribing is not rational or not scientific and non-problem solving?

For that matter, what scientific evidence do we have to imply that prescribing habits should only be evidence based? Is it just another example of our rush to join the evidence-based medicine club without realising that not only is the process of evidence gathering at its infancy but more importantly that there are still some basic flaws in the very process of evidence collection and its implication and implementation in routine clinical practice (Sikdar, 1997; Thornley & Clive, 1998)?

Thus I feel it is unfair to be suspicious of or look down upon "personal experience"-based prescribing practice as second best, so long as it works for an individual patient and does not cause any harm.

SIKDAR, S. (1997) Evidence-based psychiatry: which evidence to believe? *British Journal of Psychiatry*, **171**, 483–484.

THORNLEY, B. & ADAMS, C. (1998) Content and quality of 2000 controlled trials in schizophrenia over 50 years. *British Medical Journal*, **317**, 1181–1184.

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Maximum output of ECT machines

Author's reply: The case report by Galloway *et al* (*Psychiatric Bulletin*, November 1998, **22**, 713–714) did not include a definition of what constitutes a "satisfactory fit", and no information was given about the techniques of anaesthesia and seizure monitoring. It was not possible therefore to distinguish intrinsic and extrinsic causes of a failure to induce adequate cerebral seizure activity. This distinction is not a theoretical nicety. If the induction of cerebral seizure activity was impossible because too much intravenous methohexitone had been administered, then I would suggest it was more appropriate to ask the anaesthetist to give less anaesthetic than look for a more powerful ECT machine.

Our suggestion, and this was the word we used, that the Ectron Series 5A ECT machine was not underpowered is only amenable to scientific disproof. We have not been able to do so locally. The Edinburgh protocol to measure the initial seizure threshold and our techniques of anaesthesia and seizure monitoring were described in our paper (Dykes & Scott, 1998). We now have data from 540 courses of ECT, 158 in people aged 65 years or older (Glen & Scott, 1999). The maximum initial seizure threshold was observed in a 72-year-old man with depression treated with bilateral ECT and who also took carbamazepine as a maintenance treatment of a bipolar affective disorder. His seizure threshold was 325 mC, the maximum output of the Ectron machine is 700 mC.

DYKES, S. R. & SCOTT, A. I. F. (1998) Initial seizure threshold in bilateral electroconvulsive therapy. *Psychiatric Bulletin*, **22**, 298–299.

GLEN, T. & SCOTT, A. I. F. (1999) Rates of electroconvulsive therapy use in Edinburgh (1992–1997). *Journal of Affective Disorders*, in press.

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