LETTERS TO THE EDITOR

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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 refer to the current conductivity methods described in the paper as unreliable; it does recognize that older conductivity methods are subject to evaporation error. The NCCLS document goes on to state that the CFF has approved the use of newer conductivity analysers for the screening of cystic fibrosis (CF) at community hospitals, using a decision level of 50 mmol/l. This decision level is supported by the data presented in the Heeley article. The data presented in the article concerning equivocal patients also support the US reference interval for sweat chloride as normal below 40 mmol/l.1 Patients with chloride values greater than 40 mmol/l should be further evaluated.

The reluctance of many to accept the use of sweat conductivity in place of sweat chloride for confirming a diagnosis of CF is based on the fact that chloride determinations directly reflect the genetic mutation of the disease. Conductivity is a property of all the charged species in a sample—for example, sodium, potassium, chloride, lactate, bicarbonate, etc. As the authors point out, chloride provides greater discrimination than sweat sodium—that is, less overlap between diagnostic categories. It would seem logical then, that combining sodium with chloride in a conductivity measurement would effectively cancel out the discrimination advantage of chloride alone. Referring to the data presented in table 2, there were twice as many patients with equivocal conductivity concentrations as with chloride (albeit a very limited sample size). Additionally, there exists a paucity of data in the scientific literature comparing conductivity and chloride values in CF and non-CF individuals. Even the scientists publishing such research support the conclusion that conductivity is appropriate for initial screening and chloride for confirmatory diagnosis.2 Heeley et al’s article attempts to provide relevant data, however it is most unfortunate that the authors failed to include in their analysis a linear regression plot of chloride versus conductivity along with a bias plot of the data so that the reader could assess the correlation. Most studies need to be published comparing conductivity with chloride, particularly in patients with results in the equivocal range, before the conclusion can be made that sweat conductivity is as effective as chloride measurement for the diagnosis of CF.

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Dr Heeley et al respond

Editor,—As the principal author of the NCCLS guideline on sweat testing methodology, Dr LeGrys should be better informed of its content. It includes the clear statement that when sweat test results are obtained by conductivity measurement “the patient should be referred for quantitative sweat electrolyte testing”. In our paper we refer to this statement as implying that sweat conductivity measurement should be regarded as “unreliable for diagnostic purposes”. This surely cannot be conceived as misrepresenting the NCCLS position, as claimed by Dr LeGrys. Although the NCCLS does, by reference, attribute this advice to Cystic Fibrosis Foundation (CFF) (USA) policy, by including it in their guideline without comment or qualification, the NCCLS authors are actually promoting it.

The medical politics of the USA do not concern us, but rather the question as to whether there is any scientific evidence underpinning this advice which the NCCLS uphold. The result of our study suggests there is none.

Dr LeGrys quotes research findings which support the conclusion that sweat conductivity measurement is appropriate only for initial screening purposes. We contend that there is no data presented in this otherwise excellent paper which provide scientific justification for that conclusion.

Dr LeGrys is of the opinion that the conclusion we draw from our own study should have been supported by appropriate linear regression and bias plots of the data. The *Archives’* professional statistical adviser reviewing our manuscript, which included such data analysis, thought otherwise and requested us to remove it. It is rather ironic then, Dr LeGrys should now be pleading for more studies to be carried out to resolve the issue of the diagnostic equivalence of indirect and direct sweat electrolyte measurement, focusing on patients who produce results which are equivocal. Considering the relative rarity of such patients in general paediatric practice, if the problem revolves around these cases, why did the NCCLS guideline fail to state this in the first place? In reality, the final diagnosis of cystic fibrosis in these cases is likely to be resolved by the results of investigations other than the sweat test.

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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 al1 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 chloride data of Shwachman et al3 with the conductivity data of Hammond shows that the two are of equal discriminative power in CF diagnosis.4

Despite this evidence, Dr LeGrys has authored a document5 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 with opinions, but by providing proper citations for relevant experimentally obtained data to support her contentions in the said document.

In a separate article6 Dr LeGrys refers to conductivity as a ‘qualitative’ assay, appearing to infer that it is less reliable than chloride analysis. The term “quantitative”, used in the pad-absorption method merely indicates that
Dr Thayyil-Sudhan and Dr Gupta comment:
Our study involved a selected group of children who were at an increased risk of having urinary tract infection. The inclusion criteria were the presence of any of the following: firstly, clinical suspicion of urinary tract infection; secondly, history of previous urinary tract infections or renal anomalies; thirdly, children needing antibiotics (urine culture met the above criteria and were included in the study). Urine culture was done for all these children, which reflects the local practice at 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 is used. Whether or not nitrites or leukocyte esterase are positive.

