unnecessarily given to the over-65 age group, one-third hypnotics and tranquillisers, and one-third antidepressants (imipramine, amitriptyline, and protriptyline) and the so-called cerebrovascular dilators (cyclandelate, isoxsuprine, naftidrofuryl, pentifylline, and thymoxamine). In the hypnotic and tranquilliser group the following drugs were considered to be responsible, listed in descending order of frequency of occurrence: prochlorperazine, chlordiazepoxide, diazepam, dimenhydrinate, chlorpromazine, chlormethiazole, promazine, thioridazine, trifluoperazine, and meprobamate. No cases were found among patients taking chloral hydrate and analogues, glutethimide, or nitrazepam. Prescription of barbiturates and methaqualone hypnotics are infrequent in this area but again postural hypotension was not recorded with either.

Until such time as a simple and reliable test of autonomic nerve function comes into use, we should do well to ignore much of the advice of our enterprising pharmaceutical industry and select and administer vasodilators and antidepressant and sedative drugs to the elderly with extreme caution.

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Raynaud's phenomenon as side effect of beta-blockers

SIR,—We note the comments made by Drs C W Marsden and P F C Bayliss (17 July, p 176) concerning our study of Raynaud's phenomenon in patients taking beta-blockers (19 June, p 1498). We accept that the incidence of side effects obtained with specific questionnaires may be an overestimate. Equally, side effects may be missed when spontaneous reporting by patients is awaited.¹ We reported the results of observations made in clinical practice. The study was retrospective and consequently it would have been impracticable to have had a placebo-treated group and a double-blind design.

The mean doses of the three beta-blockers and the effect of dose on development of Raynaud's phenomenon are shown in table I. The relationship between the presence of cold extremities and blood pressure control for patients on beta-blockers is shown in table II. While there are clear differences in the doses used in the three groups, there is no relation-

ship between dose and development of Raynaud's phenomenon. Similarly, degree of blood pressure control was not associated with cold extremities. These facts, therefore, do not detract from the possibility that there are true inter-drug differences in causing this side effect. Such differences can be verified only by prospective studies.

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¹ Alexander, W D, and Evans, J I, *British Medical Journal*, 1975, 2, 501.

Alternatives to barbiturate hypnotics

SIR,—There is an obvious advantage in prescribing nitrazepam as a hypnotic because with this drug overdose is less likely to be fatal. It is certainly less dangerous than barbiturates and indeed other non-barbiturate compounds mentioned in your leading article (12 June, p 1424). However, another aspect of nitrazepam deserves a mention. It has been shown¹ in a double-blind placebo controlled design to be more persistent than amylorbarbitone. In this study single doses of 5 and 10 mg of nitrazepam and 100 and 200 mg of amylorbarbitone were given at bedtime to normal subjects. They were tested the following afternoon with psychomotor tasks and an electroencephalogram (EEG) was recorded some 18 hours after the test substances had been given. The recording was assessed "blind" in terms of drowsiness and sleep. Both the psychomotor tasks and the EEG showed significant changes after nitrazepam in terms of slowing of performance and increased amounts of drowsiness and light sleep on the EEG. Even more important was the fact that the subjects were unaware of any persistent drug effects. Anxious patients in a similar study² showed the same trends but the effects were less marked.

Of course these findings do not entirely negate the current trend away from prescribing barbiturates but emphasise the fact that no drug is ideal.

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¹ Malpas A, et al, *British Medical Journal*, 1970, 2, 762.
² Malpas A, Legg, M J, and Scott, D F, *British Journal of Psychiatry*, 1974, 124, 482.

Child-resistant containers and child poisoning

SIR,—Accidental poisoning in childhood results in some 30 000 admissions to hospital in England and Wales each year and salicylates are a major contributor to this total. The most effective measure in controlling this problem

in the USA has been found to be the use of child-resistant containers, and since 1 January 1976 all aspirin and paracetamol manufactured for children in the United Kingdom has had to be packaged in child-resistant containers.

We report the preliminary findings of an ongoing survey of accidental poisoning in children and compare the total numbers of children under the age of 6 years who were admitted to hospital either in Newcastle upon Tyne or in South Glamorgan with accidental salicylate poisoning during the first six months of 1975 and 1976.

	Newcastle	S Glamorgan	Total
Jan-March 1975	10	21	31
April-June 1975	9	25	34
Jan-March 1976	5	10	15
April-June 1976	4	13	17

Thus a total of 65 children were admitted between January and June 1975 compared with 32 during the same period of 1976; the difference between these is statistically significant ($P < 0.002$).

Many children are still being poisoned with aspirin which has not been dispensed in child-resistant containers, the manufacturers and retailers still working through old stocks, but the significant downward trend in poisonings should continue when these are exhausted. We report our early findings to give encouragement to those campaigning for better safety for children in the form of more widespread use of child-resistant containers.

