

BTS guidelines need to include IFN- γ release assay (T SPOT and QTG) alone or in combination with TST. Reactivation of LTB is associated with morbidity and mortality, and not screening for LTB should not be an option, even in regions with low prevalence of LTB.

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Comment on: Screening for *Mycobacterium tuberculosis* prior to anti-TNF therapy—an audit of impact of the British Thoracic Society guidelines on rheumatology practice in an area of low *Mycobacterium tuberculosis* prevalence: reply

SIR, We write in response to the comments made by Batsi Chikura and colleagues [1] and would like to emphasize the fact that our audit was a retrospective case note analysis of patients screened for *Mycobacterium tuberculosis* prior to August 2005 [2], when the British Thoracic Society (BTS) guidelines were not existent. The diagnosis of latent tuberculosis (LTB) in our cohort of patients was a clinical one and was made during the time when the IFN- γ release assay (T-SPOT) were non-existent and even now there exists an inherent difficulty in making a diagnosis of LTB as might be agreed even by some chest physicians.

We do concede that there may be some underestimation of prevalence of *M. tuberculosis* in our group of patients, as their BCG vaccination status was not accurately recorded and postulate that at least some difficulties were to us having a cohort of elderly patients. As one might be aware, the BCG vaccination was first introduced in the UK only in the 1950s and also that the Joint Committee on Vaccination and Immunization recommends vaccination only in infants living in areas where the incidence of *M. tuberculosis* is $\geq 40/100\,000$ persons (<http://www.immunisation.nhs.uk/publications/CMO060705.pdf>). It would be over interpreting the significance our study to conclude that no LTB screening should be done.

Do we know enough to suggest a change in BTS guidelines? We would think not. And we certainly agree that we do not know

enough from our study to suggest a change in the BTS guidelines. But we can suggest that the BTS guidelines might not be as relevant in areas with lower prevalence of MTB as opposed to high prevalence areas and that risk data have to be recalculated accordingly to avoid overestimation of MTB risk.

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Comment on: Analgesic effects of treatments for non-specific low back pain: a meta-analysis of placebo-controlled randomized trials

SIR, Much well-intentioned back pain research seems to have been in vain. The meta-analysis of treatment of non-specific low back pain by Machado *et al.* [1] serves as an excellent review.

Most importantly, only 76 treatment studies were found suitable for comparison among 1031 involved; even then treatment effects generally appeared small. Their discussion homes in on the research design problems including the lack of subgroup analysis and selection of outcome criteria. They conclude that it is unclear as to how the progress will be made. However, I would suggest that the very elements that they focus on in their discussion are the keys to elucidating effective intervention.

Actual examples of these key elements have been reviewed recently in a monograph [2] and can be summarized briefly. The importance of the control comparison group cannot be emphasized enough.

However, the patient's subjective assessment of pain severity using one version or another of the analogue pain scale is almost invariably the primary observation in such studies. It almost seems impossible to imagine that pain is not the most important element of such morbidity. Sadly, the subjective pain scale is so badly influenced by other factors, such as the effect of recent changes in pain levels and other spurious confounders, that the measure is feeble. To compare such measures from before and after treatment and use the difference as an outcome measure does nothing to mend the basic inadequacy of the mishmash of subjective effects. Hence the other measures of outcome prove superior, though this will not be evident unless several such