LETTERS TO THE EDITOR

"Dewatering" the lungs

EDITOR,—In response to the commentary by Professor Walters,[1] we must first correct him in referring to our model for the normal alveolus[2] as "dry" when, in fact, it is based on many classic morphological studies[3 4] demonstrating fluid largely confined to "pools" at the septal corners. In proposing that an oligolamellar lining of surface–active phospholipid (SAPL) adsorbed to epithelium "pushes water aside" from the gas–exchange surface, we agree that it is difficult to prove direct binding conclusively. However, any intervening aqueous layer is no more than that normally sandwiched between adjacent planes of polar groups in such structures (fig 1). Moreover, this physiological milieu contains mobile cations which can neutralise the negative phosphate ions in the SAPL molecules to render them cationic, facilitating their tight binding into a very effective molecular barrier.[5] Hence SAPL is pseudocationic.

In citing evidence to support the conventional concept of a continuous liquid layer separating the surfactant lining from alveolar epithelium, Professor Walters refers to the recent study by Bastacky, Clements, and others,[6] who do, indeed, demonstrate such a liquid layer. However, they have created a totally artefactual situation by pre-inflating the lungs to 15 cm H_2O—just enough to squeeze out all blood—before freezing and fixation. The resulting alveolar surface is totally concave with respect to air whereas, in normal air filled lungs, scanning electron microscopic photographs demonstrate how at least 60% of the alveolar surface is convex as red cells bulge their way through capillaries just beneath the septal walls.[4] Convex interfaces tend to resolve fluid whereas concave surfaces accumulate fluid.[7]

We fully appreciate the classic studies of Professor Walters and his predecessors[8 9] showing the role of ion–channel water pumps; although it is still a moot point whether β-adrenergic stimulation can increase pumping capacity to the level needed to account for such rapid water clearance during normal birth. The vital question seems to be why these pumps are so severely compromised in respiratory distress syndrome that it can take 2–6 days to clear the fluid even after administering exogenous surfactant. To be constructive, it is particularly interesting when they[8 9] find that "for a secretory organ to be capable of generating a chemical gradient, a barrier must be present to restrict molecular diffusion" and, in the fetal lung, at least, "this barrier resides in the pulmonary epithelium."[8] Surely, the oligolamellar SAPL lining shown in our paper by epifluorescence microscopy and by electron microscopy in fig 1 (for a normal infant) is ideal for this function. Even a monolayer of SAPL bound to a solid can decrease ion permeability by an order of magnitude.[10] It would also seem reasonable that, by spanning intercellular junctions, as seen in fig 1, SAPL layers not only act as a "first line of defence"[1] against airborne pathogens,[2] but also provide a membrane of known semi-permeability[2] for preventing protein leakage and allowing those proteins to pump water under the known gradients.[8] Thus an adequate lining of epithelial bound SAPL could be vital to both ion–channel and oncotic water pumps, in addition to any physical action in "dewatering."

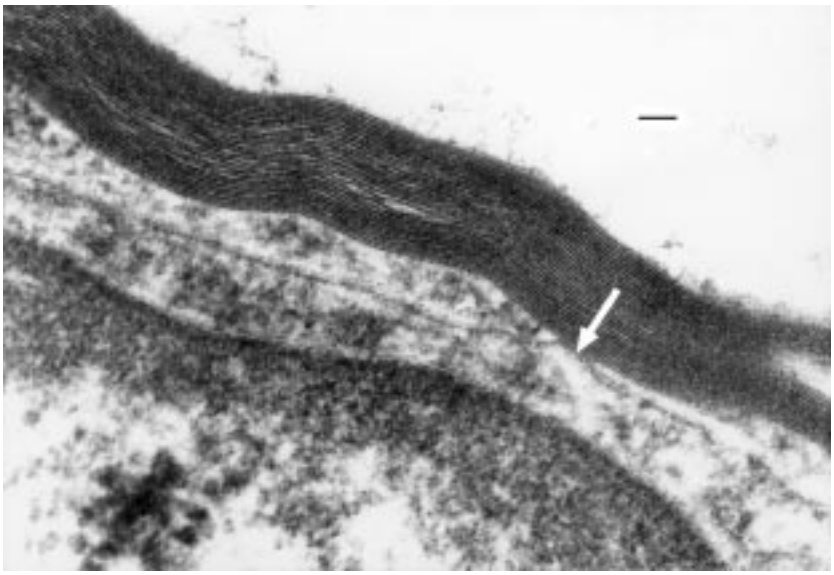

Figure 1 Electron micrograph of the alveolar wall of a 3 month old infant displaying an oligolamellar lining of SAPL apparently bound to epithelium. Note how it forms a continuous barrier spanning an intercellular junction (arrowed). The bar represents 50 nm.

