menopausal bleeding from which the women workers suffered.

Finally, the criticism of the inappropriate use of the unpaired *t* test in the statistical analysis is groundless because, firstly, there is no obvious violation of the assumptions of the *t* test and, secondly, the robustness of the *t* statistic secures its use in comparing two groups.

We have no doubt therefore about the significance of our findings.

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¹ Poller, L, in *Recent Advances in Thrombosis*, ed L Poller, p 181. London, Churchill, 1973.

² Poller, L, *British Medical Bulletin*, 1978, **34**, 151.

Serum bilirubin and hepatic enzyme induction

SIR,—The recommendation of measurement of serum unconjugated bilirubin on a fasting specimen, as a means of detecting hepatic enzyme induction, by Dr A K Scott and his associates (4 August, p 310) at the moment has more academic than practical potential. At the reported levels of serum total bilirubin of 7 $\mu\text{mol/l}$ (0.4 mg/100 ml) or less the accuracy, precision, and sensitivity of fractionation procedures, in our hands as well as in those of others, are quite poor. The result of fractionation under these circumstances is in fact little better than guesswork, and accordingly interpretation of such results could be quite hazardous. For this reason I, like many other chemical pathologists, decline requests for fractionation of serum bilirubin which has a value well inside the normal range.

Dr Scott and his colleagues state that serum unconjugated bilirubin estimation is less time-consuming and cumbersome than other methods commonly used to assess hepatic enzyme induction. They quote antipyrine half life, urinary D-glucuric acid, and 6- β -hydroxycortisol excretion as examples of such methods. Serum γ -GT (γ -glutamyl-transferase, EC 2.3.2.2), however, has been omitted from their list. Elevation of serum levels of this enzyme as a result of hepatic enzyme induction by alcohol and by anticonvulsant and other drugs was reported in 1971.¹ In our laboratory as in many others, the practical usefulness of this test has since been confirmed; quite often an isolated raised level of this enzyme, among normal results for routine biochemical parameters, reflects the state of hepatic enzyme induction. This is a test which is now widely available in most if not all clinical biochemistry laboratories; in fact it is an automated test in our laboratory, available as part of our routine liver function tests. It is fairly simple and is sensitive enough to be recommended for monitoring of patients on anticonvulsant drugs. Like all other tests used for assessment of hepatic enzyme induction during anticonvulsant therapy, interpretation of results of serum levels of this enzyme should be made with caution in the presence of hepatobiliary disease, when "routine" liver function tests give abnormal results, and in cases of alcoholism, as well as in patients receiving other drugs capable of inducing hepatic enzymes.

In conclusion, until fractionation methods

with improved sensitivity and reliability become available for routine use, clinicians as well as chemical pathologists should not consider fractionation of serum bilirubin when concentrations are normal or low. Estimation of serum γ -GT is more practical and more meaningful, and should give more reliable information for monitoring hepatic enzyme induction.

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¹ Rosalki, S B, Tarlow, D, and Rau, D, *Lancet*, 1971, **2**, 376.

Volunteer and professional treatments of dysphasia after stroke

SIR,—We noted with interest the original article "Comparative trial of volunteer and professional treatment of dysphasia after stroke" by Margaret Meikle and others (14 July, p 87). There are several aspects of their study which require clarification.

The selection of volunteer helpers is described but it is not clear if their supervisors are trained speech therapists. The patients studied would appear to be younger than most stroke patients as only four of the 31 patients are aged 75 years or more and the average age is 65 years. There appears to have been a very late start to speech therapy in the trial as only six patients commenced therapy within six weeks of the onset of dysphasia and two-thirds did not enter the study until more than three months had elapsed. No mention is made of the presence of any articulatory abnormality complicating the dysphasia and this is a crucial consideration in assessing the prognosis of the dysphasic patient undergoing conventional speech therapy.¹

No description of the form of conventional speech therapy is attempted and the use of the six-week retest Porch Index of Communicative Ability (PICA) is not explained. The table on page 88 describes 17 patients as receiving conventional speech therapy, whereas only 16 patients are thus described in the "Results" section. Although the summary states that the impact of aphasic illness on families is examined, this is not described in the text and indeed no reference is made to the key role of the family in the rehabilitation of the dysphasic patient.

We would agree with the authors that they should be reluctant to draw unequivocal conclusions from their trial and we would advocate guarding against the wholesale promotion of volunteer schemes as an alternative to professional speech therapy until more convincing evidence is available.

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¹ Williams, B O, Walker, S A, and Dall, J L C, *Age and Aging*, 1977, **6**, 96.

* * * We sent a copy of this letter to the authors, whose reply is printed below.—ED, *BMJ*.

SIR,—We are glad to have the opportunity to clarify the points raised in the letter from Dr B O Williams and Mrs Sandra A Walker concerning our paper.

Our trial was not an attempt to examine the efficacy of speech therapy or to assess spontaneous recovery in the natural history of dysphasia. Any patient who would normally have been referred for speech therapy and who met the criteria for the trial was included, regardless of time since the onset of dysphasia. We wished to assess whether some speech therapists might use part of their time profitably in supervising and advising volunteers who would have more time available to devote to individual patients. Our experience from the trial was positive in this respect. In organising the trial suitable arrangements could not be made for volunteers to assist patients in hospital. We were restricted therefore to including only those patients living at home, and this may account for the younger average age of subjects.

In the assessment of linguistic ability the presence of dyspraxia was also recorded. We would agree that this is important in considering the prognosis of dysphasic patients. Severe articulatory dyspraxia presents a problem in rehabilitation to both professionals and volunteers, and we found that the volunteers required considerably more guidance in these cases.

The guidelines for treatment as indicated from the results of individual PICA assessments were carefully followed for all patients. This was outlined in the article, but as each patient had different problems details of specific materials and approaches could not be included. It should be pointed out that similar materials and methods were used on both sides of the trial. At the outset of the trial the intention was to retest at monthly intervals according to PICA guidelines, but as the trial progressed this became impractical because of the increasing number of subjects in the trial, and it was found that in most cases the interval had to be six weeks. Reassessment continued until plateauing occurred on two successive occasions, and again this was in accordance with PICA guidelines.

There is an error in the table on page 88. Subject 28, was in fact, treated by volunteers.

The vital importance of the impact of aphasic illness on families was fully described in the article to which reference was made.¹ It was not one of the aims of the trial to examine the role of the family in rehabilitation. Nor was it at any time our intention to promote volunteer schemes as an alternative to professional speech therapy. We would like to reiterate that our main aim was to find ways of providing more assistance in overcoming the communication problems experienced by dysphasic patients and their families.

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¹ Mulhall, D J, *British Journal of Disorders of Communication*, 1978, **13**, 127.

Cubital tunnel syndrome

SIR,—I should like to make some comments about your leading article "Cubital tunnel syndrome" (25 August, p 460).

The capacity of the cubital tunnel is diminished in elbow flexion owing to tautening of the arcuate ligament, which stretches