seems illogical and makes immunotherapy impractical.

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1 Committee on Safety of Medicines. *Statement on desensitisation*. *BMJ* 1986;**293**:948.
2 Varney VA, Gaga M, Frew AJ, Aber FR, Kay AB, Durham SR. Usefulness of immunotherapy in patients with severe summer hay fever uncontrolled by antiallergic drugs. *BMJ* 1991;**302**: 265-9. (2 February.)

Freeman Hospital

SIR,—Despite the optimism of Mr Jeremy Laurance,[1] if what is happening at the Freeman Hospital is what the NHS "reforms" are really about then the first signs are not good. One of Newcastle's two dermatology wards is at the Freeman Hospital, but dermatology is not included in its trust prospectus because all dermatology services are to be centralised at the Royal Victoria Infirmary when ward space becomes available in two or three years. Yet on 1 April this year dermatology will be discontinued by the new Freeman Trust—to be replaced, it is believed, by a private surgery ward for overseas visitors.

Dermatology is a subregional specialty in Newcastle, and the loss of over 40% of its beds will affect the whole of Northumberland. Surely the Freeman can and should be compelled to maintain this essential regional service until adequate facilities are found elsewhere. If this ejection of dermatology by the new Freeman Trust Hospital is permitted we can only conclude that it is a victim not so much of the stimulating "scent of opportunity... in the air" as of the sharp whiff of commerce.

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1 Laurance J. Freeman hospital: countdown to self government. *BMJ* 1991;**302**:580-2. (9 March.)

Child health computing

SIR,—The correspondence following our editorial on child health computing[1] has raised a number of important issues.

Drs Malcolm Aylett and Allan Colver are incorrect in suggesting that the national child health system is unable to provide rapid feedback of immunisation performance to practitioners.[2] Vaccine coverage data for individual general practitioners, health visitors, and child health clinics are available as a standard request from the statistics package of the system.[3] Not all districts that use the system routinely feed back this information[4]; failure to do so, however, is due not to a deficiency in the system but to failure to use an available resource.

Drs Aylett and Colver also advocate adopting the family health services authority's list as the basis for a community register. Other correspondents (Dr Sally Jefferies and colleagues[5]), however, draw attention to the inaccuracies that exist on both this list and child health computer lists. These discrepancies, which are due mainly to the lack of information flow between professionals,[6] can be eliminated only by collaboration between the two systems working towards a common register, thereby increasing the number of opportunities for updating information on the register. It has to be remembered that up to 15% of children in inner city districts are not registered with a general practitioner, and the health authority register thus remains necessary. The development of electronic links between the child health and family health services authorities' systems is now being piloted in Stockport (ICL version) and Avon (MUMPS version). This is only the first step, and future developments for the system will include direct linkage to general practitioners' microcomputers.

Dr D J Hewitt criticises the national system for being centralised and unresponsive to local needs.[7] Many health authorities do not, however, have the resources to develop their own system. The interactive ICL version of the child health system, which will be available from July, allows considerably increased local flexibility while retaining the financial advantage of sharing the heavy development costs among user districts. Compliance with data protection and confidentiality requirements are already proved, and staff can operate the system without additional training when they move between health authorities. For every health authority to develop its own system would lead to chaos and unnecessary expense.

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1 Ross E, Begg N. Child health computing. *BMJ* 1991;**302**:5-6. (5 January.)
2 Aylett M, Colver A. Child health computing. *BMJ* 1991;**302**:409. (16 February.)
3 Child Health Computing Committee. *The child health system—a statistical guide*. Cardiff: Welsh Health Common Services Authority, 1987.
4 Begg NT, White JM. A survey of pre-school vaccination programmes in England and Wales. *Community Med* 1988;**10**: 344-50.
5 Jefferies S, Victor C, Oerton J, McShane S, Beardow R. Child health computing. *BMJ* 1991;**302**:410. (16 February.)
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7 Hewitt DJ. Child health computing. *BMJ* 1991;**302**:409-10. (16 February.)

