

LETTERS TO THE EDITOR

Survival and place of treatment after premature delivery

SIR,—Dr Field *et al* are not justified in concluding from the data presented that there should be a more centralised service for babies of 28 weeks' gestation or less.¹ They found that babies of 28 weeks' gestation or less treated in large centres had higher survival rates than those treated in smaller units. It is likely that this is due to selection in the smaller units of appropriate babies to refer to the large centres.

Data from our unit, which is in the Trent region, were included in the report. We have a policy of transferring babies of 28 weeks' gestation or less to a regional centre. Two categories of babies are not transferred: firstly babies who are so ill that they do not survive long enough to be transferred or are not likely to survive transfer, and secondly babies who are considered pre-viable. During the time of the study, three babies of 28 weeks' gestation or less were not referred from our unit to a large centre, but remained in our unit for terminal care. These were a second twin of 26 weeks' gestation with severe birth asphyxia and asystole at birth who died at 1 hour 20 minutes after unsuccessful resuscitation attempts, an infant of 24 weeks' gestation, and an infant of 21 weeks' gestation.

During the same period we referred post-natally 12 babies of 28 weeks' gestation or less to a large centre for neonatal intensive care. Four of these babies died. Clearly the difference in outcome for these two groups of babies is due to selection bias.

It is likely that similar decisions were taken in other smaller units in the region and comparison of survival in the two types of units is therefore not valid.

We believe that it is in the interest of selected very immature babies in our unit that we should transfer them to a large neonatal centre for intensive care. The results of this policy will be that those infants of 28 weeks' gestation or less who do not transfer may well be receiving terminal care in our unit. It must not be concluded that these babies would have had a better chance of survival or better care had they been transferred to a large centre.

S A W SALFIELD
P I MACFARLANE
C RAVINDRANATH
Rotherham District General Hospital,
Moorgate Road, Oakwood,
Rotherham S60 2UD

1 Field D, Hodges S, Mason E, Burton P. Survival and place of treatment after premature delivery. *Arch Dis Child* 1991;66:408-11.

Dr Field and colleagues comment:

We thank Dr Salfield and his colleagues for their comments. Clearly we were concerned that small neonatal units might retain an excess of babies who were considered to have little chance of survival. It was for this reason we employed a method of disease severity scoring.¹ This approach (as reported in the paper) did not indicate that the group of babies treated in small units were at greater risk of death. However more sophisticated

methods of disease severity scoring are now being developed and these should allow this finding to be confirmed. In addition it will then be appropriate to look at mortality in other gestation groups after correction for disease severity.

1 Patterson CC, Halliday HL. Prediction of outcome after delivery for the very low birth-weight (<1500 g) infant. *Paediatric and Perinatal Epidemiology* 1982;2:221-8.

Birthweight ratio

SIR,—In our five centre study of 429 infants under 31 weeks' gestation, birthweight ratio was strongly related to requirement for ventilation, postneonatal mortality and anthropometry at 18 months, and largely unrelated to neurodevelopmental outcome.¹ Birthweight ratio, defined as birth weight divided by the mean reference birth weight for gestation, was proposed as a more useful outcome guide than growth retardation defined using an arbitrary cut off of the 3rd or 10th birthweight centile.

Recently, Brownlee and coworkers re-examined this concept in 436 infants from Leeds and claimed major discrepancies with our observations.² However, the authors agreed with our findings on the lack of relationship with neurodevelopmental scores (we found only a small deficit in language at 18 months), though neither study examined the prognostic value of birthweight ratio in more mature preterm infants. Brownlee and coworkers did not re-examine our data on long term growth and would not have been able to relate realistically birthweight ratio to post-neonatal mortality as in their study the latter was so low.

