LETTERS TO THE EDITOR

Surfactant replacement therapy – time for thought

EDITOR,—Professor McClure acknowledges that surfactant treatment benefits most of its recipients but rightly questions whether it should be given to the smallest, sickest babies.¹ Meta-analyses of more than 30 randomised controlled trials enrolling over 6000 preterm babies have shown a consistent reduction in neonatal mortality of about 40% and of pneumothorax of between 40% and 70%.² Many of these trials excluded babies weighing less than 700 or 750 g at birth and some excluded babies with congenital anomalies, low Apgar scores, and severe grades of intraventricular haemorrhage. We agree with Dr Morley who indicates in his commentary that it is only by including extremely small or immature babies in trials that we can learn how (or if) their outcome can be improved. However we would also stress the need at randomisation to document their initial severity of disease, including the degree of hypothermia and asphyxia.

Four trials have looked at the effects of surfactant therapy for babies of <750 g or <27 weeks' gestation; three used prophylactic surfactant³⁻⁵ and one surfactant treatment of established respiratory distress syndrome.⁶ The results are summarised in the table. Prophylactic surfactant reduces the odds of neonatal mortality by about 40%, which is in keeping with the meta-analysis from more mature babies.² The risk of intraventricular haemorrhage is not significantly increased, but there is a trend that needs to be studied further. When Surfactant is used to treat respiratory distress syndrome in babies of 600–700 g there is no clear reduction in neonatal mortality,⁶ which is in keeping with the findings of Kendig *et al.*,⁷ using calf lung surfactant extract, who showed that in babies <26 weeks' gestation survival was better for prophylaxis (64/85 (75%)) than for rescue treatment (39/72 (54%); $p < 0.01$).

As Dr Morley points out in his reply the data are now beginning to support the use of surfactant, preferably by prophylaxis in even the most immature of babies. Some clinicians may accept this evidence and continue to treat these babies while others might call for further and larger randomised controlled

trials, especially if there is a chance that treatment might increase the risk of intraventricular haemorrhage. Also, perhaps one should restate the proved benefits of maternal steroids⁸ and call for their greater use in obstetric practice to limit the need for surfactant therapy.

Another strategy for learning more about the management of extremely immature babies is to undertake systematic audit of their treatment and outcome adjusted for initial clinical and physiological risk. This is being done in over 100 neonatal units in more than 12 countries through the International Neonatal Network. Colleagues who would like more information about this network are invited to contact Dr Tarnow-Mordi in Dundee.

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- 1 McClure G. Surfactant replacement therapy – time for thought. *Arch Dis Child* 1992; 67: 1228–30.
- 2 Soll RF. Surfactant treatment to treat or prevent RDS. In: Chalmers I, ed. *Oxford database of perinatal trials*. Version 1.3, disk issue 7, spring 1992. Record Numbers: 5206, 5207, 5252, 5253, 5664.
- 3 Ten Centre Study Group. Ten centre study of artificial surfactant (ALEC) in very premature babies. *BMJ* 1987; 294: 991–6.
- 4 Stevenson D, Walther F, Long W, *et al.* Controlled trial of a single dose of synthetic surfactant at birth in premature infants weighing 500–699 grams. *J Pediatr* 1992; 120: S3–S12.
- 5 Ferrara TB, Hoekstra RE, Couser RJ, *et al.* Effects of surfactant therapy on outcome of infants with birth weights of 600 to 750 grams. *J Pediatr* 1991; 119: 455–7.
- 6 Leitch EA, Donovan E, Purohit D, *et al.* Reduction of neonatal mortality after multiple doses of bovine surfactant on low birthweight neonates with respiratory distress syndrome. *Pediatrics* 1991; 88: 19–28.
- 7 Kendig JW, Notter RH, Cox C, *et al.* A comparison of surfactant as immediate prophylaxis and as rescue therapy in neonates of less than 30 weeks' gestation. *N Engl J Med* 1991; 324: 865–71.
- 8 Crowley P, Chalmers I, Keirse MJNC. The effects of corticosteroid administration before preterm delivery: an overview of the evidence from controlled trials. *Br J Obstet Gynaecol* 1990; 97: 11–25.

