older unimmunised ones should have exaggerated rather than diminished the apparent protective effect of the vaccine. I was careful, however, not to claim that my study demonstrated a lack of protective effects of vaccination to the younger children. I did discover that the only seriously ill infants were too young to have been vaccinated in any case; and that these children were threatened by an outbreak of which the onset and initial spread were entirely among immunised children.

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Mental handicap and the BBC

SIR,—Like Dr B M Laurence (30 June, p 1785) I was appalled that the mentally handicapped patient in a proposed television documentary should be stated to have suffered hypoxic brain damage under general anaesthesia.

Apart from causing unnecessary public alarm (and the subject is in the public mind—the first question a teenager asked me recently on learning that I was an anaesthetist was how many brain deaths had I caused, a question which I felt was like asking an airline pilot how many jumbo jets he had crashed recently) it is not much of a tribute to those anaesthetists who strive to keep even the sickest patients alive and well under anaesthesia, nor a reflection of the millions of anaesthetics given uneventfully every year. In fact, it is a tribute to anaesthesia that such casualties are so widely reported—road traffic accidents are not.

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Lung cancer and coal workers' pneumoconiosis

SIR,—In his review of a book on asbestosis Dr Dewi Davies (23 June, p 1701) cites *Thorax* in support of his statement that it has been claimed that coal workers' pneumoconiosis protects against lung cancer. The form of his reference suggests that this was an editorial, whereas in fact it was a paper by Dr Rooke¹ and his colleagues to which Dr Davies was referring. This paper, based on a necropsy population, showed that there was no positive link between carcinoma of the lung and pneumoconiosis. It did not, however, make any claim that pneumoconiosis protects against lung cancer, though it did quote Ashley's² theory to that effect.

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¹ Rooke, G B, *et al*, *Thorax*, 1979, **34**, 229.

² Ashley, D J B, *British Journal of Cancer*, 1967, **21**, 243.

SIR,—In an aside Dr Dewi Davies (23 June, p 1701) cites me as suggesting that coal workers' pneumoconiosis "protects one from getting lung cancer." I reported a 16-year mortality follow-up of 16 628 coalminers.¹ Complicated pneumoconiosis (PMF) had been diagnosed radiologically for 685 of these men at the start of the study period, and 13 265 had no pneumoconiosis (category 0) initially. The average age-standardised death rate attributed to lung cancer among those

with PMF was about half that found for miners with radiographs classified as category 0. Lung cancer mortality among men with categories 2 or 3 simple pneumoconiosis was also relatively low. I studied these findings further in subgroups involving more than 11 000 of the miners. Those analyses took into account the men's smoking habits and estimates of their cumulative exposure to coalmine dust. There was no evidence of a protective effect of dust exposure among men with no pneumoconiosis initially and I commented in that context that "the reduced lung cancer incidence among men with pneumoconiosis is more likely to be explicable in terms of the presence of pathology associated with radiological pneumoconiosis, rather than as a consequence of dust exposure per se." Taken out of context this argument could be misunderstood by miners and readers of the *BMJ*.

In my discussion of the results I noted that "the very high (nearly eightfold) excess lung cancer mortality among the cigarette-smoking coalminers studied, compared with their non-smoking colleagues, makes it abundantly clear that neither coalmining as such, nor exposure to dust can be regarded as an effective protection from the disease."

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¹ Jacobsen, M, PhD thesis. Edinburgh, 1976.

Hypnosis

SIR,—May I enter the list wearing two hats—the first as a physician dealing with psychosomatic disorders, the second as coauthor of two of the early controlled trials of hypnosis and autohypnosis in the treatment of asthma?

My first reaction on reading Dr H G Kinnell's two letters (17 March, p 751; 9 June, p 1563) was a feeling of weariness that an academic is again levelling criticism at hypnosis, when those of us who employ it clinically find it unequivocally to be of value in psychosomatic disorders generally, providing as it does clearance of the disorders well above the 67% level which was reported within the narrow confines of a clinical trial.

My constant plea is for the critics to come into clinical work and give the method a fair trial. It is essentially easy to learn, not costly to apply, and results in patients recovering their health without the use of expensive, at times hazardous, and often addictive drugs. Dr Peter Nixon has already commented on the clinical value that he has found when hypnotherapy is used in the field of cardiology in his department at Charing Cross Hospital.

With regard to our two trials: the problem of objectivity in terms of tests persistently bedevils the planning of prospective research in asthma. We found that although only severe cases with reversible airway obstruction were accepted for treatment a large proportion had normal respiratory function tests on the two occasions they were assessed before treatment started. Hence it was impossible to find later improvement by this yardstick; Dr Kinnell argues, without validity, that hypnosis does not exert physiological effects.

Dr Kinnell is, I fear, biased in his reporting. He chooses not to report a significant fall in wheezing score among female patients, commenting that "females differ for some obscure reason," while pinpointing, correctly, the

reported fact that significant changes were not observed among men—but he failed to point out that it was the male controls who responded obscurely, benefiting far better than would be expected from a placebo effect.

