geriatric medicine at Northwick Park Hospital, Harrow, where a CAT whole-body scanner has been available for some years now, I reviewed all admission to the unit over a seven-year period to see what differences in presentation existed between patients with intracranial neoplasm and stroke. This formed part of a review of the experience of this acute geriatric unit in the presentation and clinical course of brain tumours in the elderly.¹

Twenty per cent of all admissions to the unit were diagnosed as having a vascular stroke, as defined elsewhere.^{2,3} This represents over 1000 patients. Only four of these were subsequently found to have a brain tumour—that is, 0.4% of all strokes. During the seven-year period 30 symptomatic brain tumours were confirmed, but the majority of these were correctly diagnosed in life, the exceptions being the four patients already referred to. I would submit that the correct diagnosis is nearly always suggested by the history. Progressive mental and/or focal neurological symptoms are the two most significant features in the elderly, while headache or epileptic seizures are far less common than is the case with younger patients.

Finally, whereas over 1000 strokes and 30 symptomatic brain tumours (primary or metastatic) occurred during the study period, it is remarkable that there were only four patients in whom a diagnosis of subdural haematoma was established despite the large number of patients in whom this diagnosis was considered. Many elderly people fall; fortunately, it appears that very few of them suffer subdural haematoma as a result.

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¹ Twomey, C, *Age and Ageing*. In press.

² Geriatrics Committee, Working Group on Strokes. Royal College of Physicians, London, July, 1974.

³ Capildeo, R, Heberman, S, and Rose, F C, *British Medical Journal*, 1977, 2, 1578.

Organ transplantation and the fetal allograft

SIR,—Immunological studies may lead to the heart of a problem, but preceding clinical and experimental experience is not thereby rendered obsolete or irrelevant. Indeed it may assist immunologists.

Long ago it was shown that sensitivity could be abolished in a patient with tuberculosis by repeated small injections of tuberculin¹ and this did not diminish resistance to the disease. Similarly, the immunity conferred by BCG does not depend on the degree of tuberculin sensitivity induced in the individual.² The tuberculous mother becomes tuberculin negative during pregnancy,³ but resistance to tuberculosis is not diminished. For example, Cohen⁴ reported on 715 pregnant tuberculous women confined in a maternity unit for such patients and found that only 51 (7.15%) showed any evidence of regression, a figure no more and possibly less than would have been expected in a comparable group of non-pregnant tuberculous women. This unit was started in 1937 at Black Notley Hospital, Essex, and most of these women had no benefit from chemotherapy. Long-term follow-up of tuberculous women has shown that the effects of pregnancy have been beneficent and lasting.⁵ So also is the effect of pregnancy

at an early age on the later incidence of breast cancer.⁶

Therefore the letter from Mr J M Maxwell Anderson (29 July, p 355) may offer an analogy derived from the tenfold increase of α_2 -glycoprotein during pregnancy. There is a tenfold increase of oestriol in pregnancy which is associated with the immunological inertia of pregnancy. This inhibits the action of oestradiol. Are we therefore being offered an immunological explanation of a preceding valid clinical experience? Allergy was always a dirty word to those treating tuberculosis.

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¹ Rich, A R, *The Pathogenesis of Tuberculosis*, 2nd edn, p 523. Springfield, Thames, 1951.

² P D'Arcy Hart, P d'A, Sutherland, I, and Thomas, J, *Tubercle*, 1967, 48, 201.

³ Scadding, J G, *Tubercle*, 1956, 6, 371.

⁴ Cohen, R C, in *British Obstetric and Gynaecological Practice*, ed A Clayl and A Bourne, p 537. London, Heinemann, 1963.

⁵ Lyman, D R, *Yale Journal of Biology and Medicine*, 1943, 15, 464.

⁶ Lemon, H M, *Cancer*, 1970, 25, 423.

Sjögren's syndrome, bromhexine, and tear secretion

SIR,—Dr K Frost-Larsen and others have presented the results of a clinical study (17 June, p 1579) from which they claim that bromhexine is valuable in the treatment of Sjögren's syndrome.

Sjögren's syndrome is, by definition, a condition in which the principal components are keratoconjunctivitis sicca, arthritis, and dry mouth. Only three of their 29 patients satisfied this definition. The only criterion for admission of the patients to the trial was a Schirmer test result of less than 10 mm in both eyes on two occasions. We note that their test was performed with the patients' eyes closed, contrary to the method described in 1903 by Schirmer and later by Duke-Elder.¹ Duke-Elder states that the Schirmer test is a "rough and ready test, without any pretence to accurate assessment, which only provides an indication of excessive lacrimation, marked hyposcretion, or an absence of tears." Pinschmidt² performed Schirmer tests on the normal eyes of his laboratory staff and found inconsistent and abnormal results which varied on retesting. Bijsterveld³ and Lachman and Hazleman⁴ also found the Schirmer test to give a poor indication of tear flow. The conclusion drawn by Dr Frost-Larsen and his colleagues that bromhexine stimulates lacrimal secretion is invalid because their Schirmer test readings were not suitable for statistical analysis. Lacrimal gland function is more accurately measured by the estimation of lysozyme in the tears⁵ and we have described a method of quantitative tear lysozyme assay.⁶

The authors appear to place emphasis on the effect of bromhexine on the break-up time of the tear film, but it has been shown recently that the tear break-up time is a misleading test.⁷ If anything, it is a test of the integrity of the microvillous system of the corneal epithelium which can be disordered by diseases which mimic keratoconjunctivitis sicca.