Trials of homoeopathy

SIR,—The scoring system employed by Dr Jos Kleijnen and colleagues does not adequately reflect the credibility of publications.[1] No account is taken of peer review; indeed, it is biased against peer reviewed publications. A study by some of us that was published in the *BMJ* achieved a relatively low score—because it had not been possible to include full details of patient characteristics, randomisation, etc, in a 600 word *BMJ* short report.[2] A fuller version of the paper published in a less competitive and less rigorously reviewed journal would have scored higher. But would it really have been better?

Excessive weighting was given to trial size. The main reason cited was "worry about incomparability at baseline." This can be entirely obviated by cross over, yet the authors discriminated against cross over studies.

We wonder if Dr Kleijnen and colleagues have considered the methodological and logical problems raised by their call for checks on blinding. Presumably this would take the form of a question: "Do you think your medication is active or a placebo or don't you know?" If investigators can influence assessments, they can also influence patients to answer "don't know" to this question, which would be taken as indicating adequate blinding.

We believe that the questions surrounding homoeopathy will be resolved by the classical method: repetition of trials, with methodological improvement, at disinterested centres of excellence. This is the strategy that we are pursuing in several clinical trials at our two centres.

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1 Kleijnen J, Knipschild P, ter Riet G. Clinical trials of homoeopathy. *BMJ* 1991;**302**:316-23. (9 February.)
2 Fisher P, Greenwood A, Huskisson EC, Turner P, Belon P. Effect of homoeopathic treatment on fibrositis (primary fibromyalgia). *BMJ* 1989;**299**:365-6.

SIR,—A recent review of randomised trials of homoeopathy led the authors to the conclusion that: "in [our] opinion, the results do not provide acceptable evidence that homoeopathic treatments are effective."[1] To establish whether there is evidence of the efficacy of homoeopathy is not an easy task, and the methodology proposed by Dr Jos Kleijnen and colleagues is an important step forward.[2] However, some of their assertions seem debatable.

Firstly, is it true that "much evidence is available"? Perhaps, if the whole of homoeopathy is considered as a single medicine; surely not, if one counts the impressive number of ingredients, so that for some preparations positive results seem to be extremely scarce, whereas for the remaining preparations used in homoeopathy there are no results at all. Therefore the likelihood of a bias in reporting positive results is indeed tremendously high, probably higher than for other treatments.

Secondly, the indications given in table II were mostly for diseases the evolution of which is recognised to present huge fluctuations, making it difficult to assess the specific effect of a medical treatment. Thirdly, it seems (from the same table) that when homoeopathy had a more pronounced effect than placebo this was often on subjective symptoms (those assessed with a visual analogue scale), for which a psychological induction is likely: this must be emphasised because the authors have doubts about the blindness of most trials.

Finally, one can wonder whether the reported results, even when statistically significant, were *medically* important: is it very important to raise the percentage of recovery in influenza from 10·3% to 17·1%, or to wait for 4·0 days until first faeces rather than 4·9 (especially in view of the efficacy of laxatives when needed)?

I perfectly agree with Dr Kleijnen and colleagues that there is no reason to believe that the influence of bad methodology is much less in conventional medicine than in homoeopathy. But evidence that the assessment of allopathic treatments may be poor should be an incitement to become more demanding with our academic procedures, and by no means an encouragement to be less critical with alternative medicines.

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1 Hill C, Doyon F. Review of randomized trials in homeopathy. *Rev Epidemiol Sante Publique* 1990;**38**:139-47.
2 Kleijnen J, Knipschild P, ter Riet G. Clinical trials of homoeopathy. *BMJ* 1991;**302**:316-23. (9 February.)

Spontaneous pneumothorax

SIR,—Dr Douglas Seaton and colleagues present an interesting approach to the problem of managing patients with spontaneous pneumothoraces

treated initially by aspiration,[1] but their conclusion that the "finding of no pleural leak should avoid unnecessary tube drainage and permit early discharge, whereas had these patients been treated with an intercostal drain from the outset their hospital stay would inevitably be prolonged" is clearly not sufficient justification to purchase a flame ioniser as they suggest.

Few people would drain all these patients from the outset—a trial aspiration followed by repeat radiograph the next day if the lung re-expanded would be more usual.

