LETTERS TO THE EDITOR

BCG vaccination by multipuncture method

I write in response to the article by Al Jarad et al on this topic. The first study to compare the efficacy of BCG vaccination and its side effects using the Bignall multipuncture device with the reusable handle and disposable heads was the pilot study of neonatal BCG vaccination carried out in 1992 for the Department of Health in our health authority.1

In previous studies in neonates and children under two, referenced in the paper by Al Jarad et al,1 an 18–20 needle percutaneous head gave approximately the same degree of tuberculin conversion as did intradermal vaccination but, to achieve this in older children and adults, 36–40 punctures were required. This would require either a 40 needle head, a double vaccination with two × 18–20 needles. This is why percutaneous BCG is currently only licensed for children aged under two years. Although in neonates1 and in Al Jarad’s study1 in older children the rate of tuberculin conversion was lower with percutaneous than with intradermal vaccination, tuberculin conversion does not necessarily equate to lower efficacy. In the early studies on BCG the protective efficacy of the vaccination was related to the presence of a scar after vaccination, but not to the tuberculin test result after vaccination. Those with a BCG scar but a negative post vaccination tuberculin test— that is, no tuberculin conversion—had the same degree of protection against tuberculosis over the 15 years following vaccination as did those with a scar and a positive post vaccination tuberculin test.1

The multipuncture method is undoubtedly easier to use in neonates because their very thin skin makes intradermal vaccination difficult, and also in nervous teenagers. Further multipuncture method in schoolchildren: a comparison with the intradermal method. Thorax 1999;54:762–3.


AUTHORS’ REPLY To our knowledge our study1 was the first to compare the Bignall device with the conventional device in the multipuncture technique in schoolchildren. We were interested in assessing its efficacy in this particular group as we felt that the multipuncture technique would allow us to protect more schoolchildren of London where it is difficult to access this population. The studies by Cundall et al2 and later by Ormerod and Palmer3 made the same comparison in neonates and small children.

We agree (and stated) that the 18 needle device may not be sufficient to convey a similar conversion rate of the tuberculin test. The manufacturers were unable to produce 40 needle heads as they would require an unacceptably high pressure on the handle to release the needles. We felt that applying two successive punctures with an 18 needle head on the same skin area would not be practicable as the head comes off and would need to be changed after each application. In addition, schoolchildren (and the operators) would not appreciate two applications.

Dr Ormerod’s statement on the BCG scar being a predictor of protection may be appropriate for the intradermal method. In our study the BCG scar in children who received the multipuncture method was not visible in under one fifth of children. Dr Ormerod is in agreement with our statement that the conversion of the tuberculin test does not equate to protection from tuberculosis, but it is frequently used as an indirect measure of the efficacy of BCG vaccination.

We strongly support the PHLS system for enhanced tuberculosis surveillance in the UK, but unfortunately we do not hold out Dr Ormerod’s optimism that it will indicate that unselective BCG vaccination can be discontinued in boroughs and counties where notification rates of tuberculosis are high. Further studies on the protective values of multipuncture BCG may still be appropriate.

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Respiratory care units for non-invasive mechanical ventilation in motor neurone disease

We read with interest the review by Polkey et al pointing out the need to use all means possible to enable patients with motor neurone disease to achieve the best quality of life.

The authors state that, in order to maintain 24 hour ventilatory support, nasal ventilation must be complemented with alternative strategies during the day that are not suitable for widespread use in district general hospitals. We consider that it is possible to maintain 24 hour non-invasive ventilation in patients with motor neurone disease if nasal ventilation is combined with other non-invasive techniques such as mouth piece ventilation or a pneumobelt, and with manual or mechanical expiratory muscle aids to clear secretions in those patients whose weakness makes spontaneous coughing ineffective.1 It is important to provide this treatment early because they can delay tracheostomy and additional problems in most patients with motor neurone disease and are the only way for those patients who reject tracheostomy but want ventilatory support. However, we agree with Polkey et al that this treatment must be performed by trained staff in respiratory care units. Moreover, these units are the best place to prevent respiratory morbidity and mortality, to enhance cooperation between patients, relatives and caregivers, and to manage clinical and psychological problems during the terminal phase of the disease.

