PostScript

LETTERS

Tuberculous meningitis: more evidence for protective effect of BCG

Walker *et al*¹ rightly concluded that the bacille Calmette–Guérin (BCG) vaccination does have a noteworthy protective effect against tuberculous meningitis (TBM). However, it might be worth pointing out that the analysis does not include a few recent studies, which lead to the same conclusion based on robust data.^{2–4} This is presumably because the literature search by the authors was carried out in January 2005.

A recent hospital-based case-control study showed that an absent BCG scar was a significant independent predictor for the occurrence of TBM (adjusted odds ratio 1.98; 95% confidence interval 1.09 to 3.57).² The meta-analysis mentioned by the authors is from 1993.⁵ A more recent meta-analysis took an interesting slant on the issue and came to the conclusion that one case of TBM will be prevented for every 3435 vaccinations.³ Although this is certainly an impressive figure, region-specific analysis shows that the maximum benefit is for South-east Asia (46%), where the risk of tuberculosis is highest.³ Even among patients with TBM, vaccinated children seem to have a better outcome.⁴

Although these data do not change the conclusions reached by the authors, they are sufficiently impressive to add more strength to the analysis. Evidence-based paediatrics, even with all the resources at our disposal, is an ever-changing field. The evidence and the conclusions drawn are subject to change during the significant lag phase between submission of articles and actual publication.

M A Anjay

Paediatrics, Airedale General Hospital, Steeton, Keighley, West Yorkshire, UK

P Anoop

Paediatric Haemato-oncology, Royal Marsden Hospital, Surrey, UK

Correspondence to: Dr Anjay M A Pillai, Flat 32, Teal House, Skipton Road, Eastburn, Keighley, West Yorkshire BD20 8US, UK; anjayma@gmail.com

doi: 10.1136/adc.2006.109363

Competing interests: None declared.

References

- 1 Walker V, Selby G, Wacogne I. Does neonatal BCG vaccination protect against tuberculous meningitis? *Arch Dis Child* 2006;91:789–91.
- 2 Kumar P, Kumar R, Srivastava KL, *et al*. Protective role of BCG vaccination against tuberculous meningitis in Indian children: a reappraisal. *Natl Med J India* 2005;18:7–11.
- 3 Trunz BB, Fine P, Dye C. Effect of BCG vaccination on childhood tuberculous meningitis and military tuberculosis worldwide: a meta-analysis and assessment of cost-effectiveness. *Lancet* 2006;367:1173–80.
- 4 Kumar R, Dwivedi A, Kumar P, *et al*. Tuberculous meningitis in BCG vaccinated and unvaccinated children. *J Neurol Neurosurg Psychiatry* 2005;76:1550–4.

- 5 Rodrigues LC, Diwan VK, Wheeler JG. Protective effect of BCG against tuberculous meningitis and military tuberculosis: a meta-analysis. *Int J Epidemiol* 1993;22:1154–8.

Clarifying conflicts of interest in research

Having read the research article published by Greenough *et al*,¹ regarding home oxygen use and healthcare utilisation, I was surprised to see that the fact that Abbott Laboratories had funded the research nurses for the study was only included as an acknowledgement rather than a potential conflict of interest. Abbott Laboratories have a very strong commercial interest in children with bronchopulmonary dysplasia, the subjects of this study, as they are the manufacturers of Synagis (palivizumab), an extremely costly and heavily marketed product used in this group of children. Funding of staff by charitable foundations could appropriately be listed as an acknowledgement, but commercial organisations rarely spend money unless there is direct or indirect potential for gain, and surely this is at least a potential conflict of interest for the research team. Those of us who have worked in paediatrics in the UK may automatically be aware of the commercial interest of Abbott Laboratories in ex-premature infants, but the *Archives of Disease in Childhood* has an international readership and has a responsibility to its wider audience to clarify these issues.

Ruth V Reed

Correspondence to: Dr R V Reed, Barnet, Enfield and Haringey Mental Health NHS Trust, 58–60 Silver Street, Enfield EN1 3EP, UK; ruthvreed@gmail.com

doi: 10.1136/adc.2006.109561

Competing interests: None declared.

Reference

- 1 Greenough A, Alexander J, Burgess S, *et al*. Preschool healthcare utilisation related to home oxygen status. *Arch Dis Child Fetal Neonatal Ed* 2006;91:F337–141.

Author's response

We were extremely surprised by Dr Reed's letter regarding our paper, so we wondered if she had read it thoroughly.

She suggests there may be a conflicting interest. *The Archives of Disease in Childhood* rather terms this competing interest, which they define as "when professional interest (such as patients' welfare or the validity of research) may be influenced by a secondary interest (such as financial or personal rivalry)".

Abbott Laboratories is an international company (not UK-concentrated as implied by Dr Reed), which markets palivizumab as a respiratory syncytial virus (RSV) prophylactic agent for use in prematurely born infants with or without bronchopulmonary disease, and in term-born infants with certain types of congenital heart disease.

Our paper was about the healthcare utilisation of preschool children who had or had not

required home oxygen on discharge from the neonatal intensive care unit—it was not about RSV prophylaxis.

The joint committee on vaccination and immunisation, already in 2002, following the paper by Thomas *et al*¹ working at St George's hospital, has recommended RSV prophylaxis to prematurely born infants with bronchopulmonary disease who had been discharged home on oxygen, as Thomas has shown this was cost neutral.

Thus, we conclude that Dr Reed's implied criticism of *Archives* is unfounded and hence unjust.

A Greenough

Correspondence to: Professor A Greenough, King's College London School of Medicine, UK; anne.greenough@kcl.ac.uk

Competing interests: None declared.

Reference

- 1 Thomas M, Bedford Russell A, Sharland M. Hospitalisation for RSV infection in ex-preterm infants—implications for use of RSV immune globulin. *Arch Dis Child* 2001;84:122–7.

Splenectomy in cystic fibrosis

Splenectomy in cystic fibrosis remains controversial, mainly because the spleen is considered indispensable in this disease for its important immunological function in defence against frequent infections. Furthermore, centres that perform liver transplantation prefer this approach even after early cystic fibrosis-related liver disease becomes obvious, as it seems more aetiological.

The article by Linnane *et al*¹ is an interesting contribution to this debate. Our recently published findings² largely confirm that article although we have doubts on the correctness of the reported indications. Although several of the >500 patients we have followed up over the past 50 years had a gigantic spleen, splenic rupture was never observed, and at splenectomy the surgeons each time recorded a firm thickening of the splenic capsule. In the literature, spontaneous splenic rupture is mostly described in association with infections, inflammatory disease or malignancy—that is, conditions in which the spleen enlarges over a short period of time—whereas no such association is reported in cystic fibrosis. We therefore question "risk of rupture" in itself as a true indication for splenectomy. It might be difficult to decide on the appropriate timing for operation when this is the main indication, as no information is available on the fragility of variably enlarged spleens.

Although we agree that low platelets are indeed no single indication for splenectomy, it can be justified in individual cases because of the risk of complications. A 19-year-old patient recently underwent splenectomy with splenorenal shunt out of fear of cerebral haemorrhage. He had a thrombopenia of 35 000/mm³, and frequent cutaneous bleeding after severe cough attacks. His spleen span was 25 cm, and placement of transjugular intrahepatic