cheques. If one were placed at the front of every cheque book issued to passengers going to malaria-endemic areas the user would be constantly reminded of the disease. I am told the banks are indifferent to this idea. Maybe some future intimate contact between *Plasmodium falciparum* and a high-ranking banker might get them to change their minds. The sheet is also issued to agents and should be attached to every ticket involving an endemic area.

If everyone concerned with travel would get together, health risks for travellers could be greatly reduced. Doctors are now much more on the look-out for imported disease, especially malaria. Aircraft operators are helping. Why not the travel agents? Is it basically a commercial matter? One agent told me that he thought emphasis on health risks would frighten people away. I believe a frightened passenger is better than a dead one.

BRIAN MAEGRAITH

Liverpool

¹ Maegraith, B G, *Lancet*, 1963, 1, 401.

² *Transactions of the Royal Society of Tropical Medicine and Hygiene*, 1970, 64, 199.

³ Graves Medical Audiovisual Library, 1977, Tape 77.94.

⁴ World Health Organisation, *Weekly Epidemiological Record*, Geneva, WHO.

⁵ *ABC Guide to International Travel*. Dunstable, Beds. ABC Travel Guides Ltd. (issued quarterly).

⁶ Hall, A P, *British Medical Journal*, 1978, 2, 877.

⁷ Turner, A C, *The Traveller's Health Guide*, London, Tom Stacey Ltd, 1971.

Withdrawal of pheneturide

SIR,—The antiepileptic drug pheneturide (Benuride) is being withdrawn from sale and supply in the UK. In our opinion this is a serious loss to the neurologists' armoury, since we have found it to be a useful drug in certain cases of refractory epilepsy.

We have selected 20 patients who are going to be carefully followed during their period of restabilisation on alternative medication, but would like to study a larger group. We should therefore urgently like to hear from any physician who is similarly constrained to withdraw a patient from pheneturide, particularly with reference to the efficacy of the treatment substituted for it.

P K P HARVEY

Royal Northern Hospital,
London N7

R GOULDING
D J BERRY*

Poisons Unit,
New Cross Hospital,
London SE14 5ER

*For correspondence.

Whooping cough after stopping pertussis immunisation

SIR,—Was the pattern of whooping cough in Dr Robert Ditchburn's Shetland practice (16 June, p 1601; 21 July, p 207), with its high proportion of older children, "unusual"? In late August 1977 there was a sharp increase in whooping cough notifications in a Sheffield housing estate. Thirty-nine cases were reported in just over three months, more than half the total for the whole city. The first case was a baby of 4 months. During the next three weeks there were 22 further cases, 20 in children between 5 and 9 years and 13 of them girls.

The 16 cases in the remaining two months consisted of eight between 1 and 4 years, three schoolchildren, one adult, three with age not stated but probably adults, and one baby of 11 months. There were thus two phases, the first involving school children, mainly girls, who must have been infected during the summer holidays and the second involving pre-school children and adults.

This kind of outbreak suggests that, depending on epidemiological circumstances, there is more than one common mode of whooping cough spread, including child-child transmissions within and between different age ranks and child-adult and adult-child transmissions. The one supposedly important mode which is almost entirely missing is the infection of babies by older siblings. In Sheffield in 1977-8, of 24 notifications in babies under one year, 17 were the only cases in the household and five were either the first case or one of several appearing at the same time. Not infrequently a case in a baby was the first notification in a previously unaffected part of the city. How important is the infection of babies by asymptomatic or misdiagnosed adults? Linnemann's paper¹ describing this aspect of whooping cough control in a children's hospital is worth attention.

P E BROWN

Department of Community Medicine,
University of Sheffield Medical School,
Sheffield S10 2RX

¹ Linnemann, C C, *et al*, *Lancet*, 1975, 2, 540.

Value of cytology for detecting endometrial abnormalities

SIR,—I regret that Dr Dulcie Coleman and her colleagues have criticised the conclusions in our paper "Value of cytology for detecting endometrial abnormalities in climacteric women receiving hormone replacement therapy" (31 March, p 846) and find the study ill designed.