Surprisingly, the results of both our study and theirs are similar: sensitivity was 34.4% vs 20.0% and specificity was 90.7% vs 99.2% in our study and Sharief and colleague’s study.

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, there is still a child 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.

EDITOR,—Dr Elliman is noted for his careful methodological analysis of vaccination studies. West of England to determine the opinion of currently practising/trained 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 wide 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.


We need the full picture on both smoking 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.1 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,1 which was recently updated.2 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 use discordant 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 the same careful analysis given to concerns about vaccination.


Table 1. Results of the questionnaire survey

<table>
<thead>
<tr>
<th>Question: Do you routinely repeat LP in proven neonatal meningitis?</th>
<th>Registrars</th>
<th>Consultants</th>
</tr>
</thead>
<tbody>
<tr>
<td>172 (100%) surveys posted</td>
<td></td>
<td></td>
</tr>
<tr>
<td>112 (63%) responded</td>
<td></td>
<td></td>
</tr>
<tr>
<td>3 (1.5%) were invalid (incompletely filled)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Answer: YES, 20 (18%) do routinely repeat LP because:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>It helps in deciding duration of treatment—14 (70%)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>It reassures that infant is improving—10 (50%)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Clinical improvement is more important—61 (68%)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>It is unnecessary trauma—35 (39%)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Table 2. Breakdown of those who responded

<table>
<thead>
<tr>
<th>Routinely repeat LP</th>
<th>Registrars</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>3.6%</td>
<td>6.5%</td>
<td>5.5%</td>
<td>12.8%</td>
<td>14.6%</td>
<td>3.6%</td>
</tr>
<tr>
<td>38.5%</td>
<td>43.2%</td>
<td>47.7%</td>
<td>33.9%</td>
<td>54.1%</td>
<td>27.5%</td>
</tr>
</tbody>
</table>

DGH, district general hospital; LP, lumbar puncture.

LM, lumbar puncture.

LM, lumbar puncture.

LM, lumbar puncture.

LM, lumbar puncture.

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 casual-ity. 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.3 These observations, coupled with the abundant evi-dence that corticosteroids reduce the expres-sion of eosinophil-activating cytokines, such as IL-5, provide a convincing chain of evidence linking the clinical use of corticosteroids with reduced release of eosinophil granule pro-teins 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 unimportant by chi-squared testing. Whilst we agree that more controls might have strengthened our conclusions, nonetheless the evidence of suppressed inflammation and a decrease in eosinophil clinically adequate course of prednisolone, as shown by the elevated levels of IL-5 and sCD25, remains strong.


BOOK REVIEW


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 and prognosticate from neurological signs in the early newborn period is easily understood. The problem is that the signs available to be discerned are in themselves usually insufficient to allow precision. In addition, the child grows and develops, the range and complexity of skills are constantly changing, and the manifestations of the lesion(s) alters, or may become silent, often to reappear later as a different but nevertheless highly significant impairment.

The evaluation of the newborn nervous system was originally based upon concepts learnt from adult neurology. The baby was seen as demonstrating little or no cortical or cerebellar activity and the study of primary reflexes predominated. The approach of adult neurology, with emphasis on localisation of the lesion, becomes less applicable in the younger child. In the newborn period, focal insults to the brain will often give rise to generalised disturbances and, contrarily, generalised disturbances may show focal deviations. Recognition of these phenomena has led to a progression from the concept of a localisation based neurology to one which sees the infant displaying a neurological/behavioural repertoire. Over the past several decades Saint Anne Dargassies, Prechtl, Amiel Tison, Brazelton, Dubowitz, and others have, through meticulous study, done much to illuminate this area. Through these studies, awareness of the importance of the behavioural state of the baby, as well as the more detailed neurological items has evolved.

A second problem in this area, particularly in relation to research studies, has been the development of a systematic newborn neurological examination which is reliable and repeatable. This has been the subject of the two editions of this work. The first, published in 1981, gave a detailed, easily understood and applied system for the neonatal neurological examination. The current edition brings that work up to date. New material is presented, refinement of the scheme has occurred, and the examination is described. Items which were less discriminatory of pathology from the 1981 version have been withdrawn and, following the work of Prechtl, more emphasis is placed on the analysis of general movements. There is a further post neonatal to two year old infant neurological examination proforma presented briefly at the end of the text.

The text is essentially a manual on the application of this neurological examination scheme. It is easy to follow and the segments of the examination are presented clearly with excellent photographs and line drawings of each manoeuvre. There is also a useful addendum ("cautionary tales") to each section 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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