

guidelines for the use of ECT in both children and adults.<sup>4</sup> In many of these cases ECT could be life saving or the most effective prevailing treatment. Abrams<sup>5</sup> reviewed relevant publications and concluded that when ECT is used judiciously, there is no evidence that its use in children is less efficacious than in adults.

Finally, the use of ECT, like any other medical treatment needs training and expertise.<sup>4,5</sup> It is not meant to be used in an ad-hoc fashion, and to advocate a moratorium on its use because it has would be a mistake. It would be far more prudent on our part to educate physician and public alike on its enormous potential benefit and to ensure its proper and judicious use.

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SIR—Baker calls on the medical profession to eschew the use of ECT in the treatment of the mentally ill, especially in adolescents and children. He asks us to indulge his prejudice and to forego an effective and safe treatment. His plea is not supported by evidence of therapeutic inadequacy or undue risk of the treatment in the group he selects for special consideration.

ECT is effective and safe, accepted by medical and psychiatric communities everywhere. Both the UK Royal College of Psychiatrists<sup>1</sup> and the American Psychiatric Association (APA),<sup>2</sup> after much consideration, provide practitioners with recommendations for treatment, consent, training, and privileging (admitting privileges). These recommendations establish precise guidelines to protect the needs of the mentally ill, including those who are regarded as being additionally disadvantaged by reason of age. Although the Royal College report makes no specific consideration for adolescents, the APA report details indications, consent, and consultation guidelines for this group. The recent resurgence in interest in ECT results from the large numbers of therapy-resistant mentally ill individuals for whom present psychotherapies, milieu therapies, and pharmacotherapies have failed. For such populations, whose characteristics have been well defined, ECT is an effective and safe procedure, and is one that is used with increasing frequency.<sup>3</sup>

Baker dwells on the child and adolescent psychiatry arms of the profession that emphasise psychotherapy and family therapy. These attitudes have limited the experience with ECT in pre-pubertal children; so much so that the data are too sparse to warrant useful opinions as to the indications or risks in this population. In post-pubertal adolescents, however, there is adequate case material to support the prevailing view that ECT is a consideration in individuals whose illness, had it occurred in adults, would have been treated with ECT.<sup>4</sup> Baker's call for an interdiction of treatment in patients under age 16 is not founded on experience or physiology. It is artificial and prejudicial,

depriving adolescents for whom this treatment might provide the opportunity for help.

Baker complains that "there are no controls for the administration of ECT". This charge is untrue. The controls for ECT are even more rigorous than for most interventions in medicine and surgery. In response to public and professional puzzlement as to why or how induced seizures could be of clinical help, the profession has repeatedly reviewed the practice of ECT, established standards of treatment and consent, improved ECT devices, encouraged continuous monitoring of procedures, and fostered extensive improvements in training.<sup>1,2</sup> The best control of any clinical practice remains established community standards of practice, education of patients, and their informed consent in their treatment. Since most ECT is given in hospital, institutional privileging and quality assurance review procedures provide additional protection.

He says that the energies used to induce seizures are too high, and that the minimum energies needed for adolescents are not available in UK devices. Although I cannot speak for the energy of UK devices, I am aware that US devices are marketed in the UK. We have long known that age is an important determinant of the energy needed to induce a seizure. Various threshold stimulation dosing strategies have been elucidated and are now part of modern practice.<sup>5</sup> In view of the extensive work, for more than six decades in the UK and the USA, that has gone to support this practice, we should reject Baker's argument and not support his plea.

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## Intraluminal treatment of inoperable oesophageal tumours by intralesional photodynamic therapy with methylene blue

SIR—The quality of life of patients with inoperable oesophageal tumours is mostly impaired by intraluminal tumour growth.<sup>1</sup> Among the established therapies are thermocoagulation, X-ray irradiation, chemotherapy, and/or dilation/stenting. Since the efficacy and complications of these therapies are debated,<sup>2</sup> other techniques are being explored. Photodynamic therapy (PDT) combines the application of a sensitiser and irradiation with light of the corresponding absorption maxima. To evaluate the effectiveness of intralesional PDT in inoperable oesophageal tumours, 3 patients with small intraluminal recurring carcinomas (maximum 2×2 cm) were treated twice in 2 weeks.

The thickness of the tumours measured by endoscopic ultrasound was 5, 7, and 9 mm (to within 1 mm). 1 hour before irradiation, a methylene blue solution (1%) was injected directly into the tumour by endoscopy until the tumour, appeared deeply blue. The mechanism of distribution of the sensitiser is unknown. The 662 nm light emitted by an argon-pumped dye laser was used for therapy (dose 30 J/cm<sup>2</sup>, density 150 mW/cm<sup>2</sup>). Intraluminal light application was carried out via a modified fibre for direct

light exposure. During PDT all patients noted moderate burning sensations. 48–72 hours after PDT treatment, we found oedema and necrosis of the tumours to a depth of 4–5 mm by endosonography. After the second PDT treatment there was no macroscopic evidence of intraluminal tumour in all 3 patients. Biopsy specimens from the areas showed scar tissue with no tumour. No other intraluminal adverse side-effects or recurrent local tumour growth in the treated area were noted endoscopically. Ultrasonography showed scar tissue in the treated area. However, the sensitivity of ultrasound examination between scar and possible residue of tumour is poor.

