Archives of Disease in Childhood, 1989, **64**, 1513–1516

Correspondence

When to do a lumbar puncture in a neonate

Sir,

We enjoyed Dr Halliday's annotation on neonatal lumbar puncture but felt that his discussion of the role of lumbar puncture in suspected sepsis left an unfortunate question mark hanging over this procedure.[1]

It is important we feel to distinguish between early onset and late onset sepsis. In early onset sepsis the usual route of infection is by ascending infection that causes neonatal pneumonia, often clinically and radiologically indistinguishable from hyaline membrane disease, and septicaemia. Frequently babies with group B streptococcal pneumonia are septicaemic at birth,[2] and as meningitis occurs secondary to high level bacteraemia it is scarcely surprising that about 30% of babies with early onset group B streptococcal sepsis have meningitis.[3 4] Dr Halliday quotes Eldadah and colleagues' two year study,[5] which showed only five cases of group B streptococcal pneumonia and septicaemia, none of whom had meningitis, as a reason for not doing early lumbar punctures. Over a five year period (1984–9) with 5000 to 6000 live births per year we have seen seven cases of early onset meningitis (<48 hours old): four were due to group B streptococci, and one each to *Listeria monocytogenes, Escherichia coli,* and *Streptococcus mitis*. There were no clinical features to distinguish them from septicaemia without meningitis. In two of the seven cases blood cultures were negative. A positive lumbar puncture caused us to revise our antibiotic regime in two cases.

The situation with suspected late onset sepsis is superficially similar but the organisms are different. Over the same five year period, we have seen four cases of Gram negative bacillary meningitis (one pseudomonas, one klebsiella, one achromobacter, and one with both klebsiella and *E coli*). In all cases the blood cultures were positive, but a positive lumbar puncture altered our antibiotic regime.

It is sometimes argued that if the same antibiotic regime is used to treat late onset septicaemia alone as is used to treat meningitis, then one can rely on blood cultures without performing a lumbar puncture. However, about 10% of late onset meningitis occurs with negative blood cultures.[6] If, as is increasingly the case, antibiotics are stopped after two or three days in the face of negative blood cultures, the occasional case of meningitis will be missed and will progress. Although rare, such an outcome would be disastrous. In addition, we have seen two unsuspected cases of *Candida albicans* meningitis and four cases of enteroviral meningitis over the same five year period. It is important to diagnose the former for early treatment and the latter to prevent cross infection.

We certainly believe that lumbar puncture can lead to significant respiratory deterioration in babies with hyaline membrane disease, particularly those with pulmonary hypertension, and we defer the procedure in such babies. In general, however, we believe that for both early and late onset sepsis, it is important that a lumbar puncture, which is after all a 'biopsy' of cerebrospinal fluid giving instant results, be performed. This affects prognosis and directs antibiotic and supportive treatment appropriately.

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Dr Halliday comments:

I thank Drs Isaacs and Dobson for their comments on my annotation.[1] If a question mark hangs over the role of lumbar puncture in suspected sepsis then I am afraid that the doctors from Oxford have not removed it. Most paediatricians would agree that all babies with early and late onset sepsis should have a lumbar puncture performed, but the problem is in making a diagnosis of suspected sepsis. The Oxford data do not help in this respect, showing a low incidence of early onset meningitis with only seven cases over a five year period. Drs Isaacs and Dobson do not state their indications for doing a lumbar puncture in suspected early onset sepsis but I suspect that many hundreds of babies had this procedure performed to pick up just two babies with meningitis whose blood cultures were negative.

The incidence of late onset meningitis in Oxford is also low with only four cases detected over five years—all of whom had positive blood cultures. While agreeing that about 10% of late onset meningitis occurs with negative blood cultures[6 7] this was not so in Oxford so that Drs Isaacs and Dobson would not have stopped antibiotics 'after two or three days in the face of negative blood cultures'. Surely it is unlikely that any paediatrician would stop antibiotics after two or three days in an ill baby who has negative blood cultures?

The picture remains unclear; we know little of the cost benefit ratio of neonatal lumbar puncture for many indications. Neonatologists must be sure that the procedures they undertake to screen selectively populations of neonates are not more hazardous overall than the disease that they are trying to detect.

Perhaps the only way to answer this question is to perform a randomised controlled trial comparing liberal use of lumbar puncture with more restricted indications as suggested in the annotation. Such a trial would have to be multicentre and look carefully at both short and long term outcome. Clinicians who are sure that their own practice is better would of course be excluded from such a study. Until firm evidence is available precise indications for lumbar puncture in the newborn will remain subjective.

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Diagnosis of neonatal bacteraemia

Sir,

I would like to comment on two letters in your March 1989 issue. Miceli Sopo *et al* believe that values for positive predictive accuracy of '56·8%, 59%, or 39–62% are unacceptable' while pointing to the 'great potential usefulness of the negative predictive accuracy of the evaluated tests'.[1] However, they neglect to mention the incidence of sepsis in the populations evaluated. As my paper was quoted,[2] I should indicate that the incidence of sepsis in that study was only 8%, so that a positive predictive accuracy of 39% has improved predictiveness by almost five times—hardly 'unacceptable'. Conversely, the chance that sepsis was not present was 92%, so that the negative predictive accuracy of 99% is not quite as impressive as it might at first appear.

In their study there were 17 infected babies (15 with documented sepsis) among 70 preterm infants, with positive predictive accuracy of 59% and negative predictive accuracy of 91·6%.[1] These numbers should be compared with the incidence of infection of 24% and 'no infection' of 76% (that is, the chance of predicting each category without the tests being available).

Dellagrammaticas *et al* report their experience with C reactive protein,[3] which suggests that the response may vary with the infecting organism, that sensitivity is lower in the first 24 hours after delivery, and that C reactive protein needs to be considered together with haematological tests. Like Mathers and Pohlandt,[4] I have observed that C reactive protein may lag behind the immature/total neutrophil quotient, especially in early onset infection due to group B streptococcus.[5]

These observations emphasise the difficulty in evaluating any diagnostic tests for sepsis, because different organisms may produce different patterns of response. The paper by Kite *et al* was heavily weighted with cases of coagulase negative staphylococcal bacteraemia (21 of 34).[6] The lack of sensitivity of the immature/total neutrophil quotient with this organism has previously been noted,[7] whereas it is more likely to be deranged with early onset group B streptococcal infection.[4 5]

It is difficult to escape the conclusion that we need to evaluate more than one test when attempting to diagnose neonatal bacteraemia and that different organisms (as well as different severity of illness) may produce different responses.

References

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Does dexamethasone suppress the ACTH response in preterm babies?

Sir,

We are interested in the need to consider steroid replacement treatment for intercurrent infection or surgery in babies who had been on dexamethasone for chronic lung disease. We used a protocol similar to that of Rennie *et al* for dexamethasone and stimulation by adrenocorticotrophic hormone (ACTH). The cortisol responses to tetracosactrin (Synacthen, Ciba) in our six babies were similar to those reported in their paper.[1] The tetracosactrin (36 µg/kg) was given to six infants one week after the dexamethasone had been stopped and again four weeks later to four infants; the results are shown in the table.

We would agree with Rennie *et al* that steroid replacement treatment may not be necessary. In our series all of

Table *Cortisol concentrations after treatment with tetracosactrin. Results are median (range)*

	One week after stopping dexamethasone (n=6)	*After further four weeks (n=4)*
Cortisol concentration (nmol/l):		
Baseline	128·3 (25–400)	276·0 (30–375)
After tetracosactrin	802·5 (345–1420)	1065·0 (380–1560)