reports on bronchodilator infusions which were given as initial therapy for acute asthma and for short periods only, where it would be a simple matter to change treatment for those who failed to respond. We cannot really contest his views on the ethical committee, which may apply criteria more stringent than elsewhere. The information presented in our paper suggested that a salbutamol infusion of 10 µg/min is no more effective than the aminophylline infusion, and Professor Grant may well be correct to state, as we inferred, that with adequate administration of nebulised bronchodilator drugs an intravenous infusion may not make a significant contribution to airway function.

The analysis for significant differences in lung function between the two infusion groups included all the patients up to the time of withdrawal from the study if before 36 h. Dr Anderson assumes incorrectly that the data on those patients withdrawn before 36 h were not included in these analyses. The fact that of the six patients withdrawn from the salbutamol group before 36 h, two were responding faster than the group as a whole makes the response between the two groups less marked than Dr Anderson suggests.

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Electron microscopy of cell membranes

SIR,—Having read Minerva's comment (6 May, p 1220) on Dr Harold Hillman's controversial views, may I refer her to Ghadially's book on cell ultrastructure¹ in which there are numerous examples (for example, pp 109, 123, 175, and 231) of the widening and blurring of unit membranes due to oblique cutting and the effect of section thickness. Indeed, I could supply many more from my own work. These examples completely contradict Dr Hillman's assertion that unit membranes always appear to be of identical width in electron micrographs—a statement so patently untrue that many electron microscopists are at a loss to understand how such a fundamental misconception could occur. It is hardly surprising, therefore, that so little interest has been taken in Dr Hillman's comments.

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¹ Ghadially, F N, *Ultrastructural Pathology of the Cell*. London, Butterworths, 1975.

Ginseng and mastalgia

SIR,—Mr B V Palmer and his colleagues (13 May, p 1284), in reporting the occurrence of mastalgia in a woman taking ginseng, suggest that the preparation may have mild hormonal activity.

Such an observation is not unexpected, since small quantities of oestrone, oestradiol, and oestriol are present in ginseng root.¹ This is not the only possible type of complication with ginseng; some of the products concerned appear to affect blood levels of glucose as well as glycogen levels in liver and muscle, and it has been suggested that they may interfere with the regulation of diabetes.² At least one of the components is closely related

to digitoxin and could in theory cause problems in patients already taking cardiac glycosides.³ Unfortunately, most of the clinical literature on ginseng is very uncritical and provides no help in allaying or confirming such suspicions.

Since ginseng products, while generally evading medicines legislation, are apparently widely promoted and sold throughout western Europe there is every reason for the physician to determine whether his patients are taking them; they may well be a cause of interactions and adverse effects which have not yet been recognised as such.

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¹ Court, W E, *Pharmaceutical Journal*, 1975, **214**, 180.

² Dukes, M N G, *Side Effects of Drugs Annual*, 1977, **1**, 374.

³ Oura, H, *et al*, *Journal of Biochemistry (Tokyo)*, 1975, **77**, 1057.

If I had . . .

SIR,—Since the cataract surgeon of my choice has retired, my colleagues and I have often discussed to which surgeon we would go and what we would wish done in the event of developing cataracts in each eye. It was therefore with interest that I read the article by Mr Brian Harcourt (29 April, p 1121).

Each cataract surgeon, I am sure, believes that he or she knows the best way to deal with this common event. I agree with Mr Harcourt on most points in his detailed analysis of what he would do. I would, however, spend much time picking the surgeon of my choice, whose ability and judgment I would consider to be impeccable. I would hope that this mythical person might be persuaded to operate on me before my vision fell to 6/24 as I would find it impossible to continue my work at this visual level. I would pick a surgeon with experience of conventional intra- and extra-capsular lens extraction and one who is familiar and experienced with the technique of phacoemulsification. I would hope that he would evaluate the relative advantages of phacoemulsification versus conventional surgery in my case.

If my lens nucleus was not hard, and if I were a myope (which I am), I would value the freedom and safety of a small wound and that an intact posterior capsule would give me postoperatively. From personal experience I would not be too anxious at the thought of prolonged irrigation or corneal damage resulting from a straightforward phacoemulsification with eight seconds' irrigation, in competent hands, and I know that I would be happy with the quiet, painless eye 24 h later. I would expect to take a chance of the necessity of a posterior capsulotomy after five years if my capsule thickens, but I would hope it would not. I would like to get up and get home early—not to play golf but because, much as I think I would be treated well in hospital, home is nicer, and in common with other doctors I believe I would be an impatient patient and anxious to get on with the process of seeing again. If I had had a phacoemulsification I believe that I would be able to get my contact lens fitted sooner than if I had had conventional surgery.

If, however, my mythical surgeon chose conventional surgery I would hope that he would reduce his instrumentation to a

minimum in order to reduce the possible dangers to my corneal endothelium as much as he could. Like Mr Harcourt I would not, as yet, opt for intraocular lens insertion, though I hope that my cataract will stay away long enough for the problems of this mode of surgical replacement to be solved so that I might be able to experience it in safety. Failing that, I would of course opt for contact lenses.

