Average fall in rectal temperature (°C) in the first hour after birth. There are five subjects in each of the four cells in the table.

	With lamp	Without lamp	Total
With hat	0.48	0.90	0.69
Without hat	0.54	0.74	0.64
Total	0.51	0.82	0.66

are unlikely to be of any value in reducing heat loss immediately after birth, but that Gamgee-lined hats are effective.

We would like to acknowledge the practical assistance of Sister R Aitken in this work.

E C COLES
H B VALMAN

Clinical Research Centre and
Northwick Park Hospital,
Harrow, Middx HA1 3UJ

Kidney transplants and long-term immunosuppression

SIR,—The recent short report by Dr F Di Padova and others (18 August, p 421) regarding the necessity of long-term immunosuppressive therapy for the maintenance of good kidney graft function is perhaps not unexpected. It is correct to anticipate the emergence of specific immune tolerance to the graft in a small percentage of recipients.

There are many possible mechanisms for the production of immune tolerance.¹ Certainly, in animal experiments² the use of cyclophosphamide up to 72 hours before and 48 hours after first-time exposure of mice to sheep erythrocytes is followed by tolerance with respect to antibody response.

Such reports and studies suggest the need to consider investigation of the residual capacity of a recipient for recognition of donor tissue at an arbitrary time following grafting. This should involve at least a mixed leucocyte culture analysis using washed host cells taken during minimum immunosuppressive therapy in combination with stored donor cells, employing stored allogeneic lymphocytes in addition to non-related antigens (for example, tuberculin and candidin) as controls in order to estimate the relative extent of specific and non-specific total immunosuppression.

After such procedures had been carried out it would be possible to consider the case for a brief cessation of immunosuppressive therapy on more rational grounds according to the results.

BLEDDYN JONES

London SE3

¹ Voisin, G. A., *Progress in Allergy*, 1971, **15**, 348.

² Aisenberg, A. C., *Journal of Experimental Medicine*, 1966, **125**, 833.

Reactions to whooping cough vaccine

SIR,—A recent study by Professor G T Stewart purports to show an association between certain reactions to pertussis vaccine and subsequent severe brain damage. Because of the design of his study, however, his results may well be misleading. Not only do his methods of case finding and of selection for further study create risk of bias, but he asks the wrong questions.

The question which a parent or a doctor

wants answering is not "Is the relationship in time between immunisation and convulsions greater than would occur by chance?" It is "Will this child ultimately have a greater risk of significant disability if I vaccinate or if I do not vaccinate?" This question can be answered only by comparing the incidence of brain damage and disability at some suitable age (say 10 years) in a group of children with specified "contraindications" who were not given pertussis vaccine with that observed in a similar group having the identical "contraindications" who were vaccinated despite them.

Many of the accepted criteria for *not* vaccinating are indications of possible existing brain damage or proneness to convulsions. The child who is prone to convulsions may be provoked into a series of fits by a variety of stimuli, including infection and pyrexia as well as immunisations. Professor Stewart's test was thus inappropriate, being almost certain to show a greater-than-chance association for second convulsions after the second doses. This, however, does not prove that the ultimate brain damage would not still have occurred following "natural" febrile convulsions had the child not been vaccinated, or that the risk of brain damage due to pertussis years later might not be even greater.

J S ROBERTSON

Scunthorpe Health District,
Scunthorpe, Lincs DN15 8DT

¹ Stewart, G T., *Journal of Epidemiology and Community Health*, 1979, **33**, 150.

Rubella vaccination

SIR,—The question of abandoning the sero-testing of adult women before rubella immunisation, which was dismissed by Dr R G H Bethel (25 August, p 501), is worthy of serious consideration in family planning clinics.

There are several problems involved in providing serotesting—the need for extra staff to take the blood samples, the need for extra resources to pay for the service, and the low acceptability of blood taking.¹ Mayon-White and Bull found only 50% acceptance for serotesting in family planning clinics, with only 50% of the seronegative women immunised (failure to reattend, left the area, etc)—that is, only 25% of seronegatives were converted. These considerations mean that a programme involving serotesting would only increase the group immunity from 80% to 82.5%, at a cost of approximately £20 per conversion.

