(31–6 weeks) as those studied by Evans and