BCG vaccination by multipuncture method

I write in response to the article by Al Jarad et al 1 on this topic. The first study to compare the efficacy of BCG vaccination and its side effects using the Bignal 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. 2

In previous studies in neonates and children under two, referenced in the paper by Al Jarad et al, 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 or 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 neonates 3 and in Al Jarad’s study 4 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. 4

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, long term studies on large numbers of subjects would be required to determine whether the technique using only 18 needles in older children is as effective as intradermal vaccination. Such studies may well prove to be unnecessary. The PHLS system for enhanced tuberculin surveillance begun in 1992 to show whether England and Wales meet the internationally recommended criteria for discontinuation of unselective BCG vaccination in low prevalence countries. 5 BCG vaccination of selective at risk groups, however, would still be required.

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AUTHORS’ REPLY

To our knowledge our study 6 was the first to compare the Bignal 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 al 7 and later by Ormerod and Palmer 8 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 tuberculin surveillance in the UK, but unfortunately we do not hold Dr Ormerod’s optimism that it will indicate that unselective BCG vaccination can be discontinued in boroughs and areas where notification rates of tuberculosis are high. Further studies on the protective values of multipuncture BCG may still be appropriate.

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LETTERS TO THE EDITOR

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. 9 It is important to provide patients with alternative strategies 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 still require ventilatory support. However, we are in agreement 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


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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, there is no evidence that this is so. 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 the question as long ago as 1968\(^2\) when it inaugurated a self-admission scheme for patients known by 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\(^3\) and reports on 10 year and 15 year reviews of its progress were published in 1979\(^4\) and 1987. 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 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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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 may be that 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.

Dr Hill confidently assumes a 1:2 potency of nebulised fluticasone and budesonide? The answer is unknown, simply because there are no comparative studies. Yet Dr Hill confidently assumes a 1:2 potency ratio when giving the costs of each treatment—and fluticasone appears to be one half the price of budesonide.

Finally, any article, editorial or otherwise—and especially one that makes unfavourable comparisons between drugs—should be accompanied by a declaration of competing interests. There is nothing wrong with having a competing interest but readers need to know. Dr Hill should have stated these interests (if any) in the same detail as reported recently in a review article on asthma drugs in the BMJ\(^9\).

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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3 Grant IWB. Deaths from asthma. BMJ 1986;1:575.
6 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 certain with earlier 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 much 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 delay at the 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.
AUTHOR’S REPLY The author thanks Dr Todd for his constructive comments on her review article.1 There are few published randomised controlled trials of nebulised fluticasone or budesonide in the treatment of asthma. Although this study is small, the difference in treatment response is significant and, as Dr Todd states, there are no comparative studies of the potency ratio of these two inhaled corticosteroids. 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.2 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 available clinical evidence for the use of nebulised corticosteroids in the treatment of patients with asthma. It was not possible to, nor did I, review nebuliser pharmacokinetics and, as Dr Todd states, there are no comparative studies of the potency ratio of nebulised budesonide and fluticasone.

Finally, neither Dr Hill nor her spouse has received payment from GlaxoWellcome, manufacturing asthma treatments. She has shares in any pharmaceutical company mentioned.

We have therefore reviewed eight studies performed in our laboratory using an identical design in which a nominal dose of 1200 µg salbutamol was administered via different devices in healthy volunteers. Where the same device was evaluated in two or more

Therapeutic equivalence of inhaled salbutamol

The meta-analysis by Hughes et al was hindered by difficulties in comparing trials that were often flawed and of varied design.1 The authors correctly pointed out that, in most of the studies, the use of a placebo as the null hypothesis was invalid. In addition, all but two of the studies looked at the bronchodilator effects in the presence of basal airway tone, which can mask the bronchodilator response.

The authors of the meta-analysis were unable to identify suitable studies with a placebo control group.

1. Hughes et al. J Allergy Clin Immunol 1999;103:943-

104.

2. Barnes NC, Hallett C, Harris TAJ. Clinical experience with fluticasone propionate in asthma: a meta-analysis of efficacy and systemic activity compared with budesonide and beclomethasone dipropionate at half the microgram dose or less. Respir Med 1998;92:95-104.

Pyoderma gangrenosum

Wang et al report an interesting case of systemic pyoderma gangrenosum (PG) with associated lung injury.1 They recognise the importance of excluding Wegener’s granulomatosis (WG) in patients with respiratory symptoms and cutaneous ulceration, but in their case seem only to have done this on clinical and histopathological grounds. A more complete assessment should include testing for c-ANCA and anti-proteinase 3 (PR3).2

We are currently treating a 54-year-old ex-smoker who presented for investigation of haemoptysis and who subsequently developed episcleritis and skin lesions resembling PG. Initial investigations were Hb 11.5 g/dl, WBC 9.4 × 10⁹/l, platelets 371 × 10¹⁰/l, CRP 86 mm/h, and CRP 181 mg/l. Renal function was normal. The chest radiograph showed alveolar shadowing in the left lower zone and an HRCT scan confirmed pulmonary infiltrates. Fibrebronchoscopic and transbronchial biopsy specimens were normal. Skin biopsy specimens showed epithelial cell necrosis and acute inflammatory changes with no evidence of vasculitis or granulomas, consistent with PG. The ANCA assay was positive with a cytoplasmic distribution and was directed against the proteinase 3 epitope. Despite the absence of histological evidence, the clinical features and positive ANCA supported a diagnosis of PG.

One month into treatment with pulsed intravenous methylprednisolone and cyclophosphamide the patient is clinically better with resolution of haemoptysis, healing of the pyoderma-like lesions, and a fall in the CRP to <21 mg/l. Patients with PG frequently present with non-specific signs and symptoms and a high index of suspicion is important.3 This case highlights the difficulty of distinguishing between ANCA in patients with PG and respiratory tract symptoms as the treatment of WG requires prolonged immunosuppression for at least a year. Whilst PG itself may be associated with milder forms of WG, we have recently directed against the proteinase 3 epitope. The meta-analysis by Hughes et al was hindered by difficulties in comparing trials that were often flawed and of varied design.1

The authors correctly pointed out that, in most of the studies, the use of a placebo as the null hypothesis was invalid. In addition, all but two of the studies looked at the bronchodilator effects in the presence of basal airway tone, which can mask the bronchodilator response.

1. Hughes et al. J Allergy Clin Immunol 1999;103:943-

104.
studies, the highest value for Cmax was used. A significant difference between devices was assumed where respective 95% confidence intervals did not overlap. The results are shown in fig 1.

There were no significant differences in lung dose between CFC-pMDI, HFA-pMDI, and the dry powder inhalers, although the Accuhaler produced lower levels than the Diskhaler. As expected, the addition of a Volumatic spacer increased the lung delivery for both CFC-pMDIs and HFA-pMDIs. When used in combination with a Volumatic spacer there was greater delivery with HFA than with CFC. The Sidestream nebuliser resulted in a lower relative lung dose than any of the other devices. However, if an adjustment is made to reflect the usual 2500 µg nominal dose administered by nebuliser (Cmax = 2.92 ng/ml), the lung dose is similar to the adjusted value for a 400 µg nominal dose from a Nebuhaler spacer with HFA-pMDI (Cmax = 2.96 ng/ml).

Although decreased airway calibre in asthmatic patients will reduce the lung dose of salbutamol from a given device, the relative difference in lung bioavailability between devices will remain the same and is related to the bronchodilator response. 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 2946899 or +4202 297271; fax +4202 294610 or +4202 2416836. Email: lonekova@cls.cz
Therapeutic equivalence of inhaled salbutamol

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