their plantar fasciitis was secondary? It is well known that the peripheral manifestations of ankylosing spondylitis often respond well to deep x ray and, perusing the table of clinical details of the 17 patients, it appears that at least six of the responders did have an underlying seronegative spondylarthritis of some variety.

It would therefore be wrong to conclude from this report that when "simple" plantar fasciitis fails to improve with conventional treatment it is likely to respond to radiotherapy. A further study of "simple" plantar fasciitis in which seronegative spondylarthritis has been excluded is clearly required.

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Spurious polycythaemia in a neonate?

SIR,—There have recently been published a short report by Dr L E Ramsay (13 May, p 1251) and a letter from Drs B W S Robinson and D Corless (22 July, p 280) referring to Gaisböck's syndrome (spurious or "stress" polycythaemia or pseudopolycythaemia). We have recently seen a neonate who may have been suffering from the same syndrome and I have as yet been unable to find any references to this syndrome in neonates.

A 5-day-old full-term child was transferred to our unit with a diagnosis of Gram-negative neonatal meningitis made on day 3. The initial haemoglobin concentration was reported as 18 g/dl; when repeated on day 5 by heel prick it was 21 g/dl. It was thought that the child might be dehydrated; however her serum electrolyte values were normal and her fluid intake by intravenous infusion was adequate. Alternatively the heel prick might have given a spurious result; the haemoglobin estimation was repeated by venepuncture and the result was 20.9 g/dl. There was no evidence of renal disease or cardiac disease throughout her illness and as her meningitis resolved her haemoglobin level returned to 17-18 g/dl over three weeks.

I wonder whether this syndrome has been described in neonates and whether any other readers have come across the situation.

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Antifibrinolytic agents for ruptured intracranial aneurysms

SIR,—In contrast to the study by Mr R S Maurice-Williams (15 April, p 945), which suggests a beneficial effect of tranexamic acid after rupture of an intracranial aneurysm, we found in a double-blind controlled clinical trial no difference in either mortality or rebleeding rates.¹

Mr Maurice-Williams's study lacked a double-blind procedure and the patients investigated were restricted to those in Botterrell's grades I and II. The actual numbers of patients in a state of a clear or clouded consciousness in the two groups are not given, but theoretically this can have biased the results. Our own trial confirmed that the state of consciousness after a subarachnoid bleeding appeared to be the most important factor with regard to mortality.

The author's remark in the discussion that "the same striking improvement" with regard to the use of antifibrinolytic agents has been described in other controlled trials is hardly substantiated by the literature cited. The control group in Sengupta's series²—not double-blind—contained twice as many severely ill patients as the treatment group. The author's reference (No 10) to a personal communication cannot be regarded as additional evidence. Nibbelink's study³ had a follow-up period of only 14 days. Gibbs⁴ compared two groups of patients which were not comparable—namely, an operated group versus a group which apparently could not tolerate operation and subsequently was treated with tranexamic acid.

We do not think that this report adds valuable information to substantiate the assertion that tranexamic acid is of any use in the treatment of ruptured intracranial aneurysms.

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- ¹ van Rossum, J, *et al*, *Annals of Neurology*, 1977, 2, 238.
- ² Sengupta, R P, So, S C, and Villarejo-Ortega, F J, *Journal of Neurosurgery*, 1976, 44, 479.
- ³ Nibbelink, D W, Torner, J C, and Henderson, W G, *Stroke*, 1975, 6, 622.
- ⁴ Gibbs, J R, and Corkill, A G L, *Postgraduate Medical Journal*, 1971, 47, 199.

Autoimmunity in juvenile diabetics and their families

SIR,—Dr G F Bottazzo and his colleagues (15 July, p 165) re-emphasise the importance of both pancreatic islet cell and other antibodies in type I, "juvenile," diabetes. While these may be of importance in the aetiology of the disease, the evidence for their importance in the microangiopathic complications of diabetes is not proved by the evidence provided and their statements in the summary are misleading.

Diabetic microangiopathy, of which retinopathy is a manifestation, occurs in both type I and type II diabetes and there is no evidence to suggest that it is commoner in type I disease. It is a manifestation of the duration and also the degree of control of the metabolic abnormality.¹ After 15 years' duration up to 82% are expected to have retinopathy² and after 20 years up to 100%.³ If autoantibodies were of significance angiopathy would be expected to occur earlier. No patient in this series with disease duration under 10 years had such findings. The seven patients in table II (not 10 as suggested in the summary) with microangiopathy had all had diabetes for 12 years or more. The three who had both retinopathy and nephropathy had had it for over 20 years. In this small study of eight patients with diabetes for 12 years or more seven (88%) had retinopathy. This is not excessive and in view of the small numbers no conclusions can be drawn.

Besides duration of diabetes, diabetic control is of importance in the development of retinopathy, as shown in randomised controlled studies in man⁴ and animals⁵ and in the prospective studies by Pirart.⁶ Members of the same family are likely to have not only similar antibodies but similar eating habits, the same physician, and similar type of management. Control of the disease therefore is likely to be similar. By giving autoimmunity an unwarranted importance in the develop-

ment of the long-term sequelae of diabetes the authors suggest that, since these are "inherited," management of diabetes is of little importance. This is a view I wish to oppose strongly.

