lung cancer according to the Liverpool Lung Project risk model, which will miss the opportunity to study the impact of screening in a low risk group population, and further the Liverpool Lung Project model may not take into account the separate risk factors working in different ethnic populations[4] thus making its prediction less reliable in the cosmopolitan UK population where according to the 2001 census >15% of the population is non-white British.[5]

We hope that the investigators are open to our suggestions and will accept our ideas if they find some relevance in them.

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Authors' response

We thank Dr Tournoy for his interest in our article on the utility of endobronchial ultrasound-guided transbronchial needle aspiration (EBUS-TBNA) for the diagnosis of tuberculous intrathoracic lymphadenopathy.[1]

With regard to the patient selection in our study, we have acknowledged in the Discussion that the characteristics of patients with intrathoracic lymph node tuberculosis not submitted for EBUS-TBNA are unknown. This selection bias is an inherent problem in retrospective cohort studies, as only those patients suitable for EBUS-TBNA are included. Dr Tournoy would like to know the sensitivity of EBUS-TBNA for all cases in which tuberculous lymphadenitis is suspected; however, this is not reliably obtainable from a retrospective study. We have recently completed the recruitment to a prospective trial of EBUS-TBNA in patients with isolated mediastinal lymphadenopathy, which aims to answer this question (ClinicalTrials.gov identifier: NCT00932854).

We do not agree with Dr Tournoy's assertion that restricting the sample to patients with the condition overestimates the sensitivity. Indeed, the definition of sensitivity of a test is the number of patients diagnosed by the test (true positives) divided by the total number of patients with the condition (true positives + false negatives). Therefore, including patients without tuberculous lymphadenitis would not affect the analysis.

Dr Tournoy states that epithelioid granulomas without caseation are more likely to be consistent with sarcoidosis. However, this is based on a cohort of patients with suspected sarcoidosis and a low prevalence of tuberculosis.[2] In addition, he does not acknowledge the data from our article which show that this pathological criterion was associated with a positive culture of *Mycobacterium tuberculosis* in 34% of cases. The finding from our study that non-caseating granulomas obtained from EBUS-TBNA are consistent with tuberculosis in a setting of moderate to high disease prevalence is an important one. We are puzzled by the remark that lymph node necrosis (seen in the absence of malignant cells) is primarily consistent with cancer without reference to the prevalence of tuberculosis and malignancy in the population. Five out of the eight patients with lymph node necrosis in our study were culture positive for *M tuberculosis*. EBUS-TBNA is in clinical use in populations with high tuberculosis prevalence (eg, India), where the dogma presented by Dr Tournoy may not apply.

Finally, we do not agree that a limit should be set on the sensitivity of EBUS-TBNA for the diagnosis of tuberculous lymphadenitis. The emergence of rapid molecular techniques[3] has the potential to improve microbiological yield further from EBUS-TBNA samples and this is also currently under investigation. We believe that EBUS-TBNA can provide clinicians with strong pathological evidence and microbiological proof of intrathoracic lymph node tuberculosis. Clinicians will already be aware that if microbiological results are negative, the predictive values of EBUS-TBNA pathology grades (none of which are entirely specific for tuberculosis) will depend on the context of the clinical features and the prevalence of tuberculosis in the population to which the patient belongs.

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