area is maximally overlapped by the right ventricle when viewed from the right anterior oblique 30° projection (one of the two views used by the authors) and it is for this reason that the anterior view is preferred,² though even this does not entirely obviate problems of visualisation of inferobasal wall motion.³ The base of the left ventricle is similarly obscured in the left anterior oblique 45° projection by the aneurysm itself, which is closer to the collimator.

Two solutions to this problem exist. Firstly, the patient should be imaged in either the left lateral⁴ or the left posterior oblique projection,⁵ each of which minimises interference with visualisation of left ventricular motion by the right ventricle. Secondly, the first-pass technique can be used. This separates the ventricles temporally rather than spatially and accounts for the high accuracy of this technique reported by Dymond *et al.*⁶

Although a multicrystal camera is optimal for first-pass use, the Anger camera is adequate for a gated first-pass study,⁷ which may be obtained prior to equilibrium gated blood pool imaging with the same dose of radiopharmaceutical. The basal septum remains problematical as it is not well seen in profile by any of these methods when an apical aneurysm is present, and this may well be better visualised with single-photon emission tomography.

The major part of our criticism is that the authors have not used appropriate views for optimal visualisation of the basal wall motion, and we feel sure that if they did this they would achieve similar accuracy to that reported by previous authors.

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Prescribing for the elderly

SIR,—Dr Mary Bliss's admirable article "Prescribing for the elderly" (18 July, p 203) states so many important truths that I would like to underline one or two of them. Prochlorperazine (Stemetil) is one of the most frequently prescribed drugs for the elderly (certainly in this area). It is used in a non-specific way for "dizziness." Almost every old person will admit to dizziness if asked (usually meaning occasional light-headedness, possibly on changing posture). Prochlorperazine may only make this worse. It should not be used in the over-70s.

Diazepam, as she rightly states, should not be given to old people. I would go further and proscribe all benzodiazepines for the over-70s except in the most specific circumstances. As a group they are among the greatest causes of confusion in old people. The loss of the harmless placebo (tonics and vitamins, for instance) and their replacement by powerful drugs such as benzodiazepines has been a very dangerous change.

Predischarge training of patients in taking their own drugs is not too straightforward. Drugs may not be stored in patients' lockers

in hospital—so either they have to be delivered by the nurse at the appropriate time (which diminishes in part the object of the exercise) or a lockable cupboard must be fixed to the wall at the bedside and the patient hold the key.

Dr Bliss rightly pinpoints the problem of repeat prescriptions being written after patients' discharge from hospital giving the prehospitalisation drugs (either because the hospital discharge letter has not been received or because its contents have not been noted). Her proposed solution to confusion in prescribing—the "patient's prescription booklet"—is a serious approach to overcoming these difficulties.

Prescription abuse in the elderly is now epidemic and a national reappraisal is urgently required.

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Addiction to tranlycypromine

SIR,—I was interested in the report by Dr N Griffin and others (1 August, p 346), from a university department of psychiatry, of three patients who were addicted to tranlycypromine; and was surprised to note that the authors' research had revealed very little evidence of this condition. In our general practice of approximately 6300 patients we have identified three who have been addicted to this substance, all of whom have been successfully withdrawn from it over the last year without the necessity of referral to hospital. None of them were taking doses as high as those described by Dr Griffin and his colleagues, but in all three cases there were withdrawal symptoms persisting for several months. We would strongly support the suggestion that tranlycypromine is a drug with potent addictive properties and feel that this should be widely publicised.

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Synergistic action of metolazone with "loop" diuretics

SIR,—Both Drs R R Ghose and S K Gupta (2 May, p 1432) and Dr Janet M Allen and others (6 June, p 1873), describing their experience of the use of metolazone in resistant oedematous states, have stressed the need for hospital admission and close monitoring of the serum electrolytes when therapy is initiated. However, recent experience in this hospital suggests that reduction of the dose of "loop" diuretic does not necessarily remove the risk of uncontrolled diuresis with associated hypovolaemia and hypokalaemia.

A 55-year-old man who had suffered a myocardial infarction five years previously was admitted with bilateral basal effusions, ascites, and massive oedema. Despite treatment with frusemide 500 mg/day, spironolactone 200 mg/day, and isosorbide dinitrate 50 mg/day, he remained oedematous and bed bound. Metolazone was therefore introduced (5 mg alternate days) and the dose of frusemide was reduced to 250 mg/day. For the five days following the addition of metolazone his daily urinary output was 3.1, 5.1, 5.1, 4.8, and 6.1 litres, compared with approximately 1.5 litres during the previous therapy. Substantial potassium supplements were required and the patient lost

about 13 kg in weight. He subsequently died from an intercurrent infection.