Another intraluminal tumour was found in 1 of the patients after 3 months, which was treated in the same way. The other 2 patients showed extraluminal tumour growth (liver metastasis) after 5 and 6 months of follow-up.

These cases document the beneficial effect of palliative intralesional PDT for small upper gastrointestinal cancer. Cutaneous photosensitivity is a side-effect of systemic PDT,<sup>3,4</sup> which can be avoided if the sensitiser is applied intralesionally. Methylene blue is easy to handle and because of the colouring accumulation of the sensitiser can be limited to the tumour. Light penetration into the tissue is dependent on the light frequency. Consequently, the danger of perforation is small because tumour necrosis can be estimated in all 3 dimensions and is strictly limited to the target.

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## Evening primrose oil and atopic eczema

SIR—Horrobin and Morse (Jan 28, p 260) of Scotia Pharmaceuticals have “revisited” our study<sup>1</sup> on the use of evening primrose oil (EPO) in atopic dermatitis. Whilst their desire to defend their product is understandable, we believe that they overestimate its virtues.

It is incorrect to state that EPO is “the only treatment which has been shown in controlled trials to improve the itch”. Several trials have demonstrated the efficacy of other treatments, including emollients, topical corticosteroids, antihistamines (a subject of controversy), ultraviolet B, psoralen plus ultraviolet A, Chinese herbs, and cyclosporin. It is also incorrect to state that none of these treatments has a rational basis.

In a study where several indices are being measured it can be foreseen that some results will look more “positive” than others. It is therefore essential to decide, in advance, which is to be the main criterion by which the response will be statistically assessed. When our study was planned we specified that the “Leicester score” would be the primary response criterion. It is not appropriate to examine the data in retrospect and emphasise those aspects that appear most favourable.

Horrobin and Morse draw attention to a small part of the data—namely, the “Costa” scores, at week 16 and at the end of the washout period in a subset of patients. They prefer to

ignore the other time points. They ignore altogether the third treatment arm in which patients also received a high dose of EPO. They seem to wish to disregard four other sets of efficacy data—namely, the Leicester scores, the diary scores, the area of skin affected, and the changes in topical steroid requirement.

Horrobin and Morse make much of what they would like to see as the advantages of the Costa scoring system. This system was devised not for use in clinical trials but to facilitate routine monitoring of disease activity in outpatient clinics.<sup>2</sup> Costa et al did, however, suggest that it might be usable in trials.<sup>2</sup> We have a special interest in scoring disease activity for trials in atopic dermatitis and decided to evaluate this scoring system during our trial on EPO. In view of the need for brevity, and the limited interest to a general readership, it was not possible to discuss this in our initial report. Briefly, we have concluded that the Costa score has drawbacks. The most important of these is insensitivity to change. Since disease severity is assessed only at a single site (whichever is the most severe at each visit), very large changes in disease activity over a large proportion of the body may not be detected at all by this score. It is this tendency for the Costa score not to change that is responsible for the lower SD of the change commented upon by Horrobin and Morse. Their interpretation, that this demonstrates superior power to detect a treatment response, is not correct. We found also that one of the signs used to assess disease activity in the Costa score, pigmentary change, varied inversely with severity because changes in pigmentation became more apparent when the eczema was quiescent. We also consider it unsatisfactory to include symptoms and signs in the same score as occurs in the Costa system. These are, by definition, assessed by different observers and signs can be assessed much more objectively than symptoms.

The Leicester sign score has now been used continuously for almost a decade in clinical trials in our departments and others. It has been modified from time to time and we now advocate the use of a slightly simpler version in which only six body zones are assessed.<sup>3</sup> It has proved highly satisfactory and has been well validated. Symptoms, of which pruritus is the most important, are best recorded separately. In our study on EPO patients were issued with diaries and recorded the intensity of pruritus weekly on visual analogue scales. In these diaries the group taking EPO fared worse than those on placebo.

The study on EPO done in Leicester was planned from the outset as a single centre study. There was no prior agreement to add our results to the meta-analysis of small studies performed by Scotia. We<sup>1</sup> and others<sup>1</sup> have already expressed doubts about the validity of this meta-analysis, and the problems that have been highlighted cannot be corrected by the inclusion of our data.

In any event, we are happy to leave the results and subsequent discussion to be reviewed and analysed by the readers who will no doubt judge the facts for themselves.

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