Thus the only real area of conflict was over the relationship between birthweight ratio and need for ventilation. We found a strong linear trend—the smallest babies at any gestation requiring most ventilation. The Leeds study did not confirm this. We considered this might have been due to a secular trend in neonatal care since our cohort was recruited prior to that in Leeds. Therefore we elected to retest our findings on a new cohort of 231 babies below 31 weeks' gestation that were recruited during 1989-91 into a dietary study in two of our five previous centres. Using birthweight ratio and days of ventilation as continuous variables, and adjusting for gestation, there was a highly significant fall in duration of ventilation with increasing birthweight ratio, corresponding to an average of over seven days' reduction as birthweight ratio climbed from 0.7 to 1.2 ($p=0.002$). Using the same cut off values for ventilatory duration employed by us and by the Leeds group, of more than 7, 14, or 28 days the significance values for reduced ventilatory requirement with increasing birthweight ratio were 0.06, 0.001, and 0.009 (after adjusting for gestation), despite the smaller sample than in our previous study. We were, however, no longer able to confirm that growth retarded babies were more likely to need ventilation in the first 24 hours. Thus, apart from this, our two centre study reaffirms the results of the previous five centre one.

Possibly some aspect of respiratory management in Leeds accounts for the differences observed; we understand, for instance, that in Leeds ventilated babies are often paralysed, a procedure which was relatively uncommon in the five centres we investigated. We accept that studies on more centres would be valuable.

Nevertheless, our own data, now based on a substantial cohort, have not led us to change our view on the clinical value of birthweight ratio.

ALAN LUCAS
RUTH MORLEY
Dunn Nutritional Laboratory,
Downhams Lane,
Milton Road,
Cambridge CB4 1XJ

1 Morley R, Brooke OG, Cole TJ, Powell R, Lucas A. Birthweight ratio and outcome in preterm infants. *Arch Dis Child* 1990;65:30-4.
2 Brownlee KF, Ng PC, Roussounis SH, Dear PRF. Birthweight ratio revisited. *Arch Dis Child* 1991;66:418-21.

Atrial natriuretic peptide and blood volume during red cell transfusion in preterm infants

SIR,—Rascher *et al* conclude that a slow transfusion of less than 10 ml red cells/kg body weight does not cause volume expansion.¹¹ Their plasma volume measurements are derived by subtracting the red cell mass from the total blood volume. The total blood volume was estimated from the red cell mass divided by the packed cell volume. The accuracy of the plasma volume estimations is obviously not only dependent on the accuracy of the red cell mass measurements but also on the accuracy of the packed cell volume measurements. The authors state that in adults whole blood packed cell volume is usually calculated by multiplication of the measured venous packed cell volume by 0.9. In preterm infants the authors agree the whole body: venous packed cell volume may also vary but do not correct for this. The published conversion factor for neonates is in fact 0.87.^{2,3} Furthermore the measurement of packed cell volume is liable to further error due to trapping of plasma between red cells which is dependent on the flexibility of the red cells. Even a mechanised system, such as a Coulter counter, for measuring packed cell volume by deriving a value from the mean corpuscular volume and red cell count is subject to error.

The method used to measure red cell mass is as described by Phillips *et al*,⁴ which depends upon the dilution of fetal haemoglobin by an infusion of adult haemoglobin. The magnitude of the resulting changes in concentrations of fetal haemoglobin is not specified in either work, although the coefficient of variation of the actual assay technique for measuring fetal haemoglobin is stated to be <3.6%.³ One wonders how accurately the small changes in concentration of fetal haemoglobin can be measured.

The study of Rascher and colleagues would have been more reliable if direct measurements of red cell mass and plasma volume had been made independently of each other. As the study stands the estimation of red cell mass is open to question and the measurement of plasma volume is flawed. A simple measurement of a change in the packed cell volume would have been as informative as the calculated changes in derived plasma volumes.

M Y ANTHONY
M I LEVENE
University Department of Paediatrics
and Child Health,
D Floor, Clarendon Wing,
The General Infirmary at Leeds,
Belmont Grove, Leeds LS2 9NS

1 Rascher W, Lingens N, Linderkamp O. Atrial