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- ¹ Colwell, A R, in *Small Blood Vessel Involvement in Diabetes Mellitus*, ed M D Siperstein, A R Colwell, and K Meyer, p 253. Washington, DC, American Institute of Biological Sciences, 1964.
- ² Larsson, Y, Sterky, G, and Christianson, G, *Acta Paediatrica*, 1962, 51, suppl 130, p 1.
- ³ Dolger, H, *Journal of the American Medical Association*, 1947, 134, 1289.
- ⁴ Job, D, *et al*, *Diabetes*, 1976, 25, 463.
- ⁵ Engerman, R, Bloodworth, J N B, jun, and Nelson, S, *Diabetes*, 1977, 26, 700.
- ⁶ Pirart, J, *Diabète et Métabolisme*, 1978, 3, 97.

Lymphatic fistula: a complication of arterial surgery

SIR,—Mr R J Croft (15 July, p 205) was using the wrong dye. The Patent Blue V which is distributed by May and Baker Ltd for Laboratoire André Guérin is a 2.5% solution of a calcium salt of sulpham blue with a molecular weight of 1159 and a colour index of 42051. It differs from Patent Blue V (Sigma), which is an 11% solution, and Disulphine Blue (Imperial Chemical Industries Ltd), which is a 6.2% solution of the sodium salt of sulpham blue which has a molecular weight of 567 and a colour index of 42045. These dyes, injected into the foot, will colour the groin lymphatics and nodes.

My colleagues and I have used this technique in 38 patients undergoing femoral profundaplasty.¹ Groin lymph structures showed up well and were either avoided, ligated, or sealed with surgical diathermy. No lymph cyst or fistula developed.

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- ¹ Leaper, D J, Evans, M, and Pollock, A V, *British Journal of Surgery*. In press.

Surgery for intracranial aneurysm in the elderly

SIR,—I read with interest the paper by Mr R P Sengupta and others (22 July, p 246) on surgery for intracranial aneurysm in the elderly, but I am puzzled by the authors' conclusions that surgery is justifiable in this age group. Their data would appear to confirm the feelings of others¹ that the results of such surgery are unrewarding.

Eight patients out of 32 (or of 25 if only those over 60 are counted) died or were left seriously disabled after surgery and a further three were left partially disabled. Furthermore, we are told nothing of those patients who died from rebleeding before surgery would have been carried out and who must surely be added to the overall surgical picture. Nor are we told the interval from the haemorrhage until operation, except in the cases of those patients who fared badly. Judging from the figures given for the latter it would seem that in many cases operation was performed late, after the period of maximum rebleeding risk had passed. A person who survives three weeks in good condition without rebleeding has only a 5-10% chance of dying from rebleeding from then on² and it is against this

sort of figure that surgical results must be balanced.

While late surgery in a younger person must take account of the risks of rebleeding in the very long term,³ this is obviously not so in the elderly, whose mean expectation of life is in any case limited. It is not valid to compare the treatment of an aneurysmal subarachnoid haemorrhage with that of a fractured hip, an intestinal obstruction, or a dissecting aneurysm in an old person. Patients with the latter conditions will die if left untreated. Most patients with a ruptured aneurysm who survive the first haemorrhage and the next few weeks will recover to resume a normal life without any surgical treatment.

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¹ Martindale, B V, and Garfield, J, *British Medical Journal*, 1978, **1**, 465.

² Alvord, E C, et al, *Archives of Neurology*, 1972, **27**, 273.

³ Kaste, M, and Troupp, M, *British Medical Journal*, 1978, **1**, 1310.

Antibiotic resistance in *Streptococcus pneumoniae* and *Haemophilus influenzae*

SIR,—May I be allowed to make an observation on the findings of the study group as published by Dr A J Howard and his colleagues (24 June, p 1657) after reporting my own experience in this health district?

To determine the changing pattern of antibiotic sensitivity in this locality a survey of all significant pathogens isolated in the first three months of 1977 and 1978 was undertaken. This was done by recording the results of routine disc tests and no attempt was made to find out the minimum inhibitory concentrations. The number of strains of the two organisms surveyed by the study group which were tested in these periods were as follows: *Haemophilus influenzae* 80 in 1977 and 116 in 1978; *Streptococcus pneumoniae* 33 in 1977 and 50 in 1978 (all in the first three months of each year). The percentages of resistant strains found are given in the table.

Percentage of antibiotic-resistant strains isolated in the first three months of 1977 and 1978

Antibiotics	<i>H influenzae</i>		<i>Str pneumoniae</i>	
	1977	1978	1977	1978
Ampicillin	0	2.5*	0	0
Tetracycline	0	0	9	6
Co-trimoxazole	0	4.3	0	0

*All were β -lactamase producers (total of three strains).