In our experience the care of patients with motor neurone disease outside respiratory care units needs to be improved. These patients must not be negatively discriminated against compared with other chronic patients receiving even more expensive but socially accepted treatment. We must therefore try to ensure that all patients with motor neurone disease have access to management in a respiratory care unit in order to receive standardised quality care both in hospital and at home.

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AUTHOR’S REPLY We thank Dr Servera and colleagues for their interest in our paper. We agree that patients with motor neurone disease should have access to specialist expertise where this is necessary. However, we are also conscious that travel can be difficult for some patients with advanced disease and our experience is that, in many cases, satisfactory palliation can be achieved using non-invasive positive pressure ventilation alone. This treatment could theoretically be


provided by an interested chest physician working in a district general hospital. We recognise that, in practice, it may be difficult to identify the necessary resources and that, conversely, an under-resourced service may lead to suboptimal care; however, this is true both of district hospitals and specialist centres.

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Asthma deaths in Scotland and in Wales

It is surprising to say the least that, although the two inquiries into asthma deaths published recently in Thorax \(^1\) made the point that most asthma deaths occurred outside hospital, the finding that asthma is a relatively rare disease (the “relative rarity” of deaths in hospital), neither addressed the question as to whether more prompt admission to a hospital with respiratory intensive care facilities could have prevented some, or even many, of the domestic deaths.

The Respiratory Unit at the Northern General Hospital in Edinburgh first addressed that question as long ago as 1968 when it inaugurated a self-admission scheme for patients known to the unit to be subject to life-threatening attacks of asthma, whereby the often long delays inherent in conventional admission procedures were bypassed with the willing cooperation of their general practitioners. The scheme was more fully described in 1975 \(^2\) and reports on 10 year and 15 year reviews of its progress were published in 1987 \(^3\) and 1988. \(^4\) These showed that the death rate in patients admitted under the scheme was only 0.3%, substantially lower than that recorded in asthmatic patients admitted to other Edinburgh hospitals which relied on the conventional admission procedures.

The asthma self-admission scheme was widely welcomed as a measure which could save lives and was copied in many other countries, including Australia. Yet in neither of the studies reported in the November 1999 issue of Thorax was this important initiative even mentioned. May I ask the authors why?

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AUTHORS’ REPLY We are aware of the work to


3 Grant IWB. Deaths from asthma. BMJ 1986;1:575.


AUTHORS’ REPLY Dr Grant’s comments are welcome and highlight the impossibility of including all the information obtained in a study such as SCIAD \(^1\) in a paper of suitable length for publication. The sudden deterioration of previously well patients, so-called “brittle asthma”, was not a major feature of the deaths studied, raising the possibility that there may be relatively fewer such patients or that patients who die suddenly in the community, even with a history of asthma, are certified from other causes of death. It is noteworthy that the routine management of patients studied, including the use of inhaled steroids, was appropriate in the majority of cases, so it may be that with a general improvement in standards of asthma care, there are fewer patients with brittle disease than there were previously. Review of the cases where delays were cited as a factor showed no case where a delay in reaching hospital was the only factor in patients in whom a sudden onset of symptoms was reported; poor compliance was also commented on in these few patients. A review of the cases where death occurred in A&E likewise revealed no case of sudden deterioration (within hours) definitely due to sudden onset of severe asthma; in most cases a number of other factors including aspiration of vomit and the use of non-prescribed drugs was a factor. There is therefore no evidence of deaths which would have been prevented by fast track admission and, with the more widespread administration of oxygen and nebulised bronchodilators and the use of paramedical ambulance crews, there are other reasons for emphasising the use of normal referral services, as well as promoting patient self-management to minimise the occurrence of such episodes.

