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β agonists are widely used

EDITOR,—In addressing the controversy over the use of β agonists in asthma neither A E Tattersfield nor R W Fuller has really fuelled the debate.¹ Their arguments revolve around whether β agonists cause deaths from asthma and whether they cause increased bronchial hyperreactivity if used regularly. The first question is now largely historical and well aired. If we accept the mounting evidence that β agonists can cause worsening asthma the real issue is then the extent of their use (or misuse). To answer this we need to look at what happens in general practice, where 97% of asthmatic patients are treated² and where patients who are prescribed β agonists either alone or in combination with inhaled prophylactic agents can be identified.

In addressing the problems of potential barriers to prophylaxis against asthma in primary care my colleagues and I have identified six groups of patients in our practice. They range from those taking β agonists alone through those taking both drugs to relieve asthma and drugs to prevent it in varying ratios (unpublished data). In the six partner computerised practice of 10 682 patients (with high awareness of asthma) and an asthma clinic run by a nurse we have 980 patients (prevalence of asthma 9.2%) who during a 12 month study in 1993-4 were prescribed (and requested further prescriptions of) inhaled β agonists or prophylactic drugs. Of these patients, 469 were taking β agonists alone, of whom 103 had used six or more canisters of metered dose inhaler a year. This equates to four doses a day if used properly but to roughly three doses a day if test firing, non-compliance, and wastage are taken into account. In other words, 103 of 980 asthmatic patients were taking three or more puffs of β agonist every day without inhaled prophylaxis and 147 were taking a β agonist twice or more. Many of these patients had previously been prescribed prophylactic treatment but had been non-compliant. Our asthma clinic is now targeting this problem.

Further evidence of the widespread use of β agonists is shown by the fact that, for all eight family health services authorities areas in Wales, prescribing analysis and cost (PACT) data show that Ventolin (salbutamol, Allen and Hanburys) is among the top three drugs prescribed, sharing the top spots with such common drugs as paracetamol and co-proxamol.³ Non-specific cough rather than asthma may be being treated extensively with β agonists.⁴ If other data support the fact that 10-15% of the asthmatic population is being prescribed regular β agonists alone then the extent

of the problem is potentially considerable and can hardly be described as much ado about nothing.

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Scoring system developed for drug related morbidity

EDITOR,—Nick Steen and colleagues describe the development of a symptom based outcome measure for asthma.¹ The Midlands Thoracic Society's research group is developing a simple scoring system for morbidity related to drugs used for asthma. This will be used in asthma clinics to monitor individual patients and audit the clinic. The morbidity measures include scales for the highest peak expiratory flow in the clinic as a percentage of the predicted flow (10 items); the variability in peak expiratory flow in the week before the clinic visit (five items); the use of a bronchodilator—in addition to the doses prescribed regularly—in the 24 hours before the clinic visit (four items); nocturnal symptoms in the week before the clinic visit (seven items); symptoms affecting lifestyle since the last clinic visit (six items); and the extra drugs required to maintain treatment since the last clinic visit or in the two months before the first visit (five items). Drug use is monitored by scales based on the stepwise approach given in the British Thoracic Society's guidelines.²

The questionnaires answered by patients have been shown to be reproducible (Cohen's $\kappa=0.89$; $n=532$).³ The validity and responsiveness to change have been tested in an urban general practice in Stoke on Trent. Fifty patients with a peak flow of <75% of the predicted value attended an asthma clinic three times three months apart. The expected improvement in control of asthma was mirrored by the scores for lifestyle, nocturnal symptoms, use of bronchodilators as required, use of additional drugs, and highest peak flow (table). Scores of variability in peak flow showed an increasing trend. Morbidity scores correlated with those for drug use, with considerably more preventive (anti-inflammatory) treatment being prescribed for each patient at the end of the study than at the beginning.

The system has been integrated into the routine of one general practice and two hospital asthma clinics in the west midlands in paper format and is being integrated into the computer system in one hospital during its evaluation phase.

The morbidity scales reflect the fact that many variables can be used to assess control of asthma,

including symptoms (use of extra drugs) and physiological measurements. Steen and colleagues' "bother" scale supplies only one aspect. Any morbidity scales will probably have to contain absolute measurements since the inappropriately high morbidity that many asthmatic patients experience indicates an adaptive response to symptoms affecting daily living.^{4,5} This may lead patients to underestimate the bother that their asthma causes.

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Simple aspiration for spontaneous pneumothorax

Authors reinvent the wheel

EDITOR,—Simple aspiration of air from a pneumothorax, as recommended by John Harvey and Robert J Prescott, may be likened to the reinvention of the wheel.¹ When I was first appointed a consultant 14 years ago I was impressed by my registrar, O G Raja, who successfully used simple aspiration to treat pneumothorax. It is such an obvious thing to do that I wondered why I and, seemingly, others had not thought of doing it. When Raja published this work with a colleague² it prompted a flurry of similar publications, but the original work is rarely referred to.

Excited by the method, I presented a case managed in this way at a local meeting of chest physicians, expecting a similar response to this "new" technique. I was somewhat deflated by one of the older chest physicians, J Spencer Jones, who pointed out that he had been using the method for several decades. Having lived through the era of artificial pneumothoraces, he—and, I suspect, many others—found it natural to pump air out of the pleural cavity instead of into it. Spencer Jones subsequently published his extensive experience with the technique.³

Pleural aspiration for pneumothoraces has now been standard practice at the William Harvey Hospital for 14 years. I can attest to its effectiveness but do not recommend it for large and tension pneumothoraces, which are often still open. Use of the method renders insertion of an intercostal tube unnecessary in many cases, thereby reducing

Lung function variables and scores at each clinic visit (a reduction in scores denotes an improvement)

	Visit			Difference between visit 1 and visit 3	95% confidence interval of difference between visit 1 and visit 3
	1	2	3		
Highest peak expiratory flow (l/min)	342.5	387	429	86.5	72.3 to 101
Variability in peak expiratory flow (%)	8.40	7.60	8.80	0.39	-1.08 to 1.86
Scoring system:					
Score for highest peak expiratory flow	2.48	1.60	0.94	-1.54	-1.22 to -1.86
Score for variability in peak expiratory flow	0.30	0.22	0.48	0.18	-0.40 to 0.04
Use of bronchodilator	1.56	1.06	0.58	-0.98	-0.70 to -1.26
Nocturnal symptoms	2.08	1.28	0.58	-1.50	-1.19 to -1.81
Lifestyle	1.76	1.04	0.56	-1.20	-0.83 to -1.57
Additional drugs	1.78	1.04	0.30	-1.48	-1.10 to -1.86
Total score	9.96	6.24	3.44	-6.52	-5.37 to -7.67

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

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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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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