LETTERS TO THE EDITOR

HIV related bronchiectasis

In their review of new developments in pulmonary diseases affecting HIV infected individuals (March 1995;50:294-302) Mitchell and Miller do not discuss bronchiectasis. They mention indolent bronchopulmonary *Pseudomonas aeruginosa* infection comparable to that seen in cystic fibrosis, but do not comment on bronchiectasis which is now well described in adults and children with HIV infection.

Bronchiectasis in a series of HIV infected adults was first reported from Oxford in 1992,¹ and the association has subsequently been confirmed in reports from the USA.^{2,3} The incidence of bronchiectasis in the HIV infected population remains to be established; it is frequently undiagnosed because of a low index of suspicion and because chest radiographs may be normal or non-specific. High resolution computed tomographic (CT) scanning is the investigation of choice.²

The aetiology is likely to be multifactorial, but recurrent bronchopulmonary infection is probably one of the most important contributing factors. Some cases have been seen following *Pneumocystis carinii* infection alone, while other cases have been related to various endobronchial lesions. Most cases of bronchiectasis in HIV infected adults are seen following recurrent episodes of pyogenic infection with common pathogens such as *Streptococcus pneumoniae*, *Haemophilus influenzae*, *Moraxella catarrhalis*, and *Staphylococcus aureus*. In paediatric practice bronchiectasis has emerged as a complication of lymphocytic interstitial pneumonitis. A report of the isolation of *Burkholderia* (formerly *Pseudomonas*) *cepacia* from one case of bronchiectasis in an adult with AIDS gives cause for concern.³ This organism has a predilection for the lungs of patients with cystic fibrosis and has caused several epidemics associated with cystic fibrosis. The potential exists for *B. cepacia* to pose a similar threat to HIV patients with chronic lung disease.

At Boston City Hospital in the past year we have diagnosed bronchiectasis in three patients, aged 7, 10, and 16 years, out of 60 children with AIDS. All were boys and had congenitally acquired HIV infection. Each had experienced many previous episodes of pneumonia, and the two younger patients had previously experienced lymphocytic interstitial pneumonitis, based on chest radiographic findings or histopathological examination. Symptoms of chronic productive cough, exercise limitation, and persistent hilar pulmonary infiltrates suggested bronchiectasis, which in each case was confirmed by a chest CT scan. Sputum culture yielded *S. pneumoniae*, *H. influenzae*, non-typable, beta-lactamase negative, and normal flora in the three patients, respectively.

We conclude that bronchiectasis has become a significant problem in HIV infected patients, and has the potential to cause substantial morbidity.

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AUTHORS' REPLY In view of the extensive nature of the subject and the very large numbers of recent publications, we had to be selective in our review. It is therefore timely that Dr Holmes and colleagues have drawn attention to this interesting subset of patients who develop bronchiectasis. We would only add that, although bronchiectasis may be detectable by high resolution CT scanning in some AIDS patients who have had recurrent episodes of pneumonitis, it may not herald a major clinical problem when compared with the other pulmonary complications of HIV disease. If so, one would have expected there to be a substantial literature on this subject by now, and also to have been made aware of it by one's clinical practice, neither of which seems to have occurred. However, with more effective prophylaxis and treatment of the major infective causes of lung disease in AIDS, a new cohort of patients could emerge who live long enough to develop bronchiectasis and we may see this in the years to come.

A different point of potential interest is to distinguish between AIDS patients who have chronic or recurrent bronchopulmonary sepsis due to bacterial pathogens but without structural changes, and those who have genuine bronchiectasis.

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β_2 agonist/antagonist activity of salbutamol

The main conclusion of the paper by Drs Grove, McFarlane and Lipworth (February 1995;50:134-8) that in vivo salbutamol shows characteristics of a β_2 adrenoceptor partial antagonist in a state of high adrenergic tone (exercise) is based upon the observation of similar effects of salbutamol and the non-selective β adrenoceptor antagonist, propranolol, in augmenting exercise-induced hyperkalaemia. The authors state that the mechanism of this augmentation of exercise-

induced hyperkalaemia is by β_2 adrenoceptor antagonism since propranolol and the selective β_2 adrenoceptor antagonist ICI 118 551 would show similar effects in this respect. It is the latter assumption that we would like to challenge. We think that it is more likely that the effect of salbutamol on the exercise-induced increase in potassium is due to some other yet unknown mechanism.