By reviewing blind the cytological and histological endometrial samples from 110 postmenopausal women receiving hormone replacement therapy we could not confirm the claim of Hutton *et al*¹ that such sampling might be useful in monitoring these patients. This hopeful hypothesis had been made without reference in their study to any such patient receiving oestrogen therapy. In contrast, in our large series cytology was not only unable to distinguish proliferative and secretory endometrium but missed all three cases of hyperplasia and two out of three cases of carcinoma—although it is true that one cytologist reported "Suspicious—repeat" in the other case of carcinoma.

We are forced to conclude that, whatever the use of the Isaacs cell sampler may be in general gynaecology, it does not appear to have a place in monitoring the subtle endometrial changes which take place with oestrogen-progestogen therapy—nor can it be used to assess the reversal of hyperplasia by progestogen. Indeed, such differentiation is difficult enough by histology.

It is churlish of Dr Coleman and her colleagues to impugn the ability of senior cytologists at King's College Hospital and the Women's Hospital, Birmingham. The St Mary's team may believe that they have a unique ability to interpret endometrial cytology in postmenopausal women, but until they

publish their own results on appropriate patients or are supported by the findings of other experienced cytologists their claim must remain as flimsy as the Emperor's new clothes.

JOHN STUDD

King's College Hospital,
London SE5 9RS

¹ Hutton, J D, *et al*, *British Medical Journal*, 1978, 1, 947.

Measles and vaccine protection

SIR,—Dr G A Jackson's letter (4 August, p 332) raises doubts about the effectiveness of measles vaccination which at first sight question the value of the whole programme. However, he is probably right to make no general conclusion of this sort. Apart from the smallness of the numbers involved, if the protection afforded by inoculation was 90%, and the percentage of susceptibles in the unvaccinated population 50%, the proportion of vaccinated subjects expected in the sample would be 15%; the proportion of children notified in the sample who were reported as having been vaccinated was 30%. This difference is not significant.

However, though we do not know further details of the study there are several obvious occasions when bias may have crept in. On the assumption that about half of the number of cases of measles are notified, doctors would be more likely to remember and notify those whose parents had protested at the diagnosis because their children had been vaccinated. But a much larger error may have resulted from the unreliability of parents' memory; unless particular precautions were taken—for instance, by checking reports of vaccination against records held at the authority—parents are likely to be overoptimistic in replying in the questionnaire and unlikely to separate measles vaccination from other procedures.

The results of Dr Jackson's further studies may elucidate these and other uncertainties. Without more substantial evidence to the contrary, it is wise to accept Dr Clarke's advice (13 August 1977, p 455) that measles vaccine, before reconstitution, withstands journeys through the post without loss of potency, and Dr Christine Miller's (11 June 1977, p 1532) that the protection given in the trial arranged through the Medical Research Council was of the order of 90%.

A S HARRIS

West Sussex Area Health Authority,
Worthing BN12 4NQ

Disinfection with glutaraldehyde

SIR,—In the debate about the use of glutaraldehyde disinfecting solutions in British hospitals (14 April, p 1019, and 26 May, p 1425) it was mentioned that the expiry of the original patent from Ethicon covering the alkaline glutaraldehyde Cidex opened the door to many other imitations, which of course were only duplicates or veiled copies of the Cidex formula. This is indeed true for alkaline-buffered solutions but not for the non-ionic potentiated acid glutaraldehyde formulas, which were discovered in the USA around 1969-71.

The role of pH in the sporicidal mechanisms of dialdehydes was first suggested by Last

*et al.*¹ More recently, I reviewed² in detail the practical consequences of these pH-dependent mechanisms on the stability and efficacy of glutaraldehyde hospital formulas. This study clearly showed that there were only two fundamental types of glutaraldehyde compositions: those described in the alkaline range by the Ethicon patent (expired in January 1978) and those described in the acid and neutral range by the Wave Energy Systems patents. Under normal hospital use the alkaline compositions have a short life (maximum 14 days) and their only theoretical advantage is a slightly faster sporocidal action at room temperature. No substantial cidal advantage over the acid formula has ever been found when dealing with non-sporulated bacteria, viruses, and mycobacteria. Unlike alkaline compositions, potentiated acid glutaraldehyde solutions such as Sonacide have a slow rate of polymerisation, which corresponds to a continuous use life in hospitals of 28 days. Thanks to this extraordinary stability Sonacide decreases considerably the cost of any hospital disinfecting procedure, as recently demonstrated by Ayerst Laboratories in a two-year study covering 32 North American hospitals.³

