discomfort and risk. The technique is as simple as taking a blood sample.

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1 Harvey J, Prescott RJ. Simple aspiration versus intercostal tube drainage for spontaneous pneumothorax in patients with normal lungs. *BMJ* 1994;309:1338-9. (19 November.)
2 Raja OG, Lalor AJ. Simple aspiration of spontaneous pneumothorax. *Br J Dis Chest* 1981;75:207-8.
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May not reduce the need for pleurectomy

EDITOR,—The British Thoracic Society's research committee has shown that aspiration is less painful than intercostal tube drainage.[1] Because of differences between the two populations studied, however, I question whether the authors are justified in concluding that "aspiration reduces the need for pleurectomy." Eighteen (58%) of the patients treated by intercostal drainage had a complete pneumothorax before treatment, compared with only 10 (34%) of the patients treated by simple aspiration. This difference, which occurred by chance, may be an alternative explanation for the higher rate of pleurectomy in the group treated by intercostal drainage. The absence of a significant difference between the two groups before treatment does not rule out this possibility.

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1 Harvey J, Prescott RJ, on behalf of British Thoracic Society Research Committee. Simple aspiration versus intercostal tube drainage for spontaneous pneumothorax in patients with normal lungs. *BMJ* 1994;309:1338-9. (19 November.)

Enteroviral hypothesis for motor neurone disease

EDITOR,—Two controversial articles suggesting a possible enteroviral aetiology for motor neurone disease (amyotrophic lateral sclerosis) have recently been published.[1,2] We report on a farmer's family in which the 44 year old mother has been diagnosed as suffering from motor neurone disease. Interestingly, from the time of onset of the mother's symptoms her husband (45 years old) and two sons (16 and 18 years old) showed weakness of the legs; in the husband's case this led to a temporary paresis in the legs. These case histories suggested a transmissible agent. We therefore looked for serum antibodies to infectious agents; we also included certain veterinary agents with known or suspected pathogenicity for humans.

The serum samples from all members of the family were negative for antibodies to *Borrelia burgdorferi, Treponema pallidum,* different serotypes of leptospira and salmonella, *Brucella abortus* and *Br melitensis, Yersinia pseudotuberculosis, Chlamydia trachomatis* and *C psittaci, Coxiella burnetii,* listeria, *Mycoplasma pneumoniae,* Coxsackievirus groups A and B, different types of echovirus, encephalomyocarditis virus, HIV-1 and HIV-2, hepatitis B virus, and pseudorabies virus. In some of the serum samples low antibody titres were identified to tickborne encephalitis virus (vaccination titres), measles virus, mumps virus, herpes simplex virus, varicella zoster virus, cytomegalovirus, and Epstein-Barr virus, reflecting the average prevalence in the population.

We wish to emphasise three results found in the patient with motor neurone disease. Firstly, no antibodies to Coxsackie virus group B (either IgM or IgG) could be detected. Secondly, a relatively high titre of antibodies to poliovirus type 1 (1/512 and, in another laboratory, 1/320) was found, whereas types 2 and 3 showed titres of only 1/32 (in the other laboratory, 1/40); the patient had received a trivalent poliovirus vaccine seven years before the onset of the disease. Thirdly, an antibody titre of 1/20 to Borna disease virus was found. In contrast, the other members of the family were negative for Borna disease virus and showed moderate poliovirus vaccination titres (for all three serotypes) of 1/10 to 1/80.

While poliovirus has long been suspected of being associated with motor neurone disease,[3] this is the first report of antibodies to Borna disease virus in a patient with motor neurone disease. Antibodies to Borna disease virus have been reported in patients with psychiatric disorders in Europe and the United States,[4] but have also been shown in some healthy people.[5]

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Homoeopathy for recurrent upper respiratory tract infections

No children received no treatment

EDITOR,—We are surprised by the negative conclusions drawn by E S M de Lange de Klerk and colleagues after their trial of homoeopathy in recurrent upper respiratory tract symptoms in children.[1] These conclusions do not seem to be justified by the data.

The most serious problem is the inadequate statistical power of the trial. Although no power calculation was presented in the paper, such a calculation was performed for the study and published elsewhere.[2] This calculation estimated that 300 patients would be required for 90% power and 5% significance. The paper published in the *BMJ* was based on evaluation of only 170 patients. It would be unjustified to claim that, because the study recruited little over half its target number of patients and came close to conventional significance (P=0·06-0·07 for the main outcome measure), it would have achieved significance had the recruitment target been met. But the authors draw the converse conclusion, which in our view is equally unjustified. Ironically, the same issue of the *BMJ* contains an evaluation of a palliative care service that was considered to have failed because inadequate data could be evaluated.[3]

We also dispute the correctness of the clinical implications: the most striking result was the considerable improvement in both groups. None of the children were given no treatment: both groups received dietary counselling, which forms part of the treatment package offered by many homoeopaths and other complementary practitioners but is not part of the conventional management of this condition. Similarly, we wonder how many general practitioners (and their budget managers) would agree that a 55% reduction in the use of antibiotics in the active treatment group compared with 38% in the placebo group is "not clinically relevant."