It can be seen from this table that the resistant figures found in this district in early 1977 were not similar to the national average of findings from 20 other laboratories as reported by the study group. Moreover, there were wide variations in the percentages of resistant strains found in the 20 different laboratories and only a very few matched with the national average. The percentages of tetracycline-resistant *Str pneumoniae* and ampicillin-resistant *H influenzae* strains from Nottingham (as mentioned in the report) were 14.8 and 1.1 respectively. Our findings from this district laboratory in the same period were 9 and 0 respectively. This difference is of much interest as the distance between these two laboratories is only about 15 miles

(24 km). In the early part of 1978 the figures in our laboratory had changed to 6 and 2.5 respectively. Thus the conclusion which I would draw is that it is good to know the national average, but it is of limited practical value because of variations from district to district and year to year. The clinicians should be better guided by the local microbiologist.

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Costs in perspective

SIR,—Your leading article "Treatment with bile acids" (29 July, p 309) refers to pharmaceutical treatment with an "expensive new agent" costing about £1 a day. I wonder how many of your readers realise that an average day's stay in an acute hospital bed now costs more than 30 times as much? Will we ever see the relatively low price of medicines in perspective against the very high costs of hospital care?

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A numbers game: understanding normal standards

SIR,—Your leading article (24 June, p 1651) is a useful reminder of some of the factors which should be considered in the assessment of results of laboratory or other tests. Frequently factors which may influence the results obtained for a particular sample, including techniques of collection and analysis and other factors that may influence the specimen before it can be obtained, are completely ignored.

Under clinical conditions a doctor may consider a result in the light of the patient's sex and possibly therapy, but rarely will the multitude of other factors, including time of day, biological rhythm, diet, or exercise, be considered. It is important to point out, particularly to medical students at a time when more emphasis seems to be placed on examination of results than examination of patients, that correct interpretation of "values of x" should be based on both an appreciation of the significance of altered levels of "x" and an understanding of the extraneous factors which may influence the results. Furthermore, it may not be adequate to compare the "level for x" with an inappropriate reference range, which may take no account of either the variations of different measuring techniques or the individual's age, sex, or race. The significance of a result may be obscured by the fact that it lies inside the reference range and may be appreciated more fully when related back to the individual. Reference ranges may be constructed using "normal" people who are neither ill nor particularly healthy, and special consideration may be necessary when a fit individual (for example, an athlete) consults his doctor.

Our own work, a longitudinal study of intensively trained competitive swimmers, shows that the haematological characteristics of such individuals vary within only very narrow ranges compared with the wide normal ranges expressed in standard haematological reference books and used in clinical

practice. Small changes of the values for an individual away from his normal range, which may adversely affect his performance, may not easily be recognised. If long-term monitoring is envisaged or required personal profiles need to be developed for particular individuals. Using such personal normal standards subtle changes may be detected and assessed more easily at an early stage.

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Swaddling and congenital dislocation of the hip

SIR,—A recent article in the *Sunday Times* (16 July) described the use of swaddling and a swaddling board by a child psychologist in the treatment of hyperactive babies. From the orthopaedic standpoint, this is absolutely contraindicated^{1,2} owing to the detrimental effects of splinting the hip in flexion and adduction on the development of the young hip.³

The incidence of congenital dislocation of the hip has been well established^{4,5} and runs at approximately 1.5 to 1.7 per 1000 live births—that is, an incidence of between 0.1 and 0.2%. As the article mentioned the Navajo Indians I feel one ought to quote the incidence of congenital dislocated hip in that population. It again has been well established⁶ and is in excess of 3%. Other communities where swaddling is practised, such as northern Italy and Germany, have a similarly increased incidence of congenital dislocation of the hip.

I would therefore make a plea that swaddling be completely abandoned and rejected. I am sure your readers are only too well aware that the orthopaedic work load is quite heavy enough without any help from the child psychologist.

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¹ Apley, A G, *A System of Orthopaedics and Fractures*, 5th edn. London, Butterworth, 1977.

² Salter, R B, *Textbook of Disorders and Injuries of the Musculoskeletal System*. Baltimore, Williams and Wilkins, 1970.

³ Salter, R B, in *Modern Trends in Orthopaedics*, No 5, ed W B Graham. New York, Appleton-Century-Crofts, 1967.

⁴ Barlow, T G, *Journal of Bone and Joint Surgery*, 1962, **44B**, 292.

⁵ Rosen, S, *Journal of Bone and Joint Surgery*, 1962, **44B**, 284.

⁶ Pemberton, P A, *Journal of Bone and Joint Surgery*, 1965, **47A**, 65.

Propranolol in black hypertensives

SIR,—In 1968 Dr Gerald Humphreys and I published in the *BMJ* a paper entitled "Ineffectiveness of propranolol in hypertensive Jamaicans."¹ I was slightly surprised to find a reply to it from Dr G A C Grell (29 July, p 358) appearing some 10 years later in your columns. However, I must accept all constructive criticism, however delayed, as valid.

Briefly, back in 1968 (in the early days of propranolol) Humphreys and I did a double-blind trial in which we were unable to find any hypotensive effect in Jamaicans. We were as bewildered then as to the mechanism of