Concerning the effects of ICI 118 551 on exercise-induced hyperkalaemia, the authors refer to a paper on observations of the effects on plasma potassium levels in six healthy volunteers during a game of squash.¹ However, in that paper ICI 118 551 did not influence the exercise-induced increase in plasma potassium levels, as was also concluded by the authors of that paper. The difference in the mean plasma potassium level at the end of the exercise period between the placebo and active treatment groups can be explained by the difference in baseline potassium levels before exercise. Moreover, in another paper a direct comparison between propranolol and ICI 118 551 of the effects on plasma potassium levels during exercise was made under standardised conditions - that is, during incremental ergometer exercise.² In that experiment only plasma potassium levels after pretreatment with propranolol were different from placebo, but plasma potassium levels after ICI 118 551 were identical to those after placebo pretreatment.

On the basis of these two studies, it is very unlikely that blockade of β_2 adrenoceptors is involved in the phenomenon of exercise-induced hyperkalaemia. Either non-specific β blockade or a non- β adrenoceptor-mediated mechanism shared by several drugs of this class is involved. We think that the latter notion makes the conclusion of the paper by Grove *et al* concerning the β_2 adrenoceptor partial antagonist activity of salbutamol at least subject to doubt. In the light of this, the implicit warning for the possible negative effects of salbutamol in the setting of acute asthma is unsubstantiated, particularly since this drug has been shown for many years to be of great clinical value in this potentially life threatening situation.

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- Struthers AD, Quigly C, Brown MJ. Rapid changes in plasma potassium during a game of squash. *Clin Sci* 1988;74:397-401.
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AUTHORS' REPLY We read with interest the letter by Drs Jonkers and Koopmans and wish to make the following comments. Firstly, in the results section of the paper by Struthers *et al*¹ it is stated that pretreatment with ICI 118 551 produced a significantly higher overall exercise potassium response. Although it looks as though the baseline potassium levels were higher after treatment with ICI 118 551, unfortunately no data are given for the delta potassium response. As far as the paper by Gullestad *et al*² is concerned, it does indeed

look at first glance as though ICI 118 551 has no effect on exercise-induced hyperkalaemia compared with placebo. However, the baseline potassium concentration was significantly lower in the ICI 118 551 group than in the placebo group, and this may explain the apparent lack of effect on exercise response. Furthermore, in that study atenolol had no effect on the exercise-induced potassium response, suggesting that β_1 receptors are not involved. Indeed, in our own study salbutamol had no effect on the exercise heart rate response, showing that β_1 blockade did not occur. It has also been shown that a low dose of nadolol (5 mg), which has a minimal β_1 blockade, exhibits marked β_2 blockade as assessed by exercise-induced hyperkalaemia.³ Thus, the fact that in our study both salbutamol and propranolol significantly augmented the exercise-induced potassium response is in keeping with a β_2 receptor mediator mechanism linked to the Na/K-ATPase pump. This hypothesis is further supported by the enhancement of exercise-induced hyperkalaemia by digitalis which directly inhibits the Na/K-ATPase pump.⁴

We therefore remain of the firm opinion that, in the presence of high adrenergic tone, salbutamol exhibits β_2 antagonist activity as demonstrated by augmentation of exercise-induced hyperkalaemia. This would be in keeping with the known pharmacological properties of salbutamol as a partial β_2 agonist along with in vitro data showing salbutamol to antagonise the relaxant effect of isoprenaline in the presence of carbachol-induced bronchoconstriction.⁵ Whilst we agree that salbutamol is of great value in treating acute bronchospasm, it may be that a β_2 agonist with greater intrinsic activity may be preferable in certain situations where airway tone is increased.