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Priorities in the NHS

SIR,—In your leading article on this subject (12 June, p 1425) you made the interesting, indeed provocative, comment that "If a diagnostic aid or a new drug or procedure has been proved to make a substantial difference to mortality or morbidity then the doctors working in the specialty will not be content until they use the new treatment on their patients." This assertion was subsequently challenged by Professor A L Cochrane (3 July, p 41) who pointed out your failure to supply evidence to support the statement. In reply you listed six specific procedures, etc, in which financial constraints have caused doctors to operate a rationing system. One of these examples was the use of fibre endoscopes in the management of upper intestinal haemorrhage.

While I agree that a large number of articles have shown the diagnostic superiority of endoscopy compared at least with the conventional barium meal (rather than the double-contrast technique), I would suggest that there are few data to prove that it has made a "substantial difference to mortality or morbidity." Indeed, the authors of some well-controlled prospective studies²⁻³ have actually shown that it apparently makes no such difference at all. Even the much quoted retrospective study by Hoare⁴ found that the difference in mortality rates between the group of patients having routine endoscopy and the group of patients having selective endoscopy was not statistically significant at the 5%.

TABLE I—Mean dose $\pm$ standard deviation (mg/day)

Drug	Whole group	Raynaud's phenomenon	
		Present	Absent
Oxprenolol	536 $\pm$ 303*	453 $\pm$ 120	560 $\pm$ 336†
Atenolol	180 $\pm$ 120*	157 $\pm$ 113	192 $\pm$ 125†
Propranolol	280 $\pm$ 200*	242 $\pm$ 163	334 $\pm$ 240†

*Significantly different from other treatments ($P < 0.025$)
†No significant difference between presence and absence of Raynaud's phenomenon ($P > 0.05$)

TABLE II—Results (mm Hg) for whole group on beta-blockers. Mean $\pm$ standard deviation

	Raynaud's phenomenon		Student's <i>t</i> test
	Present	Absent	
Treated diastolic BP	92 $\pm$ 17	95 $\pm$ 13	NS
Treated systolic BP	157 $\pm$ 34	163 $\pm$ 23	NS
Change in diastolic BP	28 $\pm$ 17	28 $\pm$ 20	NS
Change in systolic BP	41 $\pm$ 27	39 $\pm$ 31	NS

NS = not significant

level. At present, therefore, it would seem that the issue has yet to be settled and is not resolved by an ex cathedra statement.

Apart from the question of whether clinical procedures are effective or not, one must still face the fact that resources are limited. Presumably it will always be necessary to choose between competing projects or forms of expenditure, and the demonstration of the clinical effectiveness of a technique is thus only a necessary (but not sufficient) condition for it to be recommended for implementation. Doctors working in the specialty may not be content about this state of affairs, but the ultimate objective must surely be the optimum allocation of resources throughout the whole NHS and indeed the entire economy.

M H MILLS

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¹ Allan, R, and Dykes, P, *Gastrointestinal Endoscopy*, 1974, 20, 154.

² Morris, D W, et al, *American Journal of Digestive Diseases*, 1975, 20, 1103.

³ Sandlow, L J, et al, *American Journal of Gastroenterology*, 1974, 61, 282.

⁴ Hoare, A M, *British Medical Journal*, 1975, 1, 27.

Craniopharyngiomas

SIR,—We would like to comment upon one point in your leading article (3 July, p 5).

In referring to our paper¹ you rightly state that in craniopharyngioma concomitant strabismus may occur as a presenting feature, a fact recorded also by Wybar.² It is, however, misleading to imply that the onset of such squints is characteristically sudden. Any form of non-paralytic squint can present suddenly, but an unobtrusive onset is much more common. This holds good in craniopharyngioma.

The possibility that direct involvement of the visual path may be providing a visual obstacle to binocularity and so initiating a concomitant deviation should be remembered in every case of squint in a small child. The presence of optic atrophy or of a sluggish pupil should be excluded.

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¹ Kennedy, H B, and Smith, R J S, *British Journal of Ophthalmology*, 1975, 59, 689.

² Wybar, K, *Annals of Ophthalmology*, 1971, 3, 6, 645.

Sexual problems clinic

SIR,—As one who dealt tremulously with a few of the patients with sexual problems not treated by our Oxford sexual problems clinic in 1974-5, I must express relief at the modesty of the results claimed by my colleague, Dr John Bancroft, and Mrs Lesley Coles (26 June, p 1575).