Tarnishing and superficial metal erosion can sometime occur with glutaraldehyde disinfecting solutions. As your correspondent (26 May, p 1425) noticed, the higher the pH the milder is the potential tarnishing problem in the 6-8 pH range. One should, however, recall that potential tarnishing occurs mainly with scratched, worn-out, or poorly plated instruments which have been overexposed to disinfectant solutions. Let us recall, for instance, that five years of nationwide use of Sonacide (pH 4-6.2) in the USA never brought to light any major tarnishing problem when the product was used according to the recommendations of the Environmental Protection Agency label.

The recent discovery⁴ of the biocidal synergistic effect of ultrasonics on acid potentiated glutaraldehyde is another indication of the promising future of dialdehyde disinfecting techniques in hospitals.

R M G BOUCHER

Research Department
Wave Energy Systems Inc,
New York,
NY 10021, USA

¹ Last, A J, Smith, D K, and Boucher, R M G, *Proceedings of the Western Pharmacology Society*, 1973, 16, 282.

² Boucher, R M G, *Respiratory Care*, 1978, 23, 1063.

³ Lin, K S, *et al*, *Respiratory Care*, 1979, 24, 321.

⁴ Boucher, R M G, *Canadian Journal of Pharmaceutical Sciences*, 1979, 14, 1.

Abnormal cilia

SIR,—We were pleased to see your leading article on abnormal cilia (23 June, p 1663) and would like to draw attention to another area where ciliary abnormalities have been reported, which may be of considerable medical significance.

We have described¹ abnormalities in nasal cilia in patients with Usher's syndrome and other forms of retinitis pigmentosa. In life direct examination of the modified cilia of the ear and eye is not feasible, but there is the possibility that they share abnormalities with other ciliated epithelia which can be biopsied. Thus new forms of investigation are opened into conditions of progressive loss of hearing and vision.

The last paragraph of your leader advises caution in the interpretation of electron microscope studies of cilia, with which we wholeheartedly agree; but we would like to point out that the electron-microscopical appearances of abnormal microtubular patterns are so striking that photographic enhancement techniques are unnecessary. More to the point, it is necessary to investigate large samples of material in many patients and controls because of the presence of abnormal microtubular patterns in a proportion of cilia in people who have no disease.

The interest of cell biologists has been aroused by these new reports. In Kartagener's syndrome the clinical observations provided the final confirmation of the essential role of dynein arms in ciliary immotility²; it may be that the asymmetries recently found in human disease will provide further clues to even more profound biophysical puzzles.

G B ARDEN

Department of Visual Science,
Institute of Ophthalmology,
London WC1H 9QS

B FOX

Department of Histopathology,
Charing Cross Hospital
Medical School,
London W6 8RF

¹ Arden, G B, and Fox, B, *Nature*, 1979, 279, 534.

² Fawcett, D W, *New England Journal of Medicine*, 1977, 297, 46.

Diabetic (insulin) oedema

SIR,—A short report on insulin oedema (21 July, p 177) affirmed that oedema of this type is rare in diabetics. We have recently described four patients (three male and one female aged 18-21 years) with typical acute diabetic oedema¹⁻³ and agree that the severe fluid retention which transiently follows the restoration of good diabetic control is not common. These episodes of oedema were associated with insulin treatment of diabetes, but, in contrast to the case reported by Mr N R Bleach and others, one patient (a boy of 12) had been on insulin for two years. All these episodes of acute oedema were not only transient but settled spontaneously *without diuretic therapy*, which should probably be eschewed in such patients.

