We agree that the additive effects of concomitant, chronic colonisation of the lungs with *Pseudomonas aeruginosa* is associated with identifiable risk factors such as age, ethnicity, co-morbidity, or medications such as immunosuppressive therapy. In addition, further research is required to determine the value of interferon-γ assays for the diagnosis of LTBI in this patient population, given the limitations of TB skin test and risk assessments.

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References

Author's reply
I thank Dr Creer and colleagues for their interest in our letter and welcome the opportunity to characterise our patients better and to make some additional comments.

All of the 69 rheumatological patients that we screened for TB infection were white, born in Italy, and none had previously received BCG vaccination. The high prevalence of latent TB (24.6%) was predominantly due to the frequent observation of radiographic lesions consistent with this diagnosis (20.3%). The radiologist had been specifically asked to look for this kind of lesion, and this probably enhanced the sensitivity of its evaluation compared with a "blind" routine observation. However, it must be stressed that there were radiographic abnormalities consistent with previous TB that are rather non-specific such as pleural scarring. This confirms the need for an evaluation on an individualised basis, and the recently published BTS recommendations are very helpful to the clinician managing this highly topical subject.

I agree that interpretation of the Mantoux test can be valid in this group of patients. In our 69 patients, although we stopped steroids and all immunosuppressive agents at least 1 week before performing the Mantoux test, this action did not preclude a significant chance of false negatives. Furthermore, the BTS guidelines stress the importance of ethnicity and place of birth in assessing the annual risk of TB and the need to take into account the risks of chemoprophylaxis before initiating treatment.

As surveillance studies suggest that the airways of healthy children are rarely colonised with *P. aeruginosa*, healthy individuals are not regarded as a potential source of *P. aeruginosa* acquisition. In addition, there is no published evidence that acquisition of *P. aeruginosa* in CF patients is advocated to prevent cross infection. As surveillance studies suggest that the airways of healthy children are rarely colonised with *P. aeruginosa*, healthy individuals are not regarded as a potential source of *P. aeruginosa* acquisition. In addition, it has been shown that acquisition of *P. aeruginosa* in CF patients is often preceded by a viral respiratory infection. We hypothesised that the incidence of *P. aeruginosa* acquisition during periods of acute respiratory infections (ARI) is equal in both healthy and CF individuals, and considerably exceeds the prevalence in asymptomatic children shown in surveillance studies.

We performed systematic oropharyngeal cultures during periods of ARI between November and May in 20 young children with CF of mean (SD) age 3.6 (2.0) years (range 0.1–7.4) and 19 unrelated age matched healthy controls of mean (SD) age 3.6 (1.7) years. All children were negative for *P. aeruginosa* at the start of the study. Subjects were contacted twice a week with a standard questionnaire regarding symptoms and any symptoms were present a physician performed an oropharyngeal culture. Cultures were negative for *P. aeruginosa*.
Cultures following a positive culture in 
P. aeruginosa healthy controls, respectively. During the 
five visits. The study was approved by the local ethics 
review committee and all parents of the children gave 
written informed consent. This study showed that 
P. aeruginosa acquisition frequently occurs in periods of 
ARI in both children with CF and healthy controls. While 
healthy individuals easily clear 
P. aeruginosa, most CF patients remain positive 
and require anti-pseudomonal treatment. In 
the present study we sampled during periods 
and require anti-pseudomonal treatment. In 
the present study we sampled during periods

**References**


**Association between sibship size and allergic diseases in the Glasgow Alumni Study**

We read the interesting study by Kinra et al which gives us important information on the relationship between sibship size, birth order, and allergic disease in British students born in the first half of the 20th century. There are, however, a few points which we would like to raise:

1. The authors observed a stronger association between sibship size and allergy in the oldest cohort and interpreted this finding as supporting the hygiene hypothesis because of a postulated larger difference in hygiene between larger and smaller families in this cohort compared with younger cohorts. However, another possible explanation—not related to the hygiene hypothesis—is the change of determinants of family size. With modernisation and emancipation of women and the discovery of the biochemical rhythm in the female reproductive cycle and the increasing popularity of condoms, all taking place in the first half of the 20th century, the determinants of family size may have shifted considerably during this period with probable consequences for the association between family size and allergy.

2. Similarly, an interaction between socio-economic status (SES) and birth order was interpreted as “supporting observation” by ———— supporting the hygiene hypothesis. However, other explanations cannot be excluded if we assume a prenatal birth order effect: a stronger relationship between birth order and allergy in lower SES categories might be due to a possibly higher rate of spontaneous abortions in these groups, leading to differential underestimation of birth order (or, rather, number of pregnancies). This scenario would also explain the fact that such an interaction was not observed for sibship size.

In the comparison of the results with those of other studies, the authors point out that two “negative” studies were due to lack of power. Firstly, it should be noted that these studies were not negative. In the study by Jarvis et al a significant negative association (adjusted for birth order and relevant determinants) between allergy and sibship size was found, while in our study the corresponding association with birth order was highly significant. Secondly, in our study the adjusted association with sibship size was indeed not significant (p value for trend 0.34), but the adjusted odds ratio (OR) for one extra sibling (allergy/no allergy) was positive (1.07) while its 95% confidence interval (95% CI) of 0.85 to 1.34 excluded any important negative trends (OR and 95% CI for trend not shown in paper).

(4) The contents of table 2 are not in agreement with the title: the results for asthma and combined allergic diseases are not shown.

**Competing interests:** none

Dr Kinra was asked to comment, but no reply was received by the time this issue of Thorax went to press.

**References**


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In the paper entitled “COPD exacerbations – a neglected problem?” by S Scott, P Walker and P M A Calverley which appeared in the May issue of *Thorax* (2006;61:440–7), the dose of iotropium used in the studies by Casaburi and Brusasco referred to in table 1 on page 444 which currently reads “18 µg twice daily” should read “18 µg once daily”. The publishers apologise for this error.
High *Pseudomonas aeruginosa* acquisition rate in CF

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