These results suggest that homoeopathy may have a role in a common form of childhood morbidity. Homoeopathy is a complex system of which several different versions exist, but no details of the homoeopathic prescribing were given. Future investigations should describe the type of homoeopathic prescribing as well as ensure adequate statistical power.

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1 De Lange de Klerk ESM, Blommers J, Kuik DJ, Bezemer PD, Feenstra L. Effect of homoeopathic medicines on daily burden of symptoms in children with recurrent upper respiratory tract infections. *BMJ* 1994;309:1329-32. (19 November.)
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3 McWhinney IR, Bass MJ, Donner A. Evaluation of a palliative care service: problems and pitfalls. *BMJ* 1994;309:1340-2. (19 November.)

Use of daily symptom score not validated

EDITOR,—The study of homoeopathy for recurrent upper tract infections has several flaws.[1] My most serious criticism concerns the main outcome measure, the daily symptom score. No references are given to validate the use of this instrument. Moreover, because daily scores were averaged over a year the baseline differences between the groups cannot be assessed. If baseline symptom scores were initially lower in the treatment group this might well explain the apparently greater efficacy of homoeopathy; if the reverse was true then the effects of homoeopathy may have been underestimated. Averaging symptom scores over a year can also obscure clinically important differences, just as the percentage risk of drowning averaged across a population will be only marginally affected by an event such as a ferry disaster. Recording changes in baseline scores at set follow up times might have been a more suitable form of analysis.

There are several other minor flaws and possible confounding factors. For example, the greater use of antibiotic treatment in the placebo group may have led to an equalisation of scores. Moreover, the participation of only one homoeopath (whose training, qualifications, and professional standing are not described) may mean that the modest results were due to his or her inadequacy rather than the inefficacy of the treatment. It also seems to be cutting intellectual corners merely to state that a P value of 0·06 is not significant.

Despite these flaws the trial shows that even if the results of homoeopathic treatment are distinguishable from those of placebo treatment the claims of homoeopaths should not necessarily be taken at face value.[2] An appreciable proportion of the effect of homoeopathy seems to be related to non-specific factors. It is unclear what should be

deduced from this. Does the relative contribution of specific and non-specific factors to a treatment affect how we judge its clinical benefits? Perhaps it would be worth comparing the results obtained by the homoeopath in the trial with those obtained by a conventional general practitioner. This would help us to evaluate the comparative effectiveness of homoeopathy in practice.

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1 De Lange de Klerk ESM, Blommers J, Kuik DJ, Bezemer PD, Feenstra L. Effect of homoeopathic medicines on daily burden of symptoms in children with recurrent upper respiratory tract infections. *BMJ* 1994;**309**:1329-32. (19 November.)
2 Campbell ACH. Children with upper respiratory tract infection. *British Homoeopathic Journal* 1977;**66**:20-5.

Power of study was not estimated

EDITOR,—E S M de Lange de Klerk and colleagues conclude that homoeopathy has little to add to the treatment of recurrent upper respiratory infections.[1] There is a flaw in this conclusion. The authors do not mention the possibility of a type 2 error, which occurs when the null hypothesis is incorrectly accepted when the alternative hypothesis is true. Why was there no estimation of the power of the study? The authors state that "the small difference in symptom score found in favour of the homoeopathic medicines was not significant." This may have been because the numbers of patients were not sufficient for a true difference between the two groups to be detected. I also question whether the provision of advice on nutrition is part of conventional treatment of recurrent upper respiratory infections. Dietary manipulation alone may be a powerful tool in the management of recurrent upper respiratory infections.

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Authors' reply

EDITOR,—The level of significance that we chose was, admittedly, an arbitrary cut off point. The difference in daily symptom score lingered around the 0·05 significance level. The estimate of the difference, however, was in our opinion—and to our disappointment—not clinically relevant. Even the clinical relevance of the upper limit of the 95% confidence interval of the difference in mean symptom scores over a year (0·83) must be questioned.

The power calculation for the trial was based on the outcome measure "change in wellbeing score."[1] After completion of the trial the confidence interval is what matters. The recorded difference in the change in wellbeing score was much smaller than expected.

Because the greater use of antibiotics in the placebo group might have reduced the difference in the symptom scores we did a combined analysis of the symptom score, use of antibiotics, and other interventions. This also gave only small differences between the two groups. The difference between the groups in the reduction in the use of antibiotics compared with the year before participation in the study did not approach significance (P=0·38), and therefore its possible clinical relevance was not an issue for discussion in our paper.

