Do antidepressants cause folic acid depletion?

Sir,
I was most interested to read the paper 'Do antidepressants cause folic acid depletion?' (January *Journal,* p.17). However, I was concerned to see so many methodological and theoretical errors and despite being a pilot study it hardly warranted publication.

If the authors wish to make any inferences about the use of antidepressants in general practice then it hardly seems relevant to study a group of elderly patients in an institution who have been taking these drugs for two years. Such a lengthy exposure would be most inadvisable in a general practice setting. We are not offered any diagnoses for these patients, so the reason for the prescription of the antidepressant is unclear — the treatment of low mood in chronic schizophrenia seems most probable. The group taking antidepressants were not all receiving the same drug. Amitriptyline is sometimes used as an appetite stimulant in chronic psychoses and presumably this might colour the results.

It is stated in the introduction that all the individuals in the study were receiving the same diet but in the method it is stated that some were on a 'light' diet and some eating 'normal' meals. We are not informed which patients these were, although the authors admit a different folate composition for these different diets.

In view of the age of these patients the existence of multiple pathologies is likely. However, we are only told that no history of malabsorption existed. Evidently the patients were not examined or otherwise investigated to exclude malabsorption or other pathologies. It would be interesting to have longitudinal data on the folate metabolism of these patients, to see whether this was changed in any way by the medication.

Finally, since the numbers in the study were so small, it would have been possible to publish a scatter plot of the results. It may be that only a few outlying data points are affecting the rest of the data. I note that the standard errors of the means are relatively large. How much the presented data supports the authors' conclusions is difficult to say.

It may be that a further study could iron out some of these problems, but as it stands this pilot study generates many more questions than even hinted-at answers.

B.H. Green

Priory Lodge
Ford Road
Upton
The Wirral
Merseyside L49 0TD

Sir,
To an extent I agree with Dr Green that our article raises problems without solving them.

I would emphasize that we were seeking the effect of a class of agent, not specific drugs, on a vulnerable group. The number of patients in the study was small as people on antidepressants are not common in institutions — a population of 600 yielded only 11 such patients.

Although their medication is not managed by general practitioners, there are patients attending outpatient clinics on long-term antidepressants. Such chronic dosage must also affect a general practitioner in his work if he is a responsible prescriber.

Breaking down the dietary data was not felt to be of benefit as the groups were already small and both 'light' and 'normal' diets were 'adequate' in folate content. The adequacy of intake is also discussed in the article.

Albeit a simple study, it was not easy to complete and may now be impossible to repeat as the population in hospitals declines by the month. Longitudinal studies are a good idea but such a study was not feasible, nor was formal clinical exclusion of a malabsorption state. Much data, including a scatter plot, was omitted to ensure brevity of presentation but it did not hide or tell any more than was given.

Generally, any pilot study is a limited affair and ours yielded a shorter duration of drug use than was expected but did allow for other factors to be controlled. We were not blind to these problems (nor were the helpful referees) but felt content to pursue the debate as the issues are important. I agree that further enquiry is indicated but suspect that to obtain enough data several groups will need to collaborate.

K.A. Farrell

44 High Street
Repton
Derbyshire DE6 6GD

Lack of training in dermatology

Dr Perkins (Letters, January *Journal,* p.36) correctly points out that hospital outpatient waiting lists are too long and could be reduced by fewer general practitioner referrals. However, the suggestion that in dermatology a definitive report could be given on the basis of a Polaroid photograph with relevant clinical details, thereby avoiding a hospital consultation, is naive.

Undoubtedly a photograph would frequently aid diagnosis prior to consultation, merely reflecting the inadequacy of the clinical description in many referrals. However, few dermatologists would be happy to treat patients like a magazine quiz and the medicolegal implications of incorrect advice on the basis of such a system would be considerable. The analogy with a cardiologist's report on an electrocardiograph is unfortunate, as such a report rarely obviates a clinical consultation.

It is general practitioners' lack of confidence in dermatology, owing to inadequate training that is the fundamental problem. Despite the fact that 60% of medical students are destined to become general practitioners, when 6% of their consultations will concern skin problems,[1] undergraduate exposure to dermatology is extremely limited and shows a wide variation. I have recently surveyed the British universities' undergraduate dermatology curricula and found the mean time allocated is nine days with a range of five to 20 days. Emphasis on the need for postgraduate education in dermatology is often the excuse for the deficiencies of the undergraduate curricula. Unfortunately, few opportunities exist nationally for postgraduate education in dermatology and its extent is difficult to define in the absence of a coordinating body.

The training of the non-dermatologist in dermatology is highly whimsical and it is only by rectifying this that waiting lists for skin problems will be reduced and the service improved.

Andrew J. Carmichael

The Skin Hospital
35 George Road
Birmingham B15 1BR

Reference

1. Royal College of General Practitioners, Office of Population Censuses and Surveys and Department of Health and Social Security. *Morbidity statistics from general practice. Third national study, 1981–82.* London: HMSO, 1986.

Regional distribution of family practitioner services

Sir,
In a recent article Birch and Maynard (December *Journal,* p.537) state that 'the use of the RAWP formula (and the corresponding formulae in Northern Ireland, Scotland and Wales) has narrowed inequalities in hospital and community health services.'

Unfortunately, while the PARR formula has been calculated on an area board