Child psychiatry liaison services

SIR: McFadyen *et al*'s descriptive account of the impact of a child psychiatry liaison service on patterns of referral (*Journal*, January 1991, **158**, 93–96) provides a timely impetus to this rapidly expanding area of child psychiatry.

While it is clear that the reorganisation of child psychiatric services resulted in a considerable increase in the number of liaison referrals, it may be useful to point out that there are other spin-offs from the provision of such services in paediatric units.

Our experience in Coventry suggests that senior registrar trainees in paediatrics, who are exposed to liaison services, incorporate some of the principles involved in those services and, subsequently, on obtaining consultant posts tend to value the contribution which child psychiatry can make to management of the psychological aspects of physical illness. This is in addition to helping to raise the awareness of nursing and other staff. We have noted that in the past six months the number of non self-harm referrals to the department of child psychiatry has virtually doubled since the appointment of a consultant paediatrician who has had training which was strongly informed by child psychiatry liaison services. This strengthening in the link with paediatrics, which we are currently monitoring prospectively, increases opportunities for involvement at many different levels beneficial to patient care.

L. MCGIBBEN
C. BALLARD

Child Psychiatry Unit
Gulson Hospital
Coventry CV1 2HR

Beyond pumpkin seeds

SIR: We are interested in Eagles' letter (*Journal*, December 1990, **157**, 937–938) regarding the use of pumpkin seeds as a source of L-tryptophan, but suggest that there may be an alternative way of tackling the problem.

Since the withdrawal of L-tryptophan, a considerable number of patients previously receiving it have relapsed. In our experience, the time lapse involved has been between two weeks and two months after discontinuation. An answer to this seemed to be the reinstitution of L-tryptophan in a number of people on a named-patient basis. To this end we have generated a 'tryptophan monitoring clinic'. This is run with a multi-disciplinary team of doctor, nurse and hospital pharmacist. Supplies of L-tryptophan in the form of Optimax are available through the hospital pharmacy, via a signed request from the consultant in charge of each case.

The eight patients involved so far are all over sixty-five years old and have a long history of treatment-resistant depression. The majority are receiving lithium carbonate and a tricyclic antidepressant at therapeutic dosage with L-tryptophan as the third part of a triple therapy.

The clinic is held at the Day Hospital. Patients are seen for mental state examination, plus discussion of any risks and problems, by the doctor. There is also an opportunity for discussion with the nurse who takes blood samples to measure eosinophil counts. Monitoring of blood follows Committee on Safety of Medicines (CSM) recommendations and advice from the drug company. The pharmacist gives each patient personal information sheets relating to L-tryptophan and any of its potential side-effects. She dispenses sufficient supplies of L-tryptophan to last until the next clinic appointment (maximum one month). General Practitioners are informed that the patients are receiving L-tryptophan from hospital.

To date, improvement of mood is noticeable in all but one patient. In one hundred and forty-two patient weeks, four eosinophil counts have been very marginally above the normal laboratory range ($0.04–0.4 \times 10^9$/l). These have all reverted to within normal range at early re-test. No patients have shown any physical symptoms of eosinophilia myalgia syndrome.

We envisage continuing this service as long as the need persists. We have also started to see out-patients who are commencing L-tryptophan for the first time and for whom the consultant in charge feels this is the next step in their treatment programme.

SHELAGH AXFORD
OWEN MUTTON
ALISON ADAMS

St Andrew's Hospital
Thorpe
Norwich NR7 0SS

Blinding trials

SIR: I think the dispute between Newcombe (*Journal*, December 1990, **157**, 934–935) and myself (*Journal*, August 1990, **157**, 300) about the value of double-blind trials arises partly because of his idealistic view of the randomised controlled trial. The logic of the method is essentially unassailable, but I am more concerned with the real world of clinical practice. The problem of unblinding will not be solved merely by pleas for improved study design and execution.