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Nebulised fluticasone

The place of nebulised inhaled corticosteroids in the treatment of patients with asthma is difficult to assess, but Dr J M Hill’s editorial in Thorax \(^5\) was inaccurate and below accepted standards for a major medical journal.

Nebulised fluticasone is frequently referred to, yet all the studies referenced \(^6\) have only been published as abstracts (sponsored by the manufacturers of fluticasone) in supplements to journals. There are insufficient details for these papers to be properly scrutinised. They have not been subject to proper peer review and should have no place as the sole references for a new treatment for asthma in the editorial of a major medical journal.

Dr Hill states that “it is clear from a number of studies that fluticasone is twice as potent as budesonide at a mg for mg dose” but references this with a study which compares fluticasone with beclomethasone \(^7\) and not budesonide.

This is clearly incorrect. She forgets that different inhaled devices influence potency ratios. Thus, fluticasone in a Diskhaler may be equipotent with budesonide in a Turbuhaler \(^8\) and fluticasone is frequently referred to, yet all the studies referenced \(^6\) have only been published as abstracts (sponsored by the manufacturers of fluticasone) in supplements to journals. There are insufficient details for these papers to be properly scrutinised. They have not been subject to proper peer review and should have no place as the sole references for a new treatment for asthma in the editorial of a major medical journal.

Conflict of interests: neither Dr Todd nor his spouse have shares in any pharmaceutical company. He has received payment from Astra, Boehringer, 3M, Forest Laboratories (USA), GlaxoWellcome, MSD and Zeneca for presentations/lectures in the past five years. He has only received payment for research from GlaxoWellcome (fluticasone).

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AUTHOR'S REPLY The author thanks Dr Todd for his constructive comments on her review article.

There are few published randomised controlled trials of nebulised fluticasone or budesonide in the treatment of asthma. Despite this, these agents are being actively marketed by the pharmaceutical industry so it is vital that the debate about the place of these agents in the treatment of asthma should continue. The author therefore thinks that it is justifiable to review what evidence is available, accepting its limitations in abstract form.

The author apologises for incorrectly quoting a paper comparing the potency of budesonide and fluticasone. The correct reference is cited below. However, the author had presumed that the readers of Thorax would be well aware that data comparing different inhaled corticosteroids apply only to the type of inhaler used in any comparison, and that this basic principle did not require explanation.

Dr Todd's comments about different nebuliser systems and drug solubility are well taken. However, this was a short review of the use of nebulised corticosteroids in the treatment of asthma. We did not aim to review nebuliser pharmacokinetics or the clinical significance of differences in drug delivery systems. The author recognises the importance of excluding Wegener's granulomatosis from the diagnosis.

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Finally, neither Dr Hill nor her spouse has any financial interest other than their editorial duties in the People's Republic of China. The author apologises for incorrectly quoting a paper comparing the potency of budesonide and fluticasone. The correct reference is cited below. However, the author had presumed that the readers of Thorax would be well aware that data comparing different inhaled corticosteroids apply only to the type of inhaler used in any comparison, and that this basic principle did not require explanation.

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studies, the highest value for Cmax was used. A significant difference in lung bioavailability between devices will remain the same and is related to the bronchodilator response.\[3\] Measurement of the lung bioavailability of salbutamol in healthy subjects may therefore represent a simple in vivo method for preliminary quantification of the relative lung dose from different inhaler devices to select rational doses for subsequent clinical equivalence studies in asthmatic patients.

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NOTICE

International Pediatric Respiratory and Allergy Congress

The International Pediatric Respiratory and Allergy Congress will be held on 1–4 April 2001 at the Prague Congress Center, Prague, Czech Republic. For further information contact the Congress Secretariat at the Congress Centre, Czech Medical Society, JEP Sokolská 31, CZ-120 26 Prague, Czech Republic. Telephone +4202 296889 or +4202 297271; fax +4202 294610 or +4202 24216836. Email: lonekova@cls.cz
BCG vaccination by multipuncture method

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