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CORRESPONDENCE

Azithromycin in cystic fibrosis

To the Editors:

We have read with interest the article by SOUTHERN and BARKER [1]. The authors critically reviewed the evidence from the randomised, controlled trials of the role of azithromycin in the management of cystic fibrosis (CF). In the three randomised, controlled studies described, the effects of up to 6 months of therapy with azithromycin in CF have been reported [2–4]. However, the controlled setting of randomised trials may differ from the clinical practice. Therefore, we can understand the authors' cautious closing remark on the precise role of azithromycin in the clinical setting.

We have recently audited our use of long-term azithromycin therapy in adult patients attending the Regional Adult CF unit, Leeds, UK [5]. A total of 36 patients (17 female), all of whom were colonised with *Pseudomonas aeruginosa*, had 500 mg of azithromycin 3 times·week^{-1} for a mean±SD period of 9±6 months. Mean age, Northern chest radiograph score, Shwachman-Kulczycki score and forced expiratory volume in one second (FEV_1) were 24±7 yrs, 12±3, 68±14, and 1.9±0.9 L, respectively. In the group as a whole, there was no overall change in lung function during the treatment period. However, we were able to identify a subgroup of 24 patients in whom therapy with azithromycin was associated with a mean±SD increase in FEV_1 by 0.3±0.2 L. This group had significantly lower levels of antibodies to *P. aeruginosa* ($p=0.007$), with a median (range) of 19 (4–136) *versus* 40 (5–325) U·mL^{-1}, and a lower number of exacerbations requiring *i.v.* antibiotics in the 12 months prior to starting azithromycin (3 (1–6) *versus* 5 (2–7); $p=0.01$) when compared to those patients where lung function remained unchanged.

We believe that our findings provide additional information on the clinical use of azithromycin. First, not all patients colonised with *P. aeruginosa* improve with azithromycin therapy. Secondly, the CF patients in whom treatment with azithromycin is more likely to be associated with improvements in lung function are characterised by a lower immunological response to *P. aeruginosa* and a lower frequency of exacerbations.

Our data suggest that therapy with azithromycin should be considered earlier in the disease process. Further data on whether therapy should be introduced before colonisation with *Pseudomonas aeruginosa* occurs is required.

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From the authors:

J.A. Kastelik and colleagues propose early use of azithromycin for cystic fibrosis (CF) based on subgroup analysis of 24 adult ''responders'' in their clinic who had lower *Pseudomonas aeruginosa* antibodies and less respiratory exacerbation prior to starting azithromycin compared to 12 nonresponders. Would these adult responders with mild respiratory disease have experienced a mean 0.3 L improvement in forced expiratory volume in one second with placebo? Subgroup *post hoc* analysis of retrospective data is fraught with bias and interpretation must be circumspect. The bottom line is we do not know why azithromycin improves respiratory function in people with CF and we do not know when to prescribe it. There is no clinical evidence, as yet, of an indirect anti-pseudomonal effect and, furthermore, the data of KASTELIK *et al*. [1] do not appear to support this assertion.

Is it possible that the widespread use of azithromycin in CF clinics throughout the world represents long-term anti-staphyloccocal chemoprophylaxis being introduced *via* the back door? If so, we need to be cautious, as azithromycin has potential for generating resistance. This may have profound long-term effects on our choice of antimicrobials. There are probably better long-term anti-staphylococcal agents with a narrower spectrum of action and less tendency to promote resistance.

It is important that cystic fibrosis teams introduce azithromycin in a structured manner and audit their results, as J.A. Kastelik and colleagues have done. Close liaison with the microbiology department to monitor clinic resistance patterns is essential.

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Value of imprint cytology for ultrasound-guided transthoracic core biopsy

To the Editors:

LIAO *et al*. [1] have shown that imprint cytology of ultrasound-guided transthoracic core biopsy is a sensitive procedure for diagnosing peripheral thoracic lesions. Similarly, we have recently reported the results of our experience of using touch imprint smears prepared from computerised tomographic-guided core needle lung biopsies [2]. We correlated the cytological diagnosis of touch imprint smears with the histological diagnosis of the corresponding core needle-biopsy specimen, which was taken as the gold standard. There were no false-positive results, and all patients with small cell lung cancer were correctly diagnosed from the imprint smear. We agree that the technique is a quick, sensitive and highly specific method of detecting lung malignancies, and this may be particularly important with small cell lung cancer, where one may wish to start urgent chemotherapy pending the result of formal histology.

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Flow limitation and dynamic hyperinflation

To the Editors:

In the January issue of the *European Respiratory Journal*, in a very well-written paper, CALVERLEY and KOULOURIS [1] comprehensively reviewed the concepts of expiratory flow limitation and dynamic hyperinflation, and assessed their potential importance in pulmonary disease.

They underlined the fact that, in nonintrathoracic airflow-limited snorers or obstructive sleep apnoea–hypopnoea syndrome (OSAHS) patients, the higher collapsible upper airway could promote an expiratory flow limitation.

Our group recently published data on the operating characteristics of the negative expiratory pressure (NEP) technique in the prediction of OSAHS in 238 snoring patients free of pulmonary and cardiac disease [2].

The findings of the study were that in the supine position: 1) the degree of instability of the upper airway measured by the NEP was correlated with the severity of OSAHS; and 2) using an expiratory flow-limitation cut-off of 27.5% of the tidal volume, NEP had moderate sensitivity (81.9%) and specificity (69.1%) in the prediction of OSAHS, defined as an apnoea–hypopnoea index $\geqslant 15 \cdot h^{-1}$.

It should be noted that, in agreement with TANTUCCI *et al*. [3], we found that only 44% of the subjects with primary snoring (OSAHS $<5 \cdot h^{-1}$) did not exhibit an expiratory flow limitation when the NEP was applied, and 23% demonstrated an expiratory flow limitation $\geqslant$27.5% of the tidal volume.

Although further studies need to confirm these results, negative expiratory pressure may be a diagnostic tool in the work-up of obstructive sleep apnoea–hypopnoea syndrome.

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