LETTERS TO THE EDITOR

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The editors will decide, as before, whether to also publish it in a future paper issue.

Sweat chloride and conductivity 1

Editor,—As a principal author of the sweat testing document published by National Committee for Clinical Laboratory Standards (NCCLS) and consultant to the Cystic Fibrosis Foundation (CFF) (USA), I write to address an inaccuracy in the article by Heeley et al. 1 The authors misrepresent the NCCLS document on the role of conductivity analysis. Nowhere does the NCCLS document address an inaccuracy in the article by Heeley et al. 1

Hence, the NCCLS document is available electronically from the NCCLS website (www.nccls.org) and may be purchased from the NCCLS office.

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Sweat chloride and conductivity 2

Editor,—As I understand the Scientific Method, a statement purporting to be factual, either in a scientific article or in a discussion with peers, must be supported by cited evidence that may be publicly examined for its scientific veracity.

The paper by Heeley et al 1 provides data to illustrate the equivalence of conductivity and chloride in cystic fibrosis (CF) diagnosis, and therefore corroborates the findings of an earlier clinical trial by Hammond et al. 2 Further, a statistical comparison of the extensive published sweat chloridic data of Shwachman et al 3 with the conductivity data of Hammond shows that the two are of equal discriminatory power in CF diagnosis.

Despite this evidence, Dr LeGrys has authored a document 4 that contains a number of assertions on this subject and on other aspects of sweat testing, that are not supported by any published results of original work of which I am aware. No clinical trial data exist which show that conductivity should only be used as a screen, that it is in any way inferior to chloride as a reliable diagnostic discriminator, or that conductivity readings of 50 mmol/l are positive for CF. Dr LeGrys’ call for more studies on this matter may be seen as an evasion of the true issue. I suggest that the time has come, albeit belatedly, for her to substantiate her case, not simply to belittle previous work.

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www.archdischild.com
our hospital of sending urine for culture. We wanted to see if a change in practice to urine culture being done only if nitrites or leukocyte esterase were positive would be effective in reducing the number of urine cultures.

The inclusion criteria for Sharief and colleague’s study was a clinical suspicion of urinary tract infection, when urine cultures were sent and dipstick testing was done. We found that urinary tract infection could easily be missed if urine culture was undertaken only if nitrites or leukocyte esterase are positive. Surprisingly, the results of both our study and theirs are similar: sensitivity was 34.4% v 20.0% and specificity was 90.7% v 99.2% in our study and Sharief’s study respectively. Negative predictive value was 92.4% in our study and 96.7% in Sharief’s study. Only the interpretation of the results is different.

A test with such a low sensitivity cannot be recommended as a screening test to exclude urinary tract infection. Urinary tract infection may result in irreversible renal damage in infants and therefore most care should be given to the detection of this infection in this age group. Unfortunately, the group where sensitivity of dipstick testing is the lowest (20%). I agree with Sharief and colleague’s study that because of its high negative predictive value, dipstick testing may have some role as a screening test for tract infection in situations where the incidence is very low. Positive nitrites have a high specificity for urinary tract infections, which was the basis of our suggestion that if nitrites are positive, especially in a febrile infant, empirical treatment with antibiotics may be considered until the result of urine culture is obtained. However, it should not be the whole criterion for diagnosing this infection.

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She should repeat lumbar punctures be routinely done in neonates with bacterial meningitis? Results of a survey into clinical practice

EDITOR,—Neonatal meningitis remains a very important cause of morbidity and mortality, with 30% death or handicap rate reported in a recent study.1 In common with other clinical situations, the evidence base for some of the management recommendations for good clinical practice is hard to find. One particular aspect of the management of neonatal bacterial meningitis, whether or not a repeat lumbar puncture should be undertaken routinely. Several standard textbooks of neonatology recommend repeating the lumbar puncture routinely in the course of neonatal bacterial meningitis, to ensure that “meningitis” continues to improve. This recommendation is based on past practice, and current evidence in favour or against repeating the lumbar puncture in neonatal bacterial meningitis is needed.

