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.)
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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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### 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