If I had cataract, however, I believe I would be too much involved to make unemotional decisions and therefore, having done my bit by picking my surgeon and behaving as I hope all my own patients will do, I would lie back and let him do the worrying, trusting to be one of the majority of cataract patients who do have an uneventful postoperative convalescence and who do achieve good post-operative sight.

I trust that neither Mr Harcourt nor I will for a long time (I hope never) be in a position to write you an article on "When I had cataract"—but it would be interesting to hear the opinions of some ophthalmic surgeons who must have had this operation done. In the meantime the search for impeccable judge and able and uninvolved surgeon continues.

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Assaults on doctors

SIR,—We read your leading article on this subject (13 May, p 1229) with some concern. At the recent annual meeting of the Casualty Surgeons' Association in conjunction with the Royal College of Nursing Accident and Emergency Forum one of the themes of the conference concerned our legal rights concerning patients. At the same meeting we heard a discussion document from a joint working party investigating the occurrence of violence in accident and emergency departments. Despite the inability of this working party to show any objective evidence to suggest an increase in violence, the general body of opinion was that measures must be taken to protect the staff from patients.

Hence against a background demonstrating a singular lack of evidence to support the contention that violence to medical staff, paramedical workers, and nurses is increasing we are confronted with a demand for more comprehensive protective measures. Certainly in Derby we have no real problem of violence in the hospital although we cannot speak for other areas, which may experience local difficulties. It seems to us that by sensationalising a problem which seems to be generally no worse than it ever has been we are at risk of attracting a violent element of the public to our hospitals and so creating a real problem.

Although we concede that medical workers must not be expected to take assault as a matter of course, it must be accepted that potentially violent situations will occur because of the very nature of our work. We have coped with them in the past using guile, tact, and sympathy, often for ill or highly stressed individuals. We must not replace this subtle approach with a display of alarms and indiscreet security men or be too ready to use our statutory powers under the Mental Health Act to deprive patients of their liberty. Let

us first objectively show that there is indeed a growing problem at all, and if there is one then consider the best way to tackle it in the interest of the profession and the public we serve.

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Dopamine and dobutamine

SIR,—I am worried at the relative lack of differentiation given to the recently introduced pharmacological agents dopamine (Inotropin) and dobutamine (Dobutrex) discussed in your recent leading articles (17 December, p 1563, and 22 April, p 1010). It is unfortunate that these agents have similar generic names as, although both have inotropic action, there are important differences between them:

(1) Dopamine is a naturally occurring hormone which is the precursor of noradrenaline,¹ whereas dobutamine is a synthetic catecholamine which acts directly on the receptor sites of myocardial muscle.²

(2) Dopamine is dependent for its effect on the release of myocardial noradrenaline,¹ while dobutamine is not.³

(3) Dopamine has been associated with peripheral vasoconstriction⁴ and there have been reports of digital gangrene with dopamine usage,⁵ neither of which has been described in relation to dobutamine.⁶

(4) Dopamine exerts a direct effect on the dopaminergic receptors within the renal vasculature, which can result in an increased urinary flow.⁷ This improvement in urinary output may or may not be related to improvement in cardiac output. Dobutamine exerts no specific effect on these dopaminergic receptors and any increase in urinary flow is related to an increase in cardiac output.⁸

It is also of interest that there are strong reasons to support the experimental evidence⁹ that the inotropic action of dobutamine does not produce expansion of myocardial ischaemia following infarction.¹⁰

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¹ Goldberg, L I, *New England Journal of Medicine*, 1974, **291**, 707.

² Stoner, J, Bolen, J, and Harrison, D C, *British Heart Journal*, 1977, **39**, 536.

³ Loeb, H, and Bredakis, J, *Circulation*, 1977, **55**, 375.

⁴ Greene, S I, and Smith, J, *New England Journal of Medicine*, 1976, **294**, 114.

⁵ Alexander, C, and Sako, Y, *New England Journal of Medicine*, 1975, **293**, 591.

⁶ Robbie, N, and Nutter, D, *Circulation Research*, 1974, **34**, 663.

⁷ Goldberg, L I, *New England Journal of Medicine*, 1974, **291**, 707.

⁸ Leier, C B, Webel, J, and Bush, C, *Circulation* **56** (3), 468.

⁹ Tuttle, R, and Pollock, G, *Circulation Research*, 1977, **41**, 357.

¹⁰ Gillespie, T, and Dieter Ambros, H, *American Journal of Cardiology*, 1977, **39**, 588.

Stiff shoulder after stroke

SIR,—Professor J C Brocklehurst and his colleagues (20 May, p 1307) found that 21 patients out of 135 with acute strokes had developed a stiff and painful shoulder within two weeks.

In our experience in this unit this early complication of a stiff and painful shoulder is entirely preventable and we have not seen it in patients admitted directly to our unit for very many years. It is not a matter of treatment

by the physiotherapists but of treatment by the nurses. All nurses in our unit are taught that whenever they attend a patient with an acute stroke, whether he is in bed or in a chair, they must lift his hemiplegic arm well above his head once in addition to whatever else they are going to do for him. This is a simple task and we often ask relatives as well as the nurses to do it for the patient. There is no reason why it should not be done by the patient's family if he is nursed at home.