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Paradoxical vocal cord adduction in cystic fibrosis

Woe to the authors who claim a first (June 1995;50:694-5), and to the editor and referees who allow the claim to be published! Paradoxical vocal cord adduction has indeed been described before in cystic fibrosis¹ and cited in a recent review.² It is not exclusively seen in girls; our own experience in asthma (in preparation) and that of others³⁻⁵ includes boys with the condition. Useful pointers in

the history include the disappearance of symptoms when asleep, and, in the investigations, a significant disparity between the results of bedside spirometric tests and those obtained by a really experienced lung function technician. We agree that the possibility of this condition should be considered in patients with known proven airway disease, preferably at an early stage, before too many unnecessary investigations and potentially toxic treatments have been inflicted on the sufferer.

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BOOK NOTICES

ABC of Asthma. 3rd edition. John Rees and John Price. (Pp 49; £8.95). London: BMJ Publishing Group, 1995. 0 7279 0882 0.

This book is one of a series of ABC publications produced by the BMJ Publishing Group, and is the third edition devoted to asthma in adults and children. Some chapters have been reproduced in recent editions of the *British Medical Journal*.

The book is divided into two sections, the first and longer one pertaining to adult asthma and the second to asthma in children, although there is of course some overlap between the two. Chapters cover the definition, diagnosis, prognosis, and treatment of acute and chronic asthma – the latter based on guidelines recently produced by the British Thoracic Society. Subsections within chapters are clearly labelled, short for easy reading, and each has an associated illustration.

Recent interesting insights into risk factors for childhood asthma and hence prospects for prevention are discussed, as are other topical issues in asthma management such as the safety of β_2 agonists and CFC-free metered dose inhalers, although gastro-oesophageal reflux is not mentioned in an otherwise excellent chapter on precipitating factors of asthma. The possibility of further reading into these subject areas is limited, however, by the absence of a bibliography.

One minor criticism is that some inconsistencies in terminology may be confusing for those unfamiliar with the subject. For example, the authors use the term "airway responsiveness" interchangeably with "bronchial reactivity", similarly β_2 agonists are variably described as β stimulants, β_2 adrenergic agonists (and even β antagonists at one point).

Overall, the book provides a simple over-

view of asthma which medical students, junior doctors, nurses, and allied professionals will find up to date and easy to read. – HB

Patient Education and Chronic Obstructive Pulmonary Disease. Annelees H S van den Broek. (Pp 158; £12.00). Netherlands: Section of Clinical and Health Psychology, Leiden University, 1995. 90 802379 2 2.

This little book is laid out and reads like a PhD thesis. Essentially it consists of a literature review followed by detailed presentation of the results from a study of the effects of a patient education group programme during pulmonary rehabilitation. It contains useful references and a few stimulating ideas, but it lacks maturity. Many of the data are presented uncritically with no attempt at a distinction between clinically important and statistically significant findings. The book cannot serve either as an authoritative review of the field or as a primer for those wishing to set up a programme. Someone experienced in the field might find useful nuggets contained within it, however. The potential reader is advised to examine it before purchase.

The book comes from Holland and reflects the powerful influence of "the Dutch hypothesis" about the underlying processes in asthma and COPD. In places, where convenient, it treats the two as part of the same disease – that is, chronic non-specific lung disease (CNSLD). However, this approach is frequently abandoned and training programmes specifically for "asthma" or "COPD" are described. This ambiguity is not the fault of the author who is a non-clinician. It does raise a question of the practical value of a term that encompasses two conditions that, by implication, may be sufficiently well distinguished to demand different educational approaches.

No price was attached to my review copy, but it should be cheap. The translation from the Dutch is idiosyncratic in places, although perfectly comprehensible. If non-English work such as this can be made readily available in English at a modest price, then I think that imperfect grammar is acceptable. No matter how cheap the product, however, there is no excuse for a publisher to produce a book with a large number of spelling mistakes that could easily have been corrected using any word processor in a few minutes. – PJ

CORRECTION

Inadvertent duplicate publication

Parts of the review article by Drs R F Miller and D M Mitchell that appeared in *Thorax* 1995;50:191-200 on *Pneumocystis carinii* pneumonia were the same as a previously published review article by Professor J R Stringer in *Infectious Agents and Disease* 1993; 2:109-117. This was not known to the Editor of *Thorax* as the responsibility for producing and editing the review article was that of the series editors. The journal and the responsible author (Dr R F Miller) sincerely regret this breach of editorial ethics.