Clearly none of these difficulties exist if immunisation is offered without serotesting. Assuming an acceptability of 80%, the group immunity would rise from 80% to 88.3%, at a cost of £2.40 per conversion. However, the cost of this improved effectiveness and efficiency is a rise in the number of terminations of pregnancy that would result. This could be minimised by offering immunisation without testing only to established oral contraceptive users, in whom the family planning doctor was reasonably confident of reliable use.

In Oxfordshire Area Health Authority (Teaching) it is estimated such a programme would result in two to three additional terminations a year (in regular oral contraceptive users who would probably request a termination anyway). This figure should be

considered in the light of the 13 planned pregnancies that were terminated in 1978 for contact with natural infection.

The idea of immunising without sero-testing is not without medicolegal difficulties, but it is a service that should at least be considered and debated.

NICHOLAS BLACK

Department of Community Medicine,
Oxford Area Health Authority
(Teaching),
Oxford OX3 9DZ

¹ Mayon-White, R T, and Bull, M J V, *Practitioner*, 1976, **216**, 317.

SIR,—I refer to Dr R G H Bethel's letter (25 August, p 501) in reply to my letter questioning the necessity of laboratory testing before immunisation. My anxiety is to ensure that all women in the reproductive age group receive the vaccine. No question of cost arises. I believe that the present necessity of ensuring tests other than for 12- and 13-year-olds puts difficulties in the way of bringing about this idea. It is accepted that all reasonable precautions should be taken to ensure that the woman is not pregnant at the time of inoculation and for the first three or four months thereafter.

Community levels of immunity can be determined by periodic surveys. Dr Bethel comes part of my way in the argument by suggesting the compromise of taking and storing blood at the time of immunisation and testing it only if subsequently it is discovered that inadvertently the woman was pregnant. Thereafter therapeutic abortion may be advised. This method would also allow for a 100% uptake, our joint aim.

The problem appears to be that some doubt exists about whether or not the vaccine is teratogenic.

T S WILSON

Glasgow G72

Effects of manufacturing oral contraceptives on blood clotting

SIR,—The three objections to our paper (30 June, p 1761) raised by Drs T W Meade and S G Thompson in their letter (25 August, p 493) on the effects of oral contraceptive manufacture on blood clotting can be refuted. These refer to the small numbers, the possible effect of diabetes in one subject, and the statistical analysis.

A point which may not have been sufficiently explicit in our paper was that, although the number of subjects who suffered high exposure was small, the many replicate tests performed allowed valid statistical analysis and produced highly significant results.

The clotting changes in this highly exposed group were marked and typical of the pattern which has now been established following long-term studies with oestrogen-progestogen combinations given by mouth as oral contraceptives.^{1,2} Such changes have not been found in the long-term assessment of controls or patients with uncomplicated diabetes. The typical "pill pattern" changes found in this group in factors VII and X, fibrinogen, and thrombelastography leave little doubt of the valid comparison between exposure during manufacture and oral ingestion of oestrogen-progestogen contraceptives. The only difference in the present study is the rise in platelet count, which we believe was due to the post-

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.

L POLLER
JEAN M THOMSON
K F YEE

National (UK) Reference Laboratory
for Anticoagulant Reagents and
Control,
Withington Hospital,
Manchester M20 8LR

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.

B F ALLAM

Department of Biochemistry,
Stobhill General Hospital,
Glasgow G21 3UW

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.

BRIAN O WILLIAMS
SANDRA A WALKER

Victoria Geriatric Unit,
Victoria Infirmary,
Glasgow G41 3DX

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.

MARGARET S MEIKLE

Northwick Park Hospital,
Harrow, Middx HA1 3UJ

ENID WECHSLER

University College Hospital,
London WC1E 6AU

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