We would like to draw particular attention, however, to subacute and chronic forms of oedema occurring in diabetics. This concept was first raised in a presentation to the British Diabetic Association in September 1977 and has been elaborated on elsewhere.^{2,3} Over the past few years we have documented 12 diabetics, all female, who suffered from severe generalised fluid retention lasting from a few months to several years. Two of these patients were not treated with insulin and all were observed to have fluid retention with the distribution described by Mr Bleach and his colleagues while on no diuretic therapy. (This latter point is of particular importance in the light of a recent report⁴ attributing unexplained oedema to diuretics.) Furthermore, in a survey of 86 insulin-requiring patients under 50 years attending a diabetic clinic, a history of intermittent mild-to-moderate fluid retention without obvious cause was obtained in 14 of 40 (27%) female patients. Only one of 46 male patients gave such a history. Subacute and chronic forms of diabetic oedema are therefore not uncommon and this diagnosis should be considered in female diabetics with

unexplained fluid retention. Clinically such diabetic oedema resembles the syndrome of idiopathic oedema of women where there is a high familial incidence of diabetes.^{2,5,6}

With regard to the aetiology of diabetic oedema, other possibilities coexist with the mechanisms mentioned by Mr Bleach and his colleagues. Acute diabetic oedema has been considered to resemble "refeeding" oedema⁷ and thus to represent a pathological degree of carbohydrate-induced antinatriuresis.^{8,9} An abnormal degree of sodium retention, not always manifest as oedema, occurs not only in carbohydrate refeeding but also follows the treatment of ketoacidosis in man¹⁰ and animals.¹¹

J R LAWRENCE
M G DUNNIGAN

Department of Endocrinology and
Metabolism,
Stobhill General Hospital,
Glasgow G21 3UW

¹ Craig, O, *Childhood Diabetes and its Management*, p 224. London, Butterworths, 1977.

² Lawrence, J R, MD thesis. University of Glasgow, 1978.

³ Dunnigan, M G, *Practitioner*, 1979, 222, 321.

⁴ McGregor, G A, *et al*, *Lancet*, 1979, 1, 393.

⁵ Sims, E A H, Mackay, B R, and Shirai, T, *Annals of Internal Medicine*, 1965, 63, 972.

⁶ Shaw, R A, *American Journal of Cardiology*, 1968, 21, 115.

⁷ Marble, A, *et al*, (editors), *Joslin's Diabetes Mellitus*, 11th ed, p 396. Philadelphia, Lea and Febiger, 1973.

⁸ Bloom, W L, *Archives of Internal Medicine*, 1962, 109, 26.

⁹ Wright, H F, Gunn, D S, and Albertsen, K, *Metabolism*, 1963, 12, 804.

¹⁰ Saudek, C D, *et al*, *Diabetes*, 1974, 23, 240.

¹¹ Blumenthal, S A, *Diabetes*, 1975, 24, 645.

Royal Commission report

SIR,—Your leading article on the report of the Royal Commission on the National Health Service (28 July, p 227) waxed lyrical about the preventive content of the proposals. As a trainee in community medicine I beg to differ.

Certainly the commitment is there (paragraph 5.1): "... we regret that more emphasis has not been placed in the past on the preventive role of the NHS. This must change if there are to be substantial improvements in health in the future. . . ." But what "change" is recommended? The answer is very little. Of the six preventive recommendations, four relate to health education (one of the least effective means of prevention), one (on screening) has preoccupied the DHSS for years, and the other (on seat belts) is no stranger to the House of Commons, which may finally legislate on the subject this session.

The most frustrating feature of the report is the total failure of the commissioners to harness their preventive philosophy (which is admirable in conception if not in application) to an existing organisational framework of implementation—namely, community medicine. For prevention is the *raison d'être* of community medicine, a point which may not have been sufficiently impressed on the commissioners by the representatives of the specialty when evidence was submitted. The commissioners' pious declaration that "We ourselves believe that the specialty has a future" (paragraph 14.55) carries little conviction when no attempt is made to spell out that future. Indeed, the only recommendation on community medicine, that community physicians should be given adequate supporting staff, merely begs the question "Adequate for what purpose?"

The commissioners have lost an unprece-