After aspiration six pneumothoraces studied showed no reduction in size and therefore an intercostal drain was inserted. In the remaining 19 the lung had re-expanded on the post-aspiration radiograph. In 12 of these episodes of pneumothorax the lung remained expanded (one after a repeat aspiration). In the 10 episodes in which a gas leak was shown seven lungs subsequently collapsed and required drainage. This technique is supposed to allow definitive management of these patients, including early discharge home, so the fact that in three of these 10 patients a positive result did not predict outcome is a considerable problem with the technique. If this is the accuracy of the technique in its advocates' hands it is hard to imagine that better results will be achieved by a hardpressed medical senior house officer or registrar on call. More worrying, false negative results might lead to a patient being discharged inappropriately.

Any unit contemplating purchase of this flame ioniser (£2340) is likely to deal with enough chest problems to have access to a respiratory function unit. If these patients were taken to a respiratory function laboratory they could inhale a helium mixture and the helium content of the aspirated pneumothorax could then be measured with ease on the equipment already available.

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1 Seaton D, Yoganathan K, Coady T, *et al.* Spontaneous pneumothorax: marker gas technique for predicting outcome of manual aspiration. *BMJ* 1991;**302**:262-5. (2 February.)

AUTHORS' REPLY,—Mr Duffy's assertion that a trial aspiration followed by a repeat radiograph the next day is the more usual management of spontaneous pneumothorax is unfortunately not borne out by the facts, particularly in the case of surgeons. A postal survey of British Thoracic Society members showed that only 21% of thoracic surgeons used syringe aspiration, the majority relying on the more traditional method of intercostal tube drainage.[1]

We also fear that Mr Duffy may have missed the main point regarding the value of the technique in predicting outcome. It is those patients with a negative result (no marker gas in the aspirate) who seem to have sealed off their leak and who can therefore be considered for early discharge. We have so far had no false negative results, and the mean duration of hospital stay in this group was 1·8 days. We agree that patients with a positive result (marker gas in the aspirate) need more cautious management as in 81% the lung collapsed again and required a further drainage procedure. A positive result therefore usually indicates a continuing leak of clinical importance.

We have already tried helium as an alternative marker gas, but unfortunately the helium analysers commonly available in lung function laboratories are not sufficiently sensitive to detect small leaks. A further point is that our method is more portable and therefore applicable at the bedside, which is important as these patients frequently present out of laboratory hours.

We emphasise that this is the first reported use of leak detection in pneumothorax by means of a tracer gas. We have found that with proper instruction the technique can be used by junior medical staff in a busy district general hospital setting. We are continuing to attempt to refine the technique in order to increase ease of use and to eliminate false positive results. We believe that the principle of leak detection by means of a tracer gas is worth further exploration in the interests of reducing patients' discomfort as well as saving time and money.

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1 Butland RJA, Spickett GP, Seaton D. A survey of the management of primary spontaneous pneumothorax in the UK. *Thorax* 1987;**42**:226-7.

Managing spinal cord compression and lung cancer

SIR,—Dr P Watson and colleagues suggest that to prevent spinal cord compression prophylactic radiotherapy should be given to patients with small cell lung cancer and back pain who have a positive bone scan or radiological abnormality.[1] Unfortunately, neither of the cases presented supports this conclusion. The first patient should have received palliative radiotherapy for pain at the time of the second positive bone scan at the latest, irrespective of any consideration of the prevention of spinal cord compression. The second developed compression before admission for chemotherapy and presumably, therefore, before any prophylactic radiotherapy could have been given, unless emergency prophylactic spinal irradiation is being suggested.