I think that Dr Kinnell should not scoff at clinical judgment; he brushes aside as "subjective" the opinion of the independent assessors in the second control trial. These were consultants who did not practise hypnotherapy and who were kept unaware of what treatments had been given; they made their full clinical assessment in the way that they normally conducted their outpatient work.

Again Dr Kinnell was selective in his reporting. He made little of the difference between 59% of patients who benefited markedly from hypnosis as compared with the 43% of the controls who fared the same, but he omitted to say that half of those physicians who were administering hypnosis were deliberately chosen as people who had had no previous experience. Analysis of the results in the hands of the other half, who were skilled hypnotherapists, showed that no less than 67% of their patients in the hypnosis group were much better, a very distinct advantage over the controls.

I consider that these figures speak for themselves, when nearly seven out of every ten bad asthmatics can be helped to this degree. In our field studies we have found similar results with migraine and other conditions associated with autonomic imbalance—this is indeed a very powerful aid which can be offered to our patients.

GILBERT MAHER-LOUGHNAN

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SIR,—Having been involved in using hypnosis and teaching the use of hypnosis for nearly 20 years I hope I may be permitted to comment on some of the views expressed by Dr H G Kinnell (9 June, p 1563).

Firstly, I would like to know what experience Dr Kinnell has in using hypnosis. It has been my experience over the years that many people write articles and books and you find on examination that they have had very little practical experience on the use of hypnosis, but rush into print. He dismisses all the trials that have been carried out which he knows about, but does not mention many other trials that have been carried out both here and in the USA¹⁻⁴ showing that hypnotic techniques can be of great value in a multitude of conditions.⁵ David Scott⁶ has shown in the plastic unit in which he works that the postoperative medication in patients taught autohypnosis and then subjected to skin grafting has been reduced to 75%. Another unit in America (a burns unit) showed that by using hypnotic techniques the healing rate of the burn is considerably enhanced.⁷ In the use of hypnosis to help people give up smoking—and I stress help give up, not cure—my own figures show that in all the people I see approximately 55-60% give up the habit. Many cases of autonomic imbalance again can be cured by using various hypnotic techniques of which possibly Dr Kinnell is not aware.

In the original editorial in the *BMJ* what was called for was a request to extend our knowledge of hypnosis and to carry out more well-controlled trials, and to explore what other further uses it might have in medicine and surgery. Dr Kinnell dismisses the "hypnosis lobby," as he describes it, as not having proved

its case. I do not think any of the letters that I have read following his original letter have tried to prove anything, but are merely suggesting that possibly it is worth considering as a way of helping people on their way to recovery.

Dr Kinnell also says that not everybody will use hypnosis with the same care and expertise as Dr Waxman (19 May, p 1354). It is precisely for this reason that the British Society of Medical and Dental Hypnosis has given training courses to numerous doctors and dentists in all parts of the country and does stress these factors, especially to the novice learning how to use it responsibly. In London, in the metropolitan branch, I personally have been involved in running seminars at regular intervals to which the novice can come and discuss any problems he may be having in the use of hypnosis in his practice; at the same time all those present learn something in the discussion which follows. Would Dr Kinnell suggest that because a surgeon amputates the wrong limb surgery should be condemned? However, this is what he is more or less suggesting with the use of hypnosis—that because some doctors use it irresponsibly the whole subject should be dismissed as a part of fringe medicine with no real value.

What I would like to suggest to Dr Kinnell is that he opens up his mind a bit more and starts running some of the trials which he condemns, to see whether there is more in the hypnotic technique than he so far likes to believe, and at the same time encourage other doctors in the Health Service to run such trials. Might I suggest, however, that it is no good having people who have been using hypnosis for a few months engaged in carrying out such trials, as it is only after many years of use that one is competent to use it most successfully.

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¹ Sacerdote, P, *American Journal of Clinical Hypnosis*, 1966, 9, 100.

² Clawson, T A, jun, and Swade, R H, *American Journal of Clinical Hypnosis*, 1975, 17, 160.

³ Willard, R D, *American Journal of Clinical Hypnosis*, 1977, 19, 195.

⁴ Friedman, H, and Taub, H A, *American Journal of Clinical Hypnosis*, 1978, 20, 184.

⁵ Frankel, F H, and Zamansky, H S (editors), *Hypnosis at its Bicentennial: Selected Papers*. New York, Plenum Press, 1978.

⁶ Scott, D, *Proceedings of the British Society of Medical and Dental Hypnosis*, 1975, 1, 10.

⁷ Ewin, D M, in *Hypnosis at its Bicentennial: Selected Papers*, ed F H Frankel and H S Zamansky, p 155. New York, Plenum Press, 1978.

Who should remove the endotracheal tube?

SIR,—Dr B H Goodrich (21 April, p 1079) and Dr J M Cundy (5 May, p 1213) have written about avoiding vomiting and aspiration following extubation after anaesthesia.