The daily symptom scores were averaged over a year because the children suffered from symptoms half of the time and close monitoring gives more reliable data. Time trends were studied by comparing differences over the last nine months as well as differences over the four quarters of the year separately. No time trend could be found. Adjustment for small differences in prognostic variables at the baseline reduced the difference in the mean daily symptom scores between the groups. As well as analysing mean daily symptom score, we analysed episodes of respiratory tract infection. The placebo group suffered an average of 8·4 episodes covering 47 days, with a mean daily symptom score of 14·1, while the treatment group suffered an average of 7·9 episodes covering 41 days, with a mean daily symptom score of 13·6 (P=0·5).[1]

The parents of most of the children stated that their child's health had improved. This may have been due to growth and development as well as the whole treatment package. Our study, however, concerned the specific effects of individually chosen medicines. The homoeopathic doctor who prescribed these medicines studied at the Faculty of Homoeopathy (a long course), passed the examination for membership of the faculty with honours, and had had 10 years' experience in homoeopathic practice. Dietary advice was given to create optimal conditions for a positive effect of homoeopathic medicine.[2]

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2 Hahnemann S. *Organon of Medicine.* 6th ed. Philadelphia: Boericke and Tafel, 1921, and 1977.

The health of leaders

Should they have occupational health screening?

EDITOR,—Psychiatrists and psychologists are not always natural allies, yet I believe that Ian Robertson was correct to draw attention to Churchill's habitual alcohol consumption.[1] He is not the first to have done so. L'Etang wrote extensively on the physical and mental incapacities of many great figures in history and commented in particular on Churchill's alcohol intake, quoting from respected sources.[2] Like all truly great people, however, Churchill rose above his personal weaknesses when it really mattered. Yes, he made mistakes, as at Gallipoli, but in all crucial decisions Churchill's judgment, with or without alcohol, was right. He insisted that we keep our fighter squadrons in reserve to face the Germans after the fall of France became inevitable. He worked tirelessly to bring the United States into the war on our side, so ensuring eventual allied victory. The fact that he probably drank ≥80 units of alcohol a week should not be taken out of context.

The debate on Churchill's achievements has tended to obscure the real question that Robertson asked—namely, should leaders undergo occupational health screening like the rest of us? I believe that this could have unforeseen consequences, possibly to the detriment of human destiny. As Shakespeare said, "Some men are born great, some achieve greatness, and some have greatness thrust upon them." The vast majority of people live out their lives in mediocrity. A nation does not have an inexhaustible supply of highly talented individuals in any particular field. A policy to screen out from this pool of talent any person with worrisome health problems could lose as much as it gains. Nelson's victory at Trafalgar secured control of the sea for Britain for the rest of the century. How many modern sea commanders are without one arm and one eye? In today's navy I doubt whether Nelson would have been allowed to command an office in a run down naval dockyard earmarked for closure. What, I wonder, would a modern occupational health assessment have made of the deaf Beethoven?

As any historian knows, great events can turn on the smallest quirk of fate. Great discoveries are made by only a few. Human destiny relies on these people being in the right place at the right time. We should be careful that in our eagerness to prevent mishap we do not also discard good fortune and success, which can come from unlikely sources.

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1 Robertson I. Drunk in charge. *BMJ* 1994;**309**:1237. (5 November.)
2 L'Etang H. *Fit to lead?* London: William Heinemann Medical, 1980.

Churchill's biographers disagree over alcohol consumption

EDITOR,—Ian Robertson offers his "sincere apologies" for his "gross error" in describing Churchill as often being dead drunk but then seems to be trying to buttress his "pseudo memory" by selective quotations from biographers known to be unsympathetic to their subject.[1] He might also have quoted Gilbert: "Winston's whisky was very much a whisky and soda. It was really a mouthwash. He used to get frightfully cross if it was too strong;" "He was remarkably moderate. He certainly drank the weakest whisky-and-sodas that I have ever known. . . . In truth in his normal drink the whisky only faintly tinged the soda."[2] These observations from close associates do not suggest that Churchill was addicted to alcohol.

In my view this further article is more illuminating of Robertson's bias and lack of scholarship than of Churchill's problem drinking (if any).

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1 Robertson I. Interpreting problems. *BMJ* 1994;**309**:1519. (3 December.)
2 Gilbert M. *Finest hour.* London: Heinemann, 1983:336.

Use of personal records for research purposes

Identification numbers help maintain confidentiality

EDITOR,—We agree with Nicholas Wald and colleagues that access to medical records is needed for research purposes,[1] but computerised record linkage does not necessarily entail the identification of people by name. The allocation of a unique identification number to each resident, as is currently the case in jurisdictions such as Saskatchewan,[2] provides a robust method for linking relevant records about a person from different databases while minimising problems of confidentiality.[3,4]

Developments in computer applications over the past 30 years have emphasised their value in medicine,[5] and the recent trends towards managed