readers might assume can be "taken as read." Hence I was astonished to find that only two of its seven conclusions (and these two were statements of the obvious) could withstand rigorous scrutiny.

I would thus challenge the assertion by Mueller *et al* that breast cancer is most rapidly lethal in the elderly, and suggest that the authors have erred in completely ignoring the possibility that the disease is diagnosed relatively late in older women. The recent paper by Dr W H Redding and his colleagues on age and prognosis in breast cancer (2 June, p 1465) comes to the same conclusion.

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[1] Mueller, C B, Ames, F, and Anderson, G D, *Surgery*, 1978, **83**, 123.

***It is an uncomfortable though hardly an unscientific fact that the majority of women who present with carcinoma of the breast will die of their disease. For a start, something like 30-40% of all women who present with breast cancer have either locally advanced or widely disseminated disease on presentation.[1] This group as a whole have little chance of living five years after diagnosis.[1] The remainder presenting with "operable or potentially curable" breast cancer can be expected to demonstrate an approximate 50% 10-year survival, irrespective of primary modalities of therapy.[2] Ten years' survival, however, does not in itself guarantee cure, and two series with long-term follow-up have clearly demonstrated that the excess risk of dying applies up to 20 years or more after primary therapy, providing an estimated 30% cure rate.[3 4] The data have nothing to do with Mueller's article and should be widely known to all clinicians involved in the treatment of breast cancer. If uncomfortable facts cannot be published in a medical journal, then we would indeed be burying our heads in the sand.

It is true that in Mueller's article, and also in the short paper by Dr Redding and his colleagues, there was a small disproportionate increase in the incidence of stage III and unstaged breast cancers among the elderly. However, Mueller and his colleagues took the trouble to exclude the unstaged cases from the analysis and to correct according to stage at diagnosis in making their comparisons between the age groups. Thus, to quote from their conclusion, "Age as well as stage at diagnosis are significant determinants of the length of survival and cause of death." But "late" breast cancer may in part reflect its aggressive nature and not simply the delay in presentation on the patients' part.[5]—ED, *BMJ*.

[1] Cutler, S J, *Seminars in Oncology*, 1974, **1**, 91.
[2] Fisher, B, *Cancer*, 1973, **31**, 1271.
[3] Adair, F, *et al*, *Cancer*, 1974, **33**, 1145.
[4] Brinkley, D, and Haybittle, J L, *Lancet*, 1975, **2**, 95.
[5] Devitt, J E, in *Risk Factors in Breast Cancer*, ed B A Stoll, p 110. London, Heinemann Medical, 1976.

Costs of unnecessary tests

SIR,—Dr Gerald Sandler's timely and important paper (7 July, p 21) on the cost of unnecessary tests omitted one relevant fact. The unnecessary investigations performed by the doctor in outpatients have already probably been unnecessarily performed by the GP, and the results given in the referral letter. Discussions with colleagues from around the country suggest that it is very rare for such prereferral tests not to be repeated in the clinic. The potential saving from dropping at least one tier of this over-investigation could be huge.

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Seat-belt legislation

SIR,—Dr Gordon Avery (9 June, p 1561) assumes that infringement of a law enforcing the use of seat belts would be easy to detect. During a recent holiday in Spain, however, where the wearing of seat belts outside urban areas is compulsory, I discovered that those who object to wearing seat belts have a simple method of getting round the law. They pass the seat belt across their bodies but do not fasten the buckle. This makes detection practically impossible; though in the event of an accident the pattern of injury might well reveal the true situation.

This way round the law, far from making things more difficult for the police, removes two of the main objections to the introduction of legislation. In the first place, it means that any motorist with a conscientious objection to the use of a seat belt could continue to exercise his conscience without fear of detection. Also, since detection in these circumstances is practically impossible, the police would have no major problem of enforcement.