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Cognitive, educational, and behavioural outcomes at 7 to 8 years

EDITOR,—In our study[1] we referred to research conducted by the Scottish Low Birthweight Study Group on developmental outcomes among a birth cohort of Scottish very low birthweight (VLBW) infants born in 1984. We stated that the Scottish study had yet to publish data on school aged outcomes among this cohort, and further that the study lacked comparative data on outcomes among a general child population sample.

It has been pointed out to us that both of these claims are, in fact, incorrect. The Scottish Group has published findings on the school attainment, cognitive ability, and motor function of their cohort at age 8 years,[2] and included, as part of their analyses, comparative outcome data for a general child sample matched for age and gender with the VLBW cohort and selected from the same school classes in which the VLBW cohort members were enrolled. Our own findings, based on a New Zealand birth cohort, show strong parallels with the Scottish study, particularly in relation to the higher rates of educational problems and poorer cognitive functioning experienced by VLBW children in comparison to their peers. It would have been useful to draw these comparisons in the discussion of our own findings. We very much regret this oversight on our part and apologise to the Scottish Group for our error.

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Diagnostic tests for bacterial infections

EDITOR,—Fowlie and Schmidt reviewed numerous publications on haematological parameters and C reactive protein for the diagnosis of bacterial infections and tried to select these publications according to well chosen criteria.[1] However, they omitted to

examine the publications for two criteria that substantially influence the study results.

Eight studies on band counts or immature to total neutrophil ratios were reviewed. But only the authors of two studies[2 3] actually defined how an immature neutrophil was differentiated from a segmented neutrophil—that is, morphological criteria, including the width of the connection between the nuclear segments. Segmented neutrophils are defined variably in published studies: most authors require an indentation of the nucleus to less than a third of the maximal nuclear diameter,[3] but others require an indentation to 50% or that the connections between nuclear segments are filiform. Discrepancies in the definition of bands and segmented neutrophils may be one of the reasons that the results for sensitivity and specificity vary largely between studies.

Diagnostic parameters and especially C reactive protein have characteristic kinetics in the course of a bacterial infection: C reactive protein has a low sensitivity at the onset of clinical signs of infection but the sensitivity improves with the course of infection.[4] Unfortunately, Fowlie and Schmidt included two studies in which the timing of blood sampling was not precisely defined.[5 6]

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1 Fowlie PW, Schmidt B. Diagnostic tests for bacterial infection from birth to 90 days: a systematic review. *Arch Dis Child* 1998;**78**:F92-F8.
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Drs Fowlie and Schmidt respond:

EDITOR,—Franz and Pohlandt raise interesting points. We agree that differences in how the various tests were carried out may explain some of the heterogeneity in the results. In addition, only 51% of the studies included in the review described the test in sufficient detail such that it could be repeated[1] so the extent of this problem was unknown. This was one of the reasons why we decided not to perform a meta-analysis of the results for any given test.

We did not set out to examine the usefulness of serial testing, but agree it is an area that may merit further investigation. However, it is important to bear in mind why diagnostic tests are performed: although the accuracy of tests may improve as the disease progresses, serial results or "late" results cannot help in deciding whether or not to start antibiotic treatment when the infant first presents. Equally, as time goes by it becomes more likely that the definitive results of the "gold standard" will become available, thereby giving the clinician the best evidence on which to base a decision whether or not to stop treatment.[1]

Alpha-coma in an infant with hypoxic–ischaemic encephalopathy

EDITOR,—Alpha coma (AC) is the combination of coma and an electroencephalographic (EEG) pattern of synchronous, rhythmic 8–13 Hz activity which has been described in certain severe neurological conditions such as post anoxic or ischaemic encephalopathy, head trauma, brain stem infarcts and drug overdoses.[1–3] In neonates AC has been associated with chromosomal abnormalities and inborn errors of metabolism and α rhythms have been described transiently during seizure activity.[4 5] Most reported cases have been associated with a poor outcome

Case report

A boy weighing 4080 g was born by vaginal delivery following prolonged fetal distress and meconium stained amniotic fluid to non-consanguineous parents. Cesarean section had been refused. Apgar scores were 1,6, and 7 at 1, 5, and 10 minutes, respectively. At one hour, arterial pH was 7.29, bicarbonate 20 mmol/l, and base deficit 6 mmol/l. The infant developed mild meconium aspiration syndrome that required mechanical ventilation.