I would like to submit that a great deal of sorrow and unhappiness could be avoided if the anaesthetist could accept the fact that no patient should be extubated—in the sense that the endotracheal tube should be left in place until the patient is awake and alert and has recovered his strength and co-ordination to such an extent that, on a verbal command such as “Reach up with your hand and pull that tube out,” he or she can then carry out that instruction. This has been my practice for years, and I have been quite satisfied that if a patient meets the above mentioned criteria he is most likely to be able to handle regurgitation.

This approach does not avoid the need for vigilance by the recovery room staff, nor does it preclude the use of the head-down or lateral position; but, unfortunately, it does require significant readjustment in thinking. Firstly, the anaesthetist should accept the fact that there are certain things that the patient can do better for himself; and, secondly, the recovery room staff should accept that their activities should encompass more than pleasant baby-sitting for the patient.

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Artificial ventilation and survival

SIR,—Dr J F Nunn and others in their recent article (9 June, p 1525) state that there is little information on mortality in wards after transfer from an intensive therapy unit. Perhaps their attention should be drawn to a report¹ of a series of 1719 consecutive patients admitted to an intensive therapy unit in whom well over 80% were ventilated for more than eight hours to give an in-unit mortality of 13.5% and a post-unit mortality of 7.1%. This differs from the 33% in-unit mortality and 30% (20 out of 67 discharges) post-unit mortality reported from Northwick Park. Their paper reports that of the 100 patients ventilated 20 died in the ward, yet their text says that this mortality was 17%. These figures are a little mystifying.

The fact of artificial ventilation of itself, however, is meaningless unless the criteria for artificial ventilation are stated. Thus in our unit the prophylactic use of artificial ventilation following major cardiac surgery has significantly reduced mortality and morbidity, similarly following reconstructive surgery of the lower aorta prophylactic use of artificial ventilation has reduced morbidity. Undoubtedly some of these patients would have survived without artificial ventilation but we have frequently regretted it when in individual cases we have attempted to predict that artificial ventilation would be unnecessary. A more recent development of our prophylactic use of artificial ventilation is following major neurosurgery with the objective of monitoring a lower PaCO₂ and so a lowered intracranial pressure. In such patients there is no question of respiratory failure. It is a matter of regret that the publication from Northwick Park does not indicate whether all the patients had established respiratory failure, which would otherwise have given a more meaningful interpretation to the follow-up data presented.

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¹ Tomlin, P J, *Anaesthesia*, 1978, 33, 710.

* * * We sent a copy of this letter to the authors, whose reply is printed below.—ED, *BMJ*.

SIR,—We are grateful to Dr P J Tomlin for drawing our attention to his review of experience from the Queen Elizabeth Hospital intensive therapy unit in Birmingham. It is difficult to compare our experience with his, however, because:

(1) The majority of patients in the Birmingham unit were recovering from cardiothoracic surgery, for which the mortality is quite

different from general surgical and medical cases.

(2) It is not possible, from Dr Tomlin's paper, to derive mortality in patients who required ventilation as opposed to those who did not.

If we take only those patients in his paper characterised as general surgery and general medicine (some of whom were ventilated and some not) we find that the average mortality in patients in the intensive therapy unit for the four years of his study was 30% and 29% respectively. This compares with our unit mortality of 33% for ventilated cases and 16% for all cases, but obviously the statistics are very dependent on the type of patient admitted and the readiness with which ventilation and other measures are adopted, so that comparisons of this sort should be treated with caution. Our main interest was not so much the mortality in the unit, or even on the ward, but in the more long-term follow-up.

We are also grateful to Dr Tomlin for pointing out the error in our text in the discussion section, where the mortality in the ward following discharge from the intensive therapy unit should read not 17% but 20%, as is clear from the relevant table and text in the results section. We apologise for this error.

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A “specialty” for second-class doctors?

SIR,—Dr M P Roseveare's letter (30 June, p 1797) prompts a reply. While welcoming advances in general practice and in particular training requirements for practice, I am a little concerned that mature and experienced hospital doctors might be discouraged from entering general practice. The division of British medicine into narrow and protected bands of interest—call them what you will—can only frustrate many young doctors, and benefit little the health service. By coincidence, perhaps, the same issue carries a letter about “disillusioned registrars” (30 June, p 1798). Surely the answer to both letters lies in the expansion of a system of general practice that allows well-trained principals opportunities to exploit their special skills and expertise, as indeed some have done for over 10 years in Livingston.¹

Rigidity of thinking that bars the entrance to general practice of doctors from any source or that frustrates and wastes their skills is to be deplored. This in no way denies the need for vocational training but is simply a plea for an open-minded attitude to recruitment.

Dr Roseveare and Dr Taylor can be assured that general practice does, if there is the will and determination, offer scope for doctors to exercise their special skills at the same time as enjoying a wider experience of medicine. Certainly there is no need to think of being a second-class doctor.

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¹ *The Livingston Project—The First Five Years*. Scottish Health Services Studies No 29 (SHHD 1973).

SIR,—It cheered me to read Dr M P Roseveare's letter (30 June, p 1797) and I hasten to rally to him in his views about the