However, we have observed that day to day clinical practice appears to have changed and fewer repeat lumbar punctures are being done. To investigate this we performed a simple questionnaire survey across the north
west of England to determine the opinion of currently practising/trainee paediatricians and neonatologists. Table 1 shows the results of the survey.

The response rate of 65% is a representative response for this type of survey. There was a good mix of experience—58% consultants and 42% trainees in paediatrics/neonatology; 47% had more than 10 years neonatal experience (table 2).

Many textbooks reflect past practices, especially when there is little new published evidence to support a change, yet in some circumstances day to day clinical practice is quite different from that promulgated in the standard texts. In an era of demand for evidence based practice and an ever increasing level of litigation it is clearly important that current practice based on experience is reflected appropriately. This study shows that there is a widely held and practised view that routinely repeating lumbar punctures in neonates with bacterial meningitis is not appropriate and that a selective approach to repeating the lumbar puncture based on the clinical situation is the preferred option. This opinion was reflected by both those with long experience and in the teaching hospitals as well as by those practising in district general hospitals and trainees in paediatrics/neonatology. A national clinical survey of the outcome for infants with meningitis under different management practices should be carried out.

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Table 1 Results of the questionnaire survey

<table>
<thead>
<tr>
<th>Question: Do you routinely repeat LP in proven neonatal meningitis?</th>
</tr>
</thead>
<tbody>
<tr>
<td>172 (100%) surveys posted</td>
</tr>
<tr>
<td>112 (63%) responded</td>
</tr>
<tr>
<td>3 (1.5%) were invalid (incompletely filled)</td>
</tr>
<tr>
<td>Answer: YES, 20 (18%) do routinely repeat LP because:</td>
</tr>
<tr>
<td>It helps in deciding duration of treatment—14 (70%)</td>
</tr>
<tr>
<td>It reassures that infant is improving—10 (50%)</td>
</tr>
<tr>
<td>It is recommended in textbooks—9 (45%)</td>
</tr>
<tr>
<td>Answer: NO, 89 (82%) do not routinely repeat LP and will only repeat if clinically indicated, because:</td>
</tr>
<tr>
<td>It will not help in deciding the duration of therapy—63 (70%)</td>
</tr>
<tr>
<td>Clinical improvement is more important—61 (68%)</td>
</tr>
<tr>
<td>It is unnecessary trauma—35 (39%)</td>
</tr>
</tbody>
</table>

Table 2 Breakdown of those who responded

<table>
<thead>
<tr>
<th>Registrars</th>
<th>Consultants</th>
<th>&lt;10 years experience</th>
<th>&gt;10 years experience</th>
<th>DGH</th>
<th>Tertiary centres</th>
</tr>
</thead>
<tbody>
<tr>
<td>Routinely repeat LP</td>
<td>3.6%</td>
<td>14.6%</td>
<td>5.5%</td>
<td>12.8%</td>
<td>14.6%</td>
</tr>
<tr>
<td>Do not routinely repeat LP</td>
<td>38.5%</td>
<td>43.2%</td>
<td>47.7%</td>
<td>33.9%</td>
<td>54.1%</td>
</tr>
</tbody>
</table>

LP, lumbar puncture.