Increased tone, fisting of both hands, and blank staring were noted shortly after birth. When 6 hours old, coma and convulsions appeared. Treatment with phenobarbital and subsequently phenytoin was partially effective, with serum concentrations in the therapeutic range. The convulsions diminished and the coma resolved over several days.

Blood count, blood glucose, serum electrolytes, calcium and magnesium and urinary amino and organic acids were normal. Metabolic acidosis was not detected throughout the hospital course. Karyotype was normal.

The EEG on day 3 showed continuous 10–11 Hertz α activity with amplitude of 15–40 uV localised over the left parasagittal and temporal regions, with sporadic generalised short bursts of mixed frequencies and sharp waves not associated with clinical correlates. Marked suppression of cortical activity was recorded over the right hemisphere with occasional 10–11 Hertz α of 10 uV amplitude. (fig 1). The EEG showed burst–suppression pattern on day 4 (fig 2) and prolonged

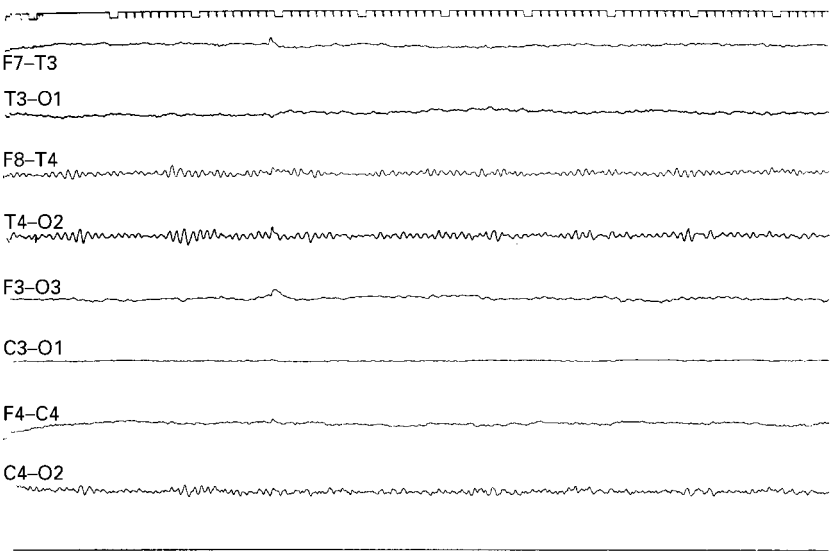

Figure 1 EEG on day 3 of life showing persistent α activity that is more pronounced over the left parasagittal and temporal regions, with suppression of activity over the right temporal area.

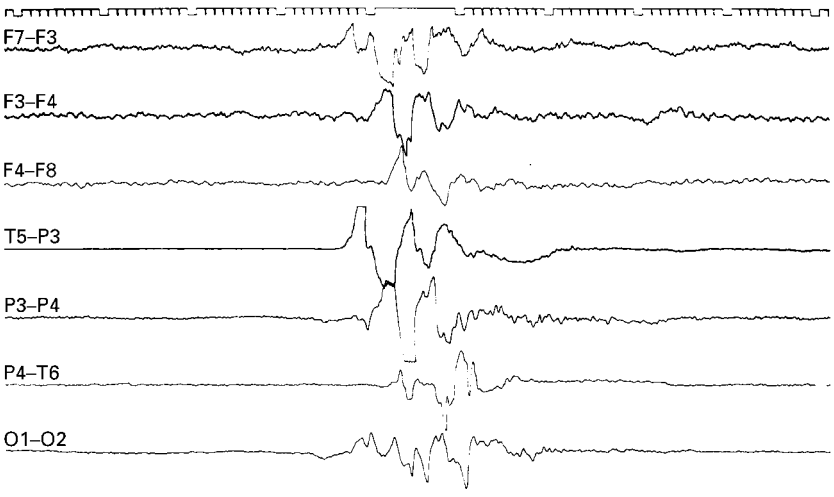

Figure 2 EEG on day 4 of life showing suppression of cortical activity with burst suppression pattern.

polymorphic delta activity of low to medium amplitude when the child was 3 months old.