Putting the clock back 30 years: neonatal care since the 1991 NHS reforms

EDITOR,—In their paper Pope and Wild propose that the 1991 NHS reforms increase the rôle of the district service at the expense of region and that this is undesirable.¹ We would

emphasise that districts have an important contribution to the provision of neonatal services, including the provision of some (1) neonatal intensive care. This leads to:

- (1) Fewer transfers to a distant regional unit leading to reduced disruption for the families involved.
- (2) Better use of a district's resources. District hospitals need a special care baby nursery: with suitable training existing staff can provide intensive, as well as special care with only a little extra nursing time.
- (3) Better resuscitation/stabilisation of babies before transfer. The district neonatal team gain confidence and competence in dealing with babies who need transfer as a result of their greater involvement in intensive care.
- (4) Better access to the regional unit. With ever increasing numbers of smaller, iller babies, and with intrinsic limits to the working size of regional units, it is desirable to relieve pressure on regional units by taking on work in districts.

We respond to other points raised:

- (1) Monitoring. Audit schemes are developing to ensure adequate checks are made on district units.
- (2) Research and training. Junior staff in teaching hospitals should be trained by treating and researching on the most challenging and interesting cases during their stay at the regional centre. All training schemes should involve some rotation out to district units.
- (3) The paper emphasises London's unique organisational problems that need tackling politically on a much larger scale.

The NHS reforms have caused us all to look at the organisation of our services; judgments made on a financial basis can be helpful and clarify thinking. The reforms encourage a district as well as a regional view, previously there may have been excessive dependence on the latter.

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- 1 Pope C, Wild D. Putting the clock back 30 years: neonatal care since the 1991 NHS reforms. *Arch Dis Child* 1992; 67: 879–81.

C reactive protein and neutrophil band forms in neonates

EDITOR,—The recent paper by Russell *et al* contains some problems that make it difficult to interpret the applicability of the tests described in the detection of neonatal infection.¹

Firstly, the operational diagnostic cut off value for the immature:total neutrophil ratio is given in the abstract as 0.2, and in the text as 0.11. Which one was used?

Secondly, there is something of a self fulfilling prophecy in that two of the indicators examined, namely the neutrophil band count and the C reactive protein concentration, were also able to be used as criteria for initiation of a septic screen. Hence it is difficult to obtain a realistic idea of the usefulness of these tests.

Thirdly, related to the above, how many neutrophil band counts and C reactive protein estimations were performed which did not lead to a septic screen, and of these how

Effect of surfactant therapy on neonatal mortality and intraventricular haemorrhage in babies <750 g or <27 weeks' gestation

	Treated	Control	Odds ratio	95% Confidence intervals
Prophylaxis				
Neonatal mortality				
ALEC ³	13/43	15/32	0.49	0.17 to 1.41
Exosurf ⁴	51/106	60/109	0.76	0.43 to 1.34
Surfactant ⁵	5/26	14/28	0.24	0.06 to 0.93
Typical estimate			0.59	0.37 to 0.93
Intraventricular haemorrhage				
ALEC ³	14/42	10/31	1.05	0.35 to 3.17
Exosurf ⁴	45/106	39/109	1.32	0.74 to 2.38
Surfactant ⁵	7/26	7/28	1.11	0.28 to 4.40
Typical estimate			1.23	0.77 to 1.98
Treatment				
Neonatal mortality				
Surfactant ⁶	35/81	36/76	0.85	0.43 to 1.67

many were above the operational diagnostic cut off value? Can the authors provide the data on the total number of tests performed during the three month period of screening of every admission to the unit, and, assuming that on those occasions when cultures were not performed the blood cultures would in fact be negative, can they then provide a revised definition of sensitivity and specificity which would incorporate all the test results available?

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- 1 Russell GAB, Smyth A, Cooke RWI. Receiver operating characteristic curves for comparison of serial neutrophil band forms and C reactive protein in neonates at risk of infection. *Arch Dis Child* 1992; 67: 808-12.

Dr Russell comments:

The indications for performing a septic screen were clearly listed as clinical signs or band count of >5% or C reactive protein >8 mg/ml. The I:T ratio was not used to prompt a septic screen but was calculated as part of the analysis of the results because the immature neutrophil:total neutrophil ratio is a more widely published index.