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Non-epileptic television syncope

SIR,—Your leading article on television-induced epilepsy and its prevention (20 May, p 1301) was most helpful, but your readers may gain the impression that all seizures which occur in front of the television set are epileptic. Once again¹ it is important to emphasise that there is another common type of seizure, the "anoxic" seizure,² otherwise known as convulsive syncope. Anoxic seizures of this kind occur when the heart stops for about 10 seconds. In young children they are commonly reflexly precipitated by pain or other unpleasant stimuli,² and they may also be induced in certain children by fever as a genetic variety of febrile convulsions (to be published). What is of considerable interest is that similar seizures may occur in front of the television set for no other apparent reason. Four such cases are briefly described below.

A 9-year-old boy was referred as ? television epilepsy after a single, long, asymmetrical, clonic seizure which began while watching television. He was about to change schools in the following week and there had been two tragic deaths in his family in the immediate past. The electroencephalogram (EEG) with photic stimulation was normal on two occasions three months apart, but ocular compression in the supine position induced cardiac asystole of 10 s followed by EEG slowing.

A 3½-year-old boy while watching television tried to stand up, shaking all over, and collapsed on the floor. The EEG and the response to photic stimulation were normal, but ocular compression when supine induced asystole of 9.5 s with diffuse EEG slowing, deviation of his eyes, and a dazed expression.

A 14-year-old mentally handicapped boy, supposedly epileptic, had his worst-ever episode while watching television (a habit which was rare for him). He had some sort of warning, fell, his eyes went up into the back of his head with the whites showing, he was blue, stiff, and rigid, and then there was some jerking and he was briefly dazed. The EEG with photic stimulation was normal, but ocular compression induced asystole of 14 s followed by jerking of his lower limbs, cyanosis, and diffuse slowing of the EEG to 1½/s for 11 s. He responded to questions 8 s later.

A 9½-year-old girl had three episodes of unconsciousness in which she became limp with possible facial twitching, two of which occurred while watching television; she was referred as ? photosensitive epilepsy. The EEG with photic stimulation was normal, but ocular compression induced asystole of 7.7 s followed by 8 s of diffuse EEG slowing to 2½/s.

All four children had seizures in front of the television set but no EEG evidence of television epilepsy. On the other hand they had a highly abnormal oculocardiac response. More than 4 s asystole on ocular compression is regarded as abnormal, occurring in no

more than 1% of the population. Frank anoxic seizures following ocular compression in the supine position have been seen only in those subject to reflex anoxic seizures or syncope² and in a subgroup of those with febrile convulsions. The evidence presented here suggests that something in the act of television watching can induce a striking anoxic seizure easily mistaken for epilepsy unless ocular compression forms part of the EEG examination.

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¹ Stephenson, J B P, *British Medical Journal*, 1977, **2**, 45.

² Stephenson, J B P, *Archives of Disease in Childhood*, 1978, **53**, 193.

Factors affecting length of hospital stay

SIR,—I would refer to the letter of Mr H B J Chishick (29 April, p 1145) purporting to compare the costs of cataract surgery in different areas of Wales.

The data presented by Mr Chishick are inaccurate. The duration of stay for cataract operations in West Glamorgan in 1975 varied from 6 to 11 days, depending on the surgeon operating and the nature of the complications, and not, as stated 17.0 to 25.4 days.

Mr Chishick's estimation of the cost of cataract surgery using erroneous lengths of stay must therefore also be incorrect. The methodology adopted to arrive at the alleged cost per inpatient week is unusual. The stated base line is data obtained from a Welsh Office publication,¹ which give the cost per inpatient week average for Wales in acute non-teaching hospitals as £139. This figure is derived from an aggregation of all specialties in all hospitals of that category in Wales. Mr Chishick apparently has then divided this amount by 7, to arrive at an inpatient day cost of £19.86. He has then multiplied this figure by the length of stay extremes in his quoted print out figures, to conclude that the inpatient week cost is £155 in Gwynedd and £504 in West Glamorgan.

Hospital Activity Analysis has value in the management of the Health Service, but its credibility is reduced by letters such as that from Mr Chishick.

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¹ Welsh Office, *Health and Personal Social Services Statistics for Wales*, No. 3. Cardiff, HMSO, 1976.

* * * We sent a copy of this letter to Mr Chishick, whose reply is printed below.—Ed, *BMJ*.

SIR,—The figures that I quoted referred not to the absolute but to the *mean* duration of stay for cataract surgery, which was 17.0 days for men and 25.4 days for women, and in fact these mean values represented a range of 6 to over 50 days. It was not my intention to make invidious comparisons between the area health authorities in Wales but merely to demonstrate that there was a wide variation in the length of stay for one type of elective surgery for which there is a low incidence of complications.

The cost of any operation, procedure, or