of venous and capillary sampling, but it would have been interesting to have had two groups, one in which capillary sampling preceded arterial sampling and one in which it was done afterwards. Our data did not show a variation in the white cell count, and I wonder what mechanism Dr Shohat proposes for his observed rise during and after lumbar puncture.

These points emphasise the difficulties in establishing a reference range of white blood count values for infants, particularly preterm infants, and in interpreting results by comparing them with published ranges. Application of the International Committee for Standardisation in Haematology guidelines for the standardisation of blood specimen collection procedures for reference values is obviously impractical, but it is clear that the method of sampling does affect the results obtained. Many of the published ranges are based almost entirely on capillary sampling so that arterial sampling, which is commonly done in these intensively monitored infants, may lead to a diagnosis of neutropenia. Conversely, as Dr Shohat points out, a painful procedure performed shortly before sampling might cause a neutrophilia equivalent to that found in older children and adults after exercise.

Reference

- 1 Christensen RD, Rothstein G. Pitfalls in the interpretation of leukocyte counts of newborn infants. *Am J Clin Pathol* 1978;72:608-11.

The metabolic load of stored blood. Implications for major transfusions in infants

Sir,

In a recent article Ratcliffe *et al*¹ described the changes in plasma osmolality, electrolyte balance, and metabolic substrates that can occur during storage of blood. These changes, an increase in osmolality, potassium, and lactate concentrations, and a decrease in sodium concentration, were ascribed to alterations in red cell permeability and continuing metabolism during storage. Infusion of large amounts of such stored blood may be harmful to the sick infant and may, for instance, result in severe hyperkalaemia.²

Interestingly, storage of fresh frozen plasma (FFP) can also produce solute concentration gradients in the infusion bags. We describe (table) the osmolality and solute concentrations incidentally measured at the surface and at the bottom of a bag containing 'unshaken' thawed plasma. To confirm this finding, we stored FFP in five 20 ml glass tubes and subsequently thawed the samples at room temperature without shaking. The following mean (SD) plasma osmolality and sodium concentrations were recorded: 237 (5) mOsm/kg and 33 (5) mmol/l at the surface, and 425 (18) mOsm/kg and 199 mmol/l at the bottom. Thorough mixing of the plasma resulted in homogenous concentrations of 321 (5) mOsm/kg and 173 mmol/l.

The differences in osmolality and concentrations in a cell free solution probably occur during freezing, which starts at the surface, and the free water is frozen first. We

Table Osmolality and electrolyte concentrations from surface and bottom of unit of unmixed thawed plasma

Measurement	Surface	Bottom
Osmolality (mOsm/kg)	237	679
Sodium (mmol/l)	127	277
Potassium (mmol/l)	2.6	5.2
Chloride (mmol/l)	57	92
Glucose (mmol/l)	21	40.7
Creatinine (μmol/l)	50	100
Protein (g/l)	43	114

conclude that the infusion of large amounts of unshaken thawed plasma could harm a sick neonate, and that this phenomenon could affect the accuracy of chemical analysis of samples of thawed plasma.

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References

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- 2 Blanchette VS, Gray E, Hardie MJ, *et al*. Hyperkalemia after neonatal exchange transfusion: risk eliminated by washing red cell concentrates. *J Pediatr* 1984;105:321-4.

Endotracheal resuscitation of preterm infants at birth

Sir,

The meticulous studies into the mechanics of neonatal resuscitation carried out by Professor Milner and his colleagues over the last few years provide a unique and valuable body of information. I would, however, like to clarify certain points relating to the methodology and interpretation of their most recent work.¹ In particular, how might the flow resistance of the resuscitation equipment affect the efficacy of resuscitation? The authors do not state the flow rate into their resuscitation circuit. The fact that their pneumotachograph was linear to 15 l/minute implies that an inspiratory flow rate up to this value could be provided for.

Careful analysis of figure 1 would suggest that the effective inspiratory flow resistance of the resuscitation circuit was very high. In the figure, the 'inspiration response' of the baby caused a peak inspiratory flow rate of 5 l/minute (0.08 l/second). This was achieved only by reducing the pressure at the endotracheal tube to -8 cm H₂O. Thus the apparatus resistance (resistance pressure/flow) was at least 100 cm H₂O/l/second, compared with the intrinsic resistance of an intubated preterm baby of roughly 200 cm H₂O/l/second.² Could this be part of the explanation for the very small tidal volumes achieved?

Finally, figure 3 needs further explanation. In it a