I would suggest that as well as reducing the dose of loop diuretic a single trial dose of metolazone (5 mg) should be given and the diuresis assessed before further doses of the drug are prescribed.

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Breast transillumination using the sinus diaphanograph

SIR,—I was interested to read the report by Mr H W Holliday and Professor R W Blamey (8 August, p 411) on the use of diaphanography in screening for breast cancer. They point out that the technique is to be the subject of further trials in other centres and, as I believe that their results are not presented in the best way to allow comparison with other studies, the following comments may be helpful.

The performance of diaphanography in their hands can be represented as a 2 × 2 table (table I), from which several standard indices

TABLE I—Accuracy of diaphanography results in terms of final diagnosis

	Final diagnosis		Total
	Cancer	Not cancer	
Diaphanography result			
Cancer	18	7	25
Not cancer	6	144	150
Total	24	151	175

of its efficiency can be derived—for example, the true positive (18/24=75%) and true negative (144/151=95%) ratios referred to by the authors. In clinical practice, however, the predictive powers of positive and negative tests are more important when assessing the efficiency of the test and its relative costs and benefits. In their series the predictive power of a positive test was only 18/25=72% (that is, 28% false positives) but that of a negative test was 144/150=96%. The relatively low false-negative ratio (that is, 4%) is, however, dependent on two important factors: firstly, the methods used to establish the final diagnosis (such as palpation, mammography, and biopsy) and the proportions of the screened population subjected to different combinations of these, which are not stated in the report; and, secondly, the prevalence of breast cancer in the screened population.

In the series of Mr Holliday and Professor Blamey breast cancer prevalence was 14%, but given the same true positive and true negative ratios which they reported the predictive values of positive and negative tests will vary markedly with changes in prevalence.¹ This is shown in table II for some reported

TABLE II—Predictive powers of positive and negative diaphanographic tests with changing prevalence of breast cancer for a true-positive ratio of 75% and true-negative ratio of 95%

Prevalence of breast cancer in screened population (%)	Predictive power of positive test (%)	Predictive power of negative test (%)
0.25 ²	3	99.9
0.85 ²	11	99.8
7 ³	53	98
12 ⁴	67	97
14 (present series)	72	96

prevalence figures, from well populations and hospital clinics, which in turn are the result of self-selection, referral patterns, and the age composition of the screened population. The data emphasise that, with a trend towards less selection of women for screening, the predictive power of a positive diaphanoscopy test would be lower and that for a negative test higher than in their series.

One case appears to be missing from their results but it does not materially influence the outcome.

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Mortality from coronary heart disease in the British army compared with the civil population

SIR,—I was most interested in the paper (8 August, p 405) "Mortality from coronary heart disease in the British Army compared with the civil population," particularly the findings of a high mortality in the age group 20-39 years.

In a paper I published,¹ while I was principal medical officer at the Ministry of Pensions, on coronary occlusion in young adults I reviewed 100 cases in the services under the age of 35, and I also found a very high mortality in these young cases—80 deaths out of 100—and in most of these fatal cases death took place suddenly. It was noteworthy that practically all the sudden deaths were in apparently fit and healthy men who had previously carried out their military duties without any signs of cardiac distress, the disease being unsuspected during life and diagnosed only at necropsy.

The degenerative changes found in the coronary vessels were similar to those found in older age groups, the arteries being thickened, tortuous, and calcified—a thrombus was found in only 22 of the 80 fatal cases. Despite the youth of the men the pathological findings indicated that the process had been present for many years and that in these young cases genetic influences appear to be more important than habits of life and work.

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¹ Newman M. *Lancet* 1951;i:1045-8.

SIR,—Major Peter Lynch and Mr B J Oelman (8 August, p 405) discuss the observed differences in mortality from coronary heart disease between officers and soldiers. These differences are then related to the respective life styles.

Although figures are not quoted it is inferred that officers smoke much less than soldiers. There is no reference to alcohol intake, which might be a factor. Dietary differences are possibly relevant but it is true that 53% soldiers who are married eat food similar to any other British household? I contend that, generally, officers may still be more sophisticated in dietary habits and probably consume

less convenience foods, carbohydrates, and fats. Army catering does indeed offer the living-in soldier a large choice of menus, generally more so than the living-in officer. Might this not be an adverse factor? Obesity is actively discouraged but it is still a problem in the Army—more so in soldiers than in officers in my current general practice experience.

