LETTERS

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References

Hypothsis waiting for proof: un wrapping neonates for transfer

During transfer from the delivery suite to the neonatal intensive care unit (NICU), infants are traditionally wrapped in pre-warmed towels. Whether this is optimal remains unknown. We compared the effects on core temperature of wrapping or not wrapping neonates during their transfer from the delivery suite to the NICU.

After resuscitation, infants in both groups were transferred to a Vickers 77-transport incubator and left wrapped or unwrapped. Core temperature was recorded using a mercury thermometer before leaving the delivery suite and again, immediately after transfer into a NICU incubator. The study was granted ethical approval.

Our findings are summarised in the table. There were no significant demographic differences between the two groups. While the mean temperature change during transit was lower in the unwrapped group, the mean temperature change during transit was lower although neither difference reached statistical significance. No hypothermia (rectal temperature <36°C) occurred in either group.

Wrapping infants in towels prevents convective heat gain. Additionally, leaving infants unwrapped allows essential clinical observation. Despite the limitations of this small study, our findings challenge the practice of wrapping infants and warrant further examination in larger clinical studies.

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Diuretics in CLD

This symposium on chronic lung disease of prematurity (CLD) by Kotecha et al. covered important aspects and controversies in the management of CLD. We accept the authors’ inability to cover all aspects of management. We feel that some space could have been devoted to diuretics in management of CLD. Nearly all patients with CLD of some stage of their disease will receive diuretics and most of them will be on them for a long time. We came across only one systematic review by Brion et al. in the Cochrane database. Conclusion of the authors was that there was no beneficial effect of using distal tubular diuretics for more than 4 weeks after initial stage. There was also no benefit in adding potassium sparing diuretics or newer diuretics like metalozone. Insipie of very little evidence base for diuretics in CLD, one finds nearly all CLD patients on a diuretic cocktail. In addition to their effect on electrolytes, they affect Ca/Po metabolism. This may exacerbate osteopenia of prematurity and may have adverse effect on lung compliance. There is a need for more discussion or clear guidelines on this issue.

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References

Table 1 Demographics of the two study groups and temperature difference

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<tr>
<td>Gestation range</td>
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<td>7 min 6 sec</td>
</tr>
<tr>
<td>Range</td>
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<td>5–10 min</td>
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<tr>
<td>Range</td>
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Aspiration pneumonia in association with oral vitamin K

Most infants born in the British Isles now receive vitamin K prophylaxis, and the trend towards oral administration continues. With the awareness that vitamin K is well absorbed from the gut and following publication of the report linking intramuscular vitamin K and childhood cancer, oral vitamin K prophylaxis has become more widespread. However, because of lack of uniform national policy, the practice of vitamin K administration varies from region to region. Cases of aspiration or anaphylaxis following oral vitamin K administration in neonates have not been previously reported.

We report three cases of aspiration associated with oral vitamin K, Orakay, the preparation of which is licensed, effective, easy to administer, and minimal adverse effect.

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Diuretics in CLD

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Positioning long lines: response to Reece et al

Percutaneously inserted central venous lines are widely used in neonatal intensive care to administer parenteral nutrition and medications. It is important to ascertain the position of the line tip before use as incorrectly positioned long lines can lead to life-threatening complications like cardiac tamponade and pulmonary oedema.1,2

Reece et al suggested that it is prudent to use a routine contrast radiograph to localise the line tip in newborn infants.3 We would like to comment on their suggestion and report a relevant study done out on our neonatal unit.

Intravenous water soluble contrast is not commonly used in neonates and very little is known about its potential side effects in premature infants.4 Studies have shown that renal clearance is prolonged in premature infants because of renal immaturity.5 Data in children have shown a number of possible side effects, including hypotension and cardiac arrhythmia.6,7 Moreover, obtaining an intravenous contrast radiograph of a long line would require additional medical and nursing time as a doctor would have to ‘gown up’ for the procedure, which may not be logistically feasible in some busy neonatal units, especially out of hours.

Reece et al were unable see the line tip clearly in two cases, even after a contrast study.8 This was due to delay between the injection of contrast and the radiographer exposing the film. This shows that fine coordination is required between the radiographer and the person injecting the contrast. Specific training may be necessary.

We performed a retrospective study of the reliability of plain radiographs in identifying the position of the long line tip in our tertiary neonatal intensive care unit. Over a 10 month period all 27 babies who had long lines inserted were included. In all cases an Epicuronate-Cava-Katheter (Vygon, UK) was inserted. This is the same catheter as that used by Reece and colleagues.4 Our placement aim was also similar to that in their study.9

The position of the line tip on the postmortem was independently reviewed by an experienced neonatal radiologist (SB). There was agreement between the two investigators in 25/27 (92.6%) cases. No complications due to line placement were observed during the study period.

We therefore feel that a plain radiograph is the safest, quickest, and cheapest way to ensure the safety of the line.