The most important deterrent to disobeying the law would come, however, not from the police, but from the insurance companies. If the wearing of seat belts were made compulsory then, in the event of an accident resulting in injury that would have been prevented by the wearing of a seat belt, the insurance companies could impose much stiffer penalties with the backing of the law. I am sure that widespread publicity regarding reduction of the damages to those who do not wear seat belts would do far more to encourage conformity to the law than any police measures.

We should therefore cease concerning ourselves with the imaginary problem of the 10% who will refuse to wear seat belts and concentrate on the known benefits to the 90% who will obey the law.

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Diazepam and traffic accidents

SIR,—In the epidemiological survey by Dr D C G Skegg and others (7 April, p 917) it was clearly shown that drivers who receive minor tranquillisers are significantly over-represented among accident victims. The authors also point out that studies of this type fail to distinguish between the effects of the drug and of the condition being treated.

People are prescribed tranquillisers because they are anxious, aggressive, or depressed, and it is recognised that these patients are more likely to be involved in accidents.[1] Notwithstanding numerous claims, the detrimental effect of diazepam on driving ability has never been established. There is considerable evidence that critical flicker fusion frequency is significantly reduced by small doses of diazepam. No case has been made, however, that reduced critical flicker fusion frequency is in any way deleterious to driving ability. A statistically significant increase and reduction in reaction time (both have been reported) does not necessarily harm driving performance. Some psychomotor tests have shown that benzodiazepines at times improve and at other times impair performance: no relationship between these skills and driving ability has been established. The presence of diazepam and its metabolites in the blood of drivers involved in car accidents varies from under 2% in a recent New Zealand study[2] to nearly 20% in an older Norwegian survey.[3]

As a general rule, it is preferable that anxious, aggressive, and depressed patients do not drive: with diazepam medication driving safety could deteriorate, remain unchanged, or improve.

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[1] Milner, G, *Drugs and Driving*. Basle, Karger, 1972.
[2] Missen, A W, *et al*, *New Zealand Medical Journal*, 1978, **87**, 275.
[3] Bø, O, *et al*, in *Alcohol, Drugs and Traffic Safety*, ed S Israelstam and S Lambert. Toronto, Addiction Research Foundation, 1975.

Whooping cough after stopping pertussis immunisation

SIR,—For too long the whooping cough debate has been fuelled by data whose interpretation leaves too much to speculation. Dr Robert K Ditchburn's (16 June, p 1601) study is no exception. The attack rate in children aged 3½ to 15 years is 45%. It would be valuable to see the age incidence of these cases and it would shed light either on the efficacy of the vaccine with age or on an unusual pattern of attack in older children.

The Keyworth study[1] showed reducing protection of the vaccine up to 5 years, but this was evident only because there was an adequate control group. Surely it is unreasonable to compare 3½ to 15 year olds with under 3½ year olds.

The Keyworth study showed 84·4% protection in 1 to 4 year olds. If I recalculate my data using Dr Ditchburn's system and compare unimmunised children under 3½ with immunised children aged 3½ to 7 years, the apparent protection falls to 50%—a very important difference.

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[1] Jenkinson, D, *British Medical Journal*, 1978, **2**, 577.

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

SIR,—I thank Dr Douglas Jenkinson for his comments on my paper. In the outbreak I describe, whooping cough occurred in six of seven immunised children aged 3½ to 5 years; in 18 of 42 aged 6 to 10 years; and in 22 of 44 children aged 11 to 15 years. Thus any reduction of vaccine efficiency with age, if it occurred, must have been before the age of 3½ years.

I do not accept that the pattern of attack in older children is necessarily unusual, though in remote Shetland it could well be so. Until there are more studies in which the unaffected children are positively identified, the "usual" attack rate in older children will not be known.

I accept the limitations of comparing children in different age groups. Of course, if it is true that young children are more susceptible to whooping cough, my study comparing young immunised children with 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 Rooke[1] 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's[2] 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 nonsmoking 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.

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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 USA[1-4] showing that hypnotic techniques can be of great value in a multitude of conditions.[5] David Scott[6] 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