We need the full picture on both smacking and vaccinations

EDITOR,—Dr Elliman is noted for his careful methodological analysis of vaccination studies, but is not so careful in his recent analysis of physical punishment. The American Academy of Pediatrics’s co-sponsored scientific consensus conference on corporal punishment used a more scientific approach than the Elliman-Lynch summary. First, it carefully defined spanking as a subset of corporal punishment. Second, it incorporated a range of scientifically validated perspectives into summary statements that were more balanced than the Elliman-Lynch perspective. Third, it solicited the first systematic review of child outcomes of non-abusive or customary physical punishment by parents, which was recently updated. Both reviews concluded that non-abusive smacking had consistently beneficial child outcomes in the most causally conclusive studies—for example, randomised trials. Both non-compliance and fighting decreased in 2–6 year olds after non-abusive smacking was used to back up milder disciplinary tactics, such as reasoning or time out. Causal evidence of detrimental effects of customary physical punishment was less conclusive and limited to overly frequent smacking—for example, three times weekly for 6–9 year olds. In head-to-head comparisons, the effects of non-abusive or customary smacking rarely compared unfavourably with any disciplinary alternative, whereas its effects were significantly better than six alternative disciplinary tactics, mostly in 2–6 year olds.

My updated review considered all 92 studies included in the unpublished 1999 Ger shoff review cited by Elliman and Lynch. Most (76%) of her studies were excluded from my review for reasons that Elliman would want to discount vaccination studies—for example, inappropriate measures, cross sectional designs.

Ellison and Lynch also presented a one sided summary of Swedish statistics since their 1979 smacking ban. Additional information on this issue and other related issues can be found at http://people.biola.edu/faculty/paulp/. The issues are complex, requiring a more careful analysis given to concerns about vaccination.

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Oral steroids and inflammatory markers in asthma

EDITOR,—We thank Dr Grigg for his interest in our work.1 We agree that the asthma attacks may have resolved spontaneously in some cases, which was precisely why we stated that the markers fell in association with steroid therapy, and not necessarily causality. Nevertheless, the statistical analysis suggests that the chances this occurred at random are extremely low.

We agree that corticosteroids do not inhibit, except at very high concentrations, degranulation of the eosinophils induced by incubation with opsonised particles, such as Sepharose beads in vitro.2 However, there is overwhelming evidence that cytokines such as IL-5 prime eosinophils for increased release of granule proteins in this situation,3,4 and that they inhibit cytokine-mediated prolongation of eosinophil survival.5 These observations, coupled with the abundant evidence that corticosteroids reduce the expression of eosinophil-active cytokines, such as IL-5, provide a convincing chain of evidence linking the clinical use of corticosteroids with reduced release of eosinophil granule proteins in vivo.

With regard to the controls in this study the ratio of atopic to non-atopic asthmatics was 4:1 and of atopic to non-atopic controls 3:1. These differences are not expected by chi-squared testing. Whilst we agree that more controls might have strengthened our conclusions, nonetheless the evidence of suppressed inflammation after a short course of corticosteroids is probably clinically adequate course of prednisolone, as shown by the elevated levels of IL-5 and sCD25, remains strong.

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1 Grigg J. Oral steroids and inflammatory markers in asthma [Rapid Response]. Arch Dis Child. 16 August 2000. http://adc.bmj.com/cgi/letters/archdischild/83/2/92.ep1

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Progress in the management of disease in the newborn has carried with it a recognition of the substantial risk of injury to the immature nervous system. The aspiration to localise the substantial risk of injury to the immature newborn has carried with it a recognition of the importance of the examination, giving guidance on possible pitfalls and sources of error. There is a lot of very useful information on the variations in findings in term and preterm infants, and particularly the changes in the neurological features of preterm infants as they grow towards term. There follows a section on the development of an optimality score from the observed items of the assessment. This section deals with the results of a survey of 224 normal term infants. In this study each item of the scheme was plotted, and centile values (and thereby optimality scores) were computed. This provides quantification of the assessment, a sense of the range of findings to be expected, and can be useful in correlating lesions observed on neuro imaging with clinical findings. Chapter six deals with the scheme in relation to findings in infants with recognised brain lesions. The book is not designed to be a text of neonatal neurology and readers looking for discussion of neurological disease states will be disappointed. As a description of a comprehensive and easily applied system of neonatal neurological examination the new edition succeeds admirably.

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Oral steroids and inflammatory markers in asthma

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Arch Dis Child 2001 84: 450
doi: 10.1136/adc.84.5.450f

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