This modesty may not be apparent either in the tables provided or in the claim, valid in itself, for "substantial benefits for two-thirds of those receiving treatment"—presumably based on table IV, which shows 53 (68%) in successful outcome or worthwhile improvement. But this 68% is of a total of only 78, while the sample studied was of 200. How comes this? For a start table IV omits altogether 19 patients (2 women, 17 men) whose problem is stated in table III as "others" and

Successful outcome	Worthwhile improvement	No worthwhile improvement	Dropped out	Given advice only	Not yet completed	Undisclosed "others"	Refused or inappropriate
29(14.5%)	24(12%)	10(5%)	15(7.5%)	2(1%)	15(7.5%)	19(9.5%)	86(43%)

whose outcome is as undisclosed as their exact problem. We need to know more about them and about the two given advice only: what advice and why only advice? Then there are a further 15 (9 women, 6 men) still receiving treatment or on the waiting list. To them must be added the 86 (43% of the whole sample) who, for reasons given in the text, refused or were refused treatment. Of the total sample, the outcome was successful in only 14.5%, and with improvement worthwhile in 12%, making a total ameliorated cohort of 53 (26.5%), a quarter of all.

All this could have been seen at a glance in the accompanying table if it had been included in the paper. May not this more modest presentation conduce to clear understanding?

SEYMOUR SPENCER

Oxford

*.*We sent a copy of this letter to Dr Bancroft and Mrs Coles, whose reply is printed below.—Ed, BMJ

SIR,—Dr Seymour Spencer is, of course, free to interpret our results in whatever way he chooses, but it is not clear to us what point he is trying to make. The issue is not whether we should be awarded a prize for the best results but whether the time and effort put into therapy is justified by the outcome.

When estimating outcome we can see no purpose in including those who were not treated, any more than those who were referred but did not attend. The reporting of outcome, as we noted, is in any case a difficult matter and our estimates must be regarded as crude. For this reason we confined the reporting of outcome to those patients with common types of sexual dysfunction. We are not concealing information which would alter the message we are hoping to convey, which, briefly stated, is that: (a) nearly half the patients seen in such a clinic reject or are not considered suitable for our methods of treatment; (b) of those who are treated, a sufficient number, in our opinion, benefit to justify the expenditure of time and the use of these treatment methods; (c) considering the extent to which treatment was carried out by inexperienced therapists, better results could be expected with more therapeutic experience; (d) the work load generated by such a service is relatively small and for a comparable area would probably require no more than six or seven sessions of therapist time per week.

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LESLEY COLES

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Hazards of modern competitive tennis

SIR,—The recent matches at Wembley have suggested two points of medical interest.

Firstly, with the advent of high-speed service there are risks of serious injury to people on the server's side of the net, especially the server's partner, spectators at the courtside,

and the net monitor. The latter—often elderly—sits with bowed head on his hands folded on the post top.

It has been estimated that in expert hands a service ball attains 140 mph (225 kph). A direct hit at this speed on the right target could produce instant death; death following intracranial haemorrhage; ruptured ear drum or eyeball; or detached retina. As there is no reason to believe otherwise, speeds will increase. Protection of side seats by wire screen is obvious. The umpire and net judge could be protected by wearing motorcycle helmets. An electronics expert could easily arrange a visual or audible signal to show that the ball has hit the net cord; it seems odd that in this age the game can depend on a man laying his skull on a net post. I suppose expert players carry insurance for third-party trouble; there could be some hefty claims.

Secondly, during the games we were frequently annoyed by the insanitary habit of a very prominent foreign competitor who filled his mouth with liquid and then spat it out in a stream at the feet of his opponent. And this in a club where permission to remove jackets in a heat wave has to be requested!

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Extrinsic allergic alveolitis

SIR,—It is indeed disappointing that our two papers^{1 2} are not mentioned in your leading article on this subject (3 April, p 791). Two points are unsatisfactory in your article. The first one relates to the simple test of detection of precipitating antibodies. Although indeed this may provide evidence of exposure to antigen, this can usually be obtained by taking the history. The point is that precipitating antibodies do not distinguish between asymptomatic exposed individuals suffering from other respiratory disease and symptomatic patients with extrinsic allergic alveolitis.

You then go on to refer to the not fully understood immunological mechanisms. In our publication¹ we were able to show clearly that although precipitins were in fact present in symptomatic individuals with pigeon breeders' disease, lymphocyte transformation in vitro was clearly a valuable test in the distinction of those patients with allergic alveolitis. This implied therefore that cellular mechanisms may be more important than the classic Arthus reaction as previously suspected. This relatively simple laboratory technique, we feel, becomes an important diagnostic test in allergic alveolitis.

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¹ Hansen, P, and Penny, R, *International Archives of Allergy and Applied Immunology*, 1974, 47, 498.

² Penny, R, *Medical Journal of Australia*, 1974, 1, 979.

*.*We agree, as we pointed out, that the presence of precipitins does not necessarily