A computed tomography scan showed severe bilateral cortical atrophy, basal ganglia infarcts, and periventricular cystic leucomalacia. At 3 years of age, the child had severe spastic quadriplegic cerebral palsy, pseudobulbar palsy, and psychomotor retardation.

In spite of the relatively mild evidence of asphyxia at birth, the intrapartum history, the course of the encephalopathy and the absence of appropriate abnormalities on metabolic or imaging studies make any diagnosis other than HIE unlikely.

The α rhythm in this infant was consistent, non-reactive, and not associated with a clinical seizure correlate, as described in AC in other settings. This description of AC in HIE adds to the usual EEG findings of initial voltage suppression followed most often by burst-suppression pattern.[6]

Poor prognostic indicators in HIE include the presence of seizures, the duration of the EEG abnormalities, and the severity of the clinical syndrome.[7] The burst–suppression pattern in HIE is of ominous clinical significance. Although we have presented only one case of AC in HIE with poor neurodevelopmental outcome, this may represent an additional poor prognostic indicator.

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Which course of dexamethasone?

EDITOR,—Skelton *et al*[1] state that "most units start [dexamethasone] by the second week of life, weaning over 2–3 weeks rather than 6" and that such a regimen "has been shown to be comparable in effectiveness with a six week course." The reference cited is a review paper that provides no evidence for the first statement, and discusses studies that showed a benefit in time to extubation but not in the duration of oxygen treatment.[2] Time of extubation is irrelevant if the incidence of chronic lung disease has not been reduced, and, to our knowledge, a three week course has not been shown to have this beneficial effect. Indeed, Cummings *et al*, in the study in which he reported beneficial effects of the 6 week course, found that an 18 day course was not effective at reducing time in oxygen.[3] There are now at least four dexamethasone regimens described which do reduce the incidence of chronic lung disease. In these days of evidence based medicine, should we not be using one of these?[3–6]

The cardiac effects of a short course of dexamethasone have been reported before, by Brozanski *et al*, in their trial of a three day repeatable pulsed course.[4] They also found significant hypertrophy which regressed by discharge. Definition of myocardial hypertrophy by statistical comparison with the "control" group in the study of Skelton *et al* is of doubtful value. Controls were very different babies, being a median of 3 weeks older and 400 g heavier at birth. Their echocardiograms were performed at different gestational and postnatal ages. Furthermore, the increases in interventricular septal and left ventricular posterior wall thicknesses are described for a 21 day period in the control group and for a variable period (to maximal hypertrophy) in the treatment group. Inspection of the data provided in the figures suggests that comparison between groups after 21 days may not have shown a significant difference.

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1 Skelton R, Gill AB, Parsons JM. Cardiac effects of short course dexamethasone in preterm infants. *Arch Dis Child* 1998;**78**:F133-F7.
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Drs Skelton, Parsons, and Gill reply:

EDITOR,—We feel that Bloomfield *et al* may have missed the main purpose of our study. Evans[7] recommended screening for left ventricular haemorrhage for infants receiving dexamethasone. We examined the safety of current prevailing practice. In the UK dexamethasone courses have been reduced in length due to concerns about side effects. Pulsed dexamethasone[8] is not often used. Whatever its merits and despite many studies of different regimens, ours reflects current use. Although we hope to prevent chronic lung disease, in practice, early extubation and the stabilisation short courses produce are not "irrelevant."

Due to the nature of infants receiving dexamethasone, well matched controls were not possible. However, the changes on real time echocardiography were very clear cut. Our controls, as well matched as possible, were included to provide comparison and emphasis of the severity of the left ventricular haemorrhage, rather than prove that it had occurred. In trying to compare groups at 21 days, Bloomfield *et al* seem to have misunderstood. Twenty eight day old controls was chosen to try to coincide with maximum hypertrophy, at around 10 days after starting dexamethasone. Two time points were felt sufficient, as we found no left ventricular haemorrhage in a normal preterm population of a similar age.[9]

Brozanski's[8] study is not comparable. The study population and dexamethasone use differed: repeated doses 3 days every 10 days up to 36 weeks is hardly "short course." The ethics of placebos is of concern and a low incidence of left ventricular haemorrhage (24%) suggests different natural history or inappropriate reference ranges.[9] Our study is the first to examine in depth left ventricular haemorrhage using current dexamethasone practice.

Finally, these studies emphasise our poor knowledge of dexamethasone and left ventricular haemorrhage. Greater emphasis on the mechanism of action of dexamethasone rather than differing regimens may improve use in small susceptible infants.

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