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References

Neonatal sepsis in Peshawar

We wish to raise a few concerns regarding the study reported by Rahman and colleagues.10

We found it surprising that only five species of microorganisms were isolated in this series of over 1000 blood cultures obtained from neonates with sepsis. Similar studies done in other major cities of Pakistan, with much smaller sample sizes, have shown a wider spectrum of pathogens. Anwer et al11 showed 11 species types in 109 blood cultures, Bhutta and Yusuf12 showed 13 species types in 125 cultures. In addition to the five species causing neonatal sepsis reported by Rahman et al (Esherichia coli 36.6%, Staphylococcus aureus 29.5%, Pseudomonas 22.4%, Klebsiella 7.6%, and Proteus 3.8%), all the other investigators have also reported Serratia spp and Enterococcus, and most reported Streptococcus pneumoniae, Salmonella spp, and group B Streptococcus. Although the authors do not clearly state whether they excluded hospital acquired infections in their series, the studies reported by Bhutta13 did exclude nosocomial infections.

The antimicrobial susceptibility data reported by Rahman10 are not interpretable as the number of microorganisms on which antimicrobial susceptibility testing was performed is not presented. In addition, the susceptibility results are not internally consistent; 60% of the Staphylococcus aureus tested are reported to be ampicillin sensitive but only 27% were Amoxicillin + Clavulanate (Augmentin) sensitive. This represents a highly unusual susceptibility result with a high percentage of S aureus not producing beta-lactamase enzymes to inactivate penicillin (ampicillin), but still showing resistance to a penicillin-beta-lactamase-inhibitor combination such as Augmentin. We wonder if the 60% reported sensitivity of S aureus to ampicillin is erroneous since the vast majority of S aureus, even in developing countries, are now penicillin (ampicillin) resistant.14,15 We also find the 73% resistance rate of S aureus to amoxicillin-clavulanate (which is equivalent to methicillin resistance for S aureus) surprisingly high, and question if this indicates the presence of hospital acquired infections in this series.

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References

Effect of head up tilting on oxygenation

We read with interest the paper by Dimitriou et al1 in which it was confirmed again that head up tilting to 45° degrees results in better oxygenation in stable preterm neonates. However compared with our study,1 which measured the same effect was observed, there is a (probably) significant difference. Their infants were studied in the horizontal prone, in the horizontal supine and in the 45° head up tilt supine position whereas in our study all infants were studied in the prone position including the 45° head up tilt.

We therefore feel that the combination of the prone position and the 45° head up tilt could facilitate diaphragmatic activity. We do not think that this hypothesis can be totally dismissed by the results of Dimitriou et al as suggested by the authors, since their infants were studied in different postures that is, supine in their study and prone in our study.

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References

Authors’ reply
We thank Professor Dellaragiamarchas for his comments on our study.1 Dellaragiamarchas et al hypothesised that the combination of the prone posture and the 45° head up tilt position could facilitate diaphragmatic activity. We however, propose that the improvement in oxygenation seen in the head up tilt position was more likely to be due to a change in lung volume. In the head up tilt position, the weight of the abdominal contents on the diaphragm is reduced, tending to increase functional residual capacity.2 In contrast, ultrasonographic examination3 has demonstrated that the diaphragm was significantly thicker at end expiratory volume in the prone rather than the supine position, which is likely to result in reduced diaphragm strength. Indeed, we demonstrated4 Pimax (a measure of respiratory muscle strength) was lower in the prone compared to the supine position and the supine posture with 45° head tilt.

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sodium deprivation, to improve somatic stability, and to avoid untoward clinical consequences."

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References

Author’s reply

Methinks Professor Sulyok doth protest too much. His early, pioneering work on electrolyte balance in the newborn is well known, and extensively cited in an earlier review of the subject co-authored by myself.1 In this, inter alia, his study of the effect of salt supplementation on the renin-angiotensin-aldosterone system2 is quoted in support of the hypothesis that hyponatraemia in premature infants is due to salt depletion rather than water retention. The reason these papers were not cited in the present paper is that they are not relevant to it. The paper is not a historical or general review of hyponatraemia in the newborn but the results of a study specifically designed to examine neurodevelopmental outcome in two particular groups of infants previously studied by ourselves.3 His recent study of hyponatraemia and sensorineural deafness in preterm infants4 had not been published when our paper was submitted to the Archives, although we would certainly have referred to it if it had been.

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References

CORRECTION

We would like to apologise for an error that occurred in the paper. Oxygen therapy for infants with chronic lung disease by S Kotecha and J Allen (Arch Dis Child Fetal Neonatal Ed 2002;87:F11–F14). The following sentence, under the heading Weaning from home oxygen, should have read: Vermeulen et al showed that infants who could be weaned from oxygen had awake median saturations of 97% during one hour awake studies, spent only 14% of time with saturation ≤ 95% and 2% of time ≤ 92%.

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Aspiration pneumonia in association with oral vitamin K

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