Dr Newcombe's useful critique of the study by Karlowski *et al* (1975) fails to reach a practical conclusion about whether ascorbic acid is effective in the treatment of a common cold. Should we, therefore, take the advice of the Nobel Prize winner, Linus

Pauling (1970), with his regime of vitamin C to combat the common cold, based on his assessment of the evidence as a scientist?

Have clinical trials produced firm conclusions about the effectiveness of any treatment in psychiatry? If so, which are the methodologically sound studies? If not, considering the number of studies that have been conducted, would it not suggest there is an inherent difficulty in the design of the 'double-blind' method itself?

As an example, consider the evidence for the use of tricyclic antidepressants in depression in general practice. Hollyman *et al* (1988) found amitryptyline to be effective. By contrast, Porter (1970) found no difference between imipramine and placebo. Interestingly, Porter did not pretend his trial was double-blind, because he recognised that no trial of this kind can be conducted under completely blind conditions. In fact, he openly declared his bias that tricyclic antidepressants probably had no specific action in depression illness, although they may suppress anxiety and agitation by their sedative effect. He argued that his attitude towards the effectiveness of the drug might neutralise the influence of the breaking of the blind. The bias of Hollyman *et al* (1988) is less clear. Has their use of double-blind methods eliminated potential expectancy effects? It is a legitimate question. I am not suggesting it is easy to answer, but some evaluation may be possible with evidence from participants' guesses about medication status. An insistence on statistical purity in the analysis might produce a lack of awareness of the fallacy of the method.

The problem is that the results of 'double-blind' studies tend to be automatically accepted as scientifically valid. A misleading self-deception is encouraged that trials can be conducted double-blind, and the role of expectancies is underestimated. I understand the wish for a scientific basis for psychiatric treatment, but professional status should not mean that the challenge to double-blind methodology goes unnoticed (Oxtoby *et al*, *Journal*, 1989, **155**, 700–701).

DUNCAN DOUBLE

University of Sheffield
Northern General Hospital
Sheffield S5 7AU

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Demand for psychogeriatric services

SIR: We were interested to read the paper by Christie & Wood (*Journal*, August 1990, **157**, 228–231), and share the authors' concern over the failure to match resources to increasing demand, but wish to make two points. Firstly, from our own experience we doubt the findings can be easily generalised to other areas as the authors suggest and secondly, we are concerned that the problem of functional illness in old age may be overlooked because dementia so preoccupies the debate about the ageing population and service provision.

We have had 336 admission episodes (over the age of 65) during the past two years of which 44% were dementias (34% excluding planned respite care), 33% affective disorders, 12% schizophrenias (early and late onset), 1% acute confusion and 10% other conditions, mostly adjustment reactions or medical problems. Christie & Wood report an astonishing 76% of their admissions to be dementia and only 16% functional illness, whereas over 45% of our episodes were functional, over 53% if dementia respites are excluded.

Accepting that the Crichton Royal study only included patients over 69 years of age, these differences are considerable. Christie & Wood cite similarities between Blessed & Wilson's Newcastle study (*Journal*, 1982, **141**, 59–67) and the early Crichton Royal study (Christie, *Journal*, 1982, **140**, 154–159) – both performed in the mid-1970s – as evidence that their findings are of more than parochial interest. However, the Newcastle admission rates more closely resemble our own than the Crichton Royal data at that time with 41% functional, 43% dementia (Newcastle) and 29% functional, 50% dementia (Crichton).

These differences may reflect widely-differing clinical practices, varying illness prevalence, different community provision, social and family support or other factors that distinguish deprived city catchment areas like Liverpool and Newcastle from rural areas like South-West Scotland. Without far more information about these variables it is difficult to interpret the Crichton Royal findings or see how they may translate to other areas. The requirement for long stay dementia beds, for instance, depends on several variables (Blessed, 1988), including the provision of residential care, and the private sector contribution to this varies eight-fold nationwide (Joint Colleges' Report, 1989).