Band count and C reactive protein concentrations were determined daily ('serially') in babies at risk of infection. We assumed that a negative test would indicate the absence of infection. The *sensitivity* (true positive/true positive+false negative) and *specificity* (true negative/true negative+false positive) of the tests was calculated from the results obtained at the time of screening. The *true negative* count for the test was determined if the test and the culture were negative when the screen was performed as a result of another indication. The indication for the screen that produced a negative culture was therefore a *false positive* test.

Bands and C reactive protein more than the cut off levels prompted a septic screen in all cases even if no other clinical indication was present. Therefore all false positive results would be determined besides the true positive results. The *sensitivity at the time of the culture*, is therefore correct as it depends on the number of true positive and false negative results (test negative and positive culture).

Drs Etches and Finer seem to propose that for the calculation of specificity, account should be taken of all negative test results obtained during serial infection surveillance (even if cultures were not taken). We determined the test specificity only at the time of the culture to avoid a result based on the assumption of a negative culture. To obtain all true negatives during serial infection

surveillance would require that all the negative tests be confirmed by a negative 'gold standard'. This is clearly unethical when an extra invasive procedure such as a blood culture is required. In the calculation of test specificity the true negative count occurs in both the numerator and denominator, and therefore an increase in this factor would increase the calculated test specificity.

On the basis of these practical and ethical reasons we feel that the sensitivity and specificity we have calculated do not require further revision.

Recovery of Intralipid from lumbar puncture after migration of saphenous vein catheter

EDITOR,—I read with great interest the paper of Odaibo, Fajardo, and Cronin¹ on the migration of a saphenous vein catheter. On inspection of their fig 1 it is clear that the catheter tip is not in the left common iliac vein nor the inferior vena cava. The catheter tip lies lateral to the left pedicle of L4 or L5 (it is difficult to be certain which due to the poor quality reproduction of bony detail). The contrast medium injected runs a thin curvilinear streak round the inferior border and up the medial side of this pedicle.

If one observes the course of the catheter below its tip one can see an angular deviation of its course – initially upwards and medially and then upwards and laterally. This change of course is at the origin of the left ascending lumbar vein and the contrast medium is filling the vertebral venous plexus. In other words I am sure this catheter was never correctly sited

but from the start was in the ascending lumbar vein and migrated from there.

I do not wish to diminish the importance of the point the authors make about this unusual complication of using a venous long line, it is very important. But their paper really makes a further point – that is, if the catheter tip is not correctly sited initially problems are more likely to ensue.

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- 1 Odaibo F, Fajardo CA, Cronin C. Recovery of intralipid from lumbar puncture after migration of saphenous vein catheter. *Arch Dis Child* 1992; 67: 1201-3.

Please note that an error occurred in the paper by Odaibo *et al*; intralipid should have been Intralipid (Kabi Pharmacia).

Retropharyngeal abscess secondary to nasopharyngeal CPAP in a preterm neonate

EDITOR,—We would like to describe a previously unreported complication of nasopharyngeal continuous positive airway pressure (CPAP) that occurred on our unit.

A baby of 26 weeks' gestation weighing 910 g was treated with nasopharyngeal CPAP for apnoeas and bradycardias from day 9 of age. A 6 FG suction catheter was used to determine the required length prong (2.5 mm Portex ivory endotracheal tube) under direct vision. The treatment was successful until day 22 when further bradycardias ensued. Examination by direct laryngoscopy showed the terminal 1 cm of the prong had eroded into the pharyngeal wall, with associated erythema and swelling. Cultures from blood and base of the pouch created were negative but the infant did require intubation for five days and was treated with antibiotics to cover retropharyngeal abscess.¹

This case demonstrates a previously unreported complication of nasopharyngeal CPAP and has changed practice in our unit. We now use shorter prongs not designed to reach the pharynx and confirm that by direct visualisation. We feel that the suction catheter used to measure the distance may have kinked in the nose leading to an overestimate of the required length of the nasal prong. We hope this report will minimise the chance of any further cases developing.

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- 1 Coulthard M, Isaacs D. Retropharyngeal abscess. *Arch Dis Child* 1991; 66: 1227-30.