The general hypothesis that spinal radiotherapy may prevent cord compression is supported by the absence of this complication in 11 patients with small cell lung cancer with radiological change plus persistent spinal pain who received palliative radiotherapy[2] and the rarity of cord compression in spinal segments that have previously received palliative radiotherapy (unpublished data). The question is whether prophylactic rather than palliative radiotherapy is indicated. In support of prophylactic radiotherapy Dr Watson and colleagues quote a report by Goldman *et al* suggesting that the incidence of spinal cord compression is 36% (9/24) in patients with small cell lung cancer who have a bone scan or radiological abnormality in the spine plus back pain at presentation.[3] According to table 1 in this report, however, six of these patients presented with spinal cord compression and could not have been eligible for prophylactic radiotherapy. This implies that of the remaining 18 patients, three (16%) subsequently developed spinal cord compression. Further, of those with late compression (as opposed to that occurring at presentation), only five of 18 (28%) had an abnormal bone scan at presentation and only three (16%) an abnormal bone scan plus back pain, making the potential benefit of prophylactic irradiation given as suggested three of 24 cases of spinal cord compression occurring in a total of 610 patients. Late compression in small cell lung cancer is associated with radiological abnormalities in only 20% of cases,[2] presumably because it is secondary to meningeal carcinomatosis rather than osseous metastasis and therefore unlikely to be prevented by local radiotherapy.

The necessity for radiotherapy in all patients with back pain plus radiological abnormality in the spine at presentation is not clear. Pedersen *et al* reports on 15 patients with these features but without spinal cord compression, of whom six received chemotherapy alone and nine needed radiotherapy for persistent pain.[2] None of these patients developed spinal cord compression. Thus spinal disease may be controlled by chemotherapy alone in a sizeable proportion of patients.

The logical approach to spinal disease in small cell lung cancer (in the absence of spinal cord compression) is appropriate palliative radiotherapy for severe or persistent pain. If this is given promptly then little additional benefit is likely to accrue from prophylactic spinal irradiation.

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1 Watson P, Spiro SG, Barrett-Lee P, *et al.* Managing spinal cord compression in small cell lung cancer. *BMJ* 1991;**302**:103-5. (12 January.)
2 Pedersen AG, Bach F, Melgaard B. Frequency, diagnosis and progress of spinal cord compression in small cell bronchogenic carcinoma. *Cancer* 1985;**55**:1818-22.
3 Goldman JM, Ash CM, Souhami RL, *et al.* Spinal cord compression in small cell lung cancer: a retrospective study of 610 patients. *Br J Cancer* 1989;**59**:591-3.

SIR,—The discussion of the management of spinal cord compression in small cell lung cancer is pessimistic and somewhat misleading.[1] The discussions state quite correctly that it is difficult to reverse the effects of cord compression with laminectomy. Laminectomy is indicated for spinal cord compression only if the extradural tumour is situated posteriorly, and in our experience that is unusual. Apart from not relieving cord compression it leads to further instability of the vertebral column, with collapse of the diseased vertebrae and increased cord embarrassment. Laminectomy is not indicated for anterior cord compression.

It is reported that paraplegia is often due to spinal artery occlusion and hence the reason for the poor results of surgery. In our own series of metastatic spinal cord compression due in the main to anteriorly placed tumours, anterior decompression and stabilisation allowed 63% of patients to regain the ability to walk and 73% to regain bladder and bowel function.[2] Our figures agree with those of other authors.[3,4] I would agree that surgery is unrewarding to the completely paraplegic patient and is not undertaken in our unit. The message to physicians is to refer patients with early signs of cord compression to a spinal surgeon as soon as possible.

Finally, I concur that the survival rate of patients with metastatic disease of the spine secondary to a primary tumour in the lung is poor. The mean survival time of 11 patients was only 11·3 weeks (range 1 to 32 weeks). The quantile estimate showed that 75% of patients survived two weeks, 50% survived eight weeks, and only 25% of patients survived 24 weeks after spinal decompression. The survival of patients with the lung metastases was significantly shorter than the survival of patients with the breast metastases ($p = 0·001$) and is significantly shorter than the overall survival of patients with other tumours.

However, the surgery allowed them to walk home and certainly improved their quality of life for their last few precious months.

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1 Watson P. Managing spinal cord compression in small cell lung cancer. *BMJ* 1991;**302**:103-5. (12 January.)
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4 Sanderson N, Galicich JH, Bains MS, Martini N, Beattie EJ Jr. Vertebral body resection in the treatment of cancer involving the spine. *Cancer* 1984;**53**:1395-6.