Moreover, the physical service experience and standards of fitness of officers and soldiers may not in fact be similar. I suggest that, at regimental level, the under-40 officers are, in general, fitter than many of their soldiers. Finally, are there other differences between officers and soldiers—for example, between those living in married quarters and those living in single accommodation in barracks, etc?

I make these few suggestions to stimulate further studies to complement this good and useful paper by Major Lynch and Mr Oelman. We need to know much more about the differences in life styles as well as the socio-economic backgrounds of officers and soldiers. As these factors are constantly changing, ongoing prospective studies would seem to be really well worth while.

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Why treat cirrhosis?

SIR,—Although it is often right to question the importance and cost effectiveness of our treatment, your leading article "Why treat cirrhosis?" (1 August, p 338) contains a number of misleading generalisations and half truths. Many of the statements are based on a 20-year study which was completed in 1976¹ and the most recent advances could not have had any impact on survival by that time. In the management of variceal haemorrhage, you do not mention the vital importance of endoscopy to determine the site of bleeding; few surgeons would now recommend a laparotomy because they were not sure of the source of bleeding. Endoscopic sclerotherapy after prompt and effective resuscitation can stop bleeding in over 90% of cases² and survival following long-term sclerotherapy is looking promising, with two-thirds surviving over one year after the initial bleed³ and half over two years (H D Sinnett *et al*, unpublished observations).

While it is true that abstinence from alcohol significantly improves the prognosis of alcoholic cirrhosis, there is no mention of the effects of treatment in autoimmune chronic active hepatitis, Wilson's disease, or primary biliary cirrhosis; 66% of patients with primary biliary cirrhosis survive five years after portal decompression for haemorrhage.⁴ Of course, much of our treatment is palliative and there are times when it is right not to employ heroic methods, but this applies equally to many other diseases, from cancer of the stomach and bronchus to coronary thrombosis.

To advocate sedation as part of the treatment of bleeding varices is inconsistent with your earlier statement that liver failure can be induced by overenthusiastic use of drugs. A more positive approach to the high mortality following massive blood loss would be to arrest the haemorrhage and treat the varices early. Awareness of the increasing frequency of chronic liver disease and its likely complications may do much to improve the treatment

and prognosis. The nihilistic attitude taken in your editorial will deny many people many years of worthwhile life.

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¹ Saunders JB, Walters JRF, Davies P, Paton A. *Br Med J* 1981;282:263-6.

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* * * The 20-year study of cirrhosis followed up patients until 31 December 1978, and the only "advance" that could not be assessed was endoscopic sclerotherapy. While this may be effective in the short term, there is as yet no consensus about its role in the long term; and it was the lack of any change in long-term outlook from costly treatments that we sought to emphasise. We accept that treatment is available for chronic active hepatitis, Wilson's disease, and primary biliary cirrhosis; but these constitute less than 10% of all cases of cirrhosis. Surely it is reasonable to concentrate resources on preventing alcoholic cirrhosis, which now accounts for 70% of patients, and on seeking the cause of cryptogenic cirrhosis, which accounts for the rest.—Ed, *BMJ*.

Career structure: an impediment in academic medicine

SIR,—Professor H A F Dudley's comments (30 May, p 1771) on the uncertain future of academic surgery raise issues that have equal relevance to other specialties.

He is less than optimistic that the "movers and shakers of the world" will triumph over "vibrating mediocrity." I venture to suggest that there are many United Kingdom graduates able and willing to take up this challenge, were it not that their future might thereby be jeopardised; and that the great strength of academic clinical medicine in North America is in no small part the result of the disparate career structure on the two sides of the North Atlantic.

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Management of scientific services and the changing face of the laboratory

SIR,—Miss Mary Warner (1 August, p 380) felt it necessary to explain the term medical laboratory scientist to "your predominantly clinical audience," as she put it. Unfortunately her personal interpretation is highly misleading. Semantic discussion is not always fruitful but the interpretation of the Institute of Medical Laboratory Sciences—with 17 000 medical laboratory scientists in membership—may be considered relevant by your readers.

There are two main groups of staff associated with medical laboratories: those medically qualified and those—medical laboratory scientists—holding a variety of scientific qualifications. Miss Warner's attempt to separate groups of medical laboratory scientists