We were interested to read that some patients' symptoms of dryness resolved while they were on bromhexine. However, our experience has taught us that a patient's complaints of "dry eyes" cannot be considered

diagnostic as the concept of dryness is often conveyed to the patient at a previous consultation. We cannot understand why bromhexine should stimulate lacrimal secretion. Wright⁸ suggested that the drug was capable of influencing mucus production by the conjunctival goblet cells.

Notwithstanding our remarks we will be interested to investigate further this line of treatment because anything which mitigates the lot of the patient with keratoconjunctivitis sicca is very much worth while.

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¹ Duke-Elder, S, in *A System of Ophthalmology*, ed S Duke-Elder, vol 8, p 132. London, Kimpton, 1965.

² Pinschmidt, N W, *Southern Medical Journal*, 1970, 63, 1256.

³ van Bijsterveld, O P, *Ophthalmologica*, 1973, 167, 429.

⁴ Lachmann, S, and Hazleman, B, *Hospital Update*, 1975, 1, 617.

⁵ Mackie, I A, and Seal, D V, *British Journal of Ophthalmology*, 1976, 60, 70.

⁶ Mackie, I A, and Seal, D V, *British Journal of Ophthalmology*, 1977, 61, 354.

⁷ Vanley, G T, Leopold, L M, and Gregg, P, *Archives of Ophthalmology*, 1977, 95, 445.

⁸ Wright, P, *Transactions of the Ophthalmological Society of the United Kingdom*, 1975, 95, 260.

Rape and the laboratory: blood-grouping of hair

SIR,—Following the review of the laboratory examinations available for investigating allegations of rape in your leading article (15 July, p 154) may I draw your attention to recently published work which will significantly increase the evidential value of human hair examination in such cases?

The microscopic comparison of hairs from victim and alleged assailant is unlikely to be of significant evidential value unless there are gross dissimilarities between the hairs with respect to colour and structure, when it may be possible to make an exclusion on such grounds. Certainly, as you point out, a positive identity match is impossible. However, a feature of living hairs which have been forcibly removed from the scalp, such as may happen during the fracas accompanying a sexual assault, is the presence of sheath cells associated with the root of the hair. Indeed, the presence of such cells is often cited as evidence of a hair having been snatched or tugged rapidly from the scalp.^{1,2} We³ and independent workers in Japan^{4,5} have shown recently that such cells provide the basis for the blood-grouping of hair using the genetic variants associated with enzymes such as phosphoglucosyltransferase (PGM), 6-phosphogluconate dehydrogenase (6PGD), and esterase-D (EsD). These variants are easily demonstrated using conventional starch-gel electrophoresis on a single hair root.³ PGM is especially valuable as it divides the population into three common phenotypes, PGM 1, 2-1, and 2, with frequencies of 58%, 36%, and 5.5% respectively. In addition, it is possible to carry out the electrophoretic examination either before or after microscopy without loss of root morphology or other microscopic characteristics of the hair. The PGM variants may also be detected in pubic and chest hairs forcibly removed.

The blood-grouping of hair based on the ABO characteristics associated with the

hair has been advocated by a number of authors⁶ but is not a procedure which has been widely accepted in the United Kingdom. However, the use of enzyme variants employing a simple technique on single hairs possesses certain advantages over ABO grouping and it promises to enhance significantly the value of hair examination in corroborating, or disproving by exclusion, allegations of rape in which the circumstances of the crime have led to the forcible removal of hair by one or other of the participants. Such methods are already developed in the United Kingdom to the stage where they have been adopted into routine casework and produced in court as evidence.⁷

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¹ Polson, C J, *The Essentials of Forensic Medicine*, p 70. London, Arnold, 1926

² Moenns, A A, et al, *Scientific Evidence in Criminal Cases*, p 361. New York, Foundation Press, 1973.

³ Twibell, J, and Whitehead, P H, *Journal of Forensic Sciences*, 1978, **23**, 356.

⁴ Oya, M, et al, *Forensic Science*, 1978, **11**, 135.

⁵ Yoshida, H, et al, *Reports of the National Institute of Police Science (Japan)*, 1978, **31**, 7.

⁶ Gramer, L, and Tausch, D, *Zeitschrift für Rechtsmedizin*, 1973, **72**, 63.

⁷ Fereday, L, personal communication.

Iodine and acetone-containing plastic spray dressings

SIR,—We would like to draw attention to the hazard of using Op-Site, an acetone-containing plastic spray dressing, in conjunction with tincture of iodine (weak iodine solution BP).

Following the substitution of an acetone-containing spray dressing for a non-acetone spray blistering of the skin occurred on the backs of two patients who had epidural analgesia during labour. The skin had been prepared with tincture of iodine. A gauze swab, used to prevent kinking of the catheter at the skin edge, had been held in place with the plastic spray while the dressing towels were removed. The gauze and catheter were then secured with Sleek waterproof plaster. Blistering occurred only where the iodine had been covered by both the spray dressing and Sleek.

Patch tests were performed on the ventral surface of the forearms of seven volunteers. No reaction was observed to the plastic spray dressing covered by Sleek. Erythema occurred in all areas painted with three coats of tincture of iodine and covered by Sleek. One subject had three minute blisters. More severe reactions occurred in areas treated with both tincture of iodine and the plastic spray dressing and covered with Sleek. In three subjects, including the one who produced blisters with tincture of iodine and Sleek alone, severe blistering occurred.

Although it is well known that an irritant compound is formed by the interaction of iodine and acetone in spirit,¹ the reaction between iodine and acetone in plastic spray dressing has not, to our knowledge, been reported.

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¹ Martindale: *The Extra Pharmacopoeia*, ed A Wade, 27th edn, p 825. London, Pharmaceutical Press, 1977.

Glycolysated haemoglobin and diabetic control

SIR,—We read with great interest your leading article (27 May, p 1373) suggesting that Hb A_{1c} might be used as an index of long-term blood sugar control, and thus the metabolic abnormalities, of diabetes mellitus.

Hb A_{1c} is of course a glycoprotein, as are most of the blood proteins, and the time course of its synthesis and degradation are such as to reflect the long-term changes in blood sugar levels. However, this also applies to other blood glycoproteins. Two years ago we published¹ the results of our retrospective investigations into the relationships between five other blood glycoproteins and diabetic control and complications in 145 patients. In essence we found the same relationship between blood sugar control and the levels of these circulating glycoproteins and a similar relationship to plasma lipid levels as has been described for Hb A_{1c}.

While we agree that Hb A_{1c} is unlikely to be harmful in itself to diabetic patients, our results suggest that other glycoproteins might play a greater part in the pathogenesis of diabetic complications—for example, fibrinogen, α₂-macroglobulin, etc.

It is our feeling that there is a general increase in glycoprotein levels as a part of the abnormal metabolic processes found in diabetes which is best observed in the levels of those glycoproteins with highest hexose content. Whether these increases are causative or just associated with the development of diabetic complications is not clear.

We agree with you that serial measurements of Hb A_{1c}, and perhaps other glycoproteins such as fibrinogen or haptoglobin, might have great potential in monitoring long-term diabetic control, but prospective studies are clearly required before their value can be fully ascertained.

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¹ Jonsson, A, and Wales, J K, *Diabetologia*, 1976, **12**, 245.

Expectations of a pregnant woman

SIR,—As a general practitioner obstetrician I should like to take issue with two aspects of the discussion (15 July, p 188) on the expectations of a pregnant woman in relation to her treatment.

Firstly, item (2) of the National Childbirth Trust's list states: "It would be assumed that she would go into labour normally unless it was considered to be unsafe for her baby or her." This presupposes that few women will wish labour to be induced near term if given the option, a fact which experience shows to be untrue. I find that given a free and informed choice many women opt for induction by amniotomy and oral prostaglandins between 40 and 41 weeks gestation rather than an at times stressful and frustrating wait for the spontaneous onset of labour.

Secondly, it is stated by Professor Richard Beard that "there is an almost insuperable administrative problem in ensuring that the same doctor... who is seeing the patient in

the antenatal period is available when she comes in in labour." With efficient organisation, the support of his partners, and the encouragement of his consultant colleagues it is perfectly feasible for the GP obstetrician to be available for the vast majority of the patients in labour booked under his care, and in our own practice I have managed to be present during labour for more than 90% of our patients booked into GP beds within a specialist unit.

Dr Geoffrey Marsh¹ has shown that where a GP conducts a large proportion of the obstetrics of the practice there is a reduction rather than an increase in the perinatal morbidity compared with the area rate. It is regrettable both for women in labour and for general practice that more GPs are not following his example.

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¹ Marsh, G N, *British Medical Journal*, 1977, **2**, 1004.

Relapse in acute lymphoblastic leukaemia

SIR,—We read with interest the article by Drs M A Cornblett and Judith M Chessells (8 July, p 104) on children with acute lymphoblastic leukaemia (ALL) relapsing both on and off treatment.

We have been looking at all children in nine major centres in the UK who have relapsed in their bone marrow while receiving combination chemotherapy. We confined the study to children who had received standard induction therapy, central nervous system prophylaxis, and combination chemotherapy for two years or more. The median duration of survival of 190 children was 157 days and there were no survivors beyond just over two years from the time of relapse (see figure). Included in the 190 children were the 37 children in group A of Drs Cornblett and Chessells. Ninety-three children achieved a second remission; the median duration of haematological remission was 97 days and the median duration of complete remission 80 days. We have no cures among this group of children and can confirm the very short expectancy of life for children with modern therapy who relapse while receiving treatment.

An improved response to combinations with asparaginase was noted in another of the centres reviewed besides Great Ormond Street, and the importance of asparaginase in refractory ALL has been reported by other

Life table showing duration of survival from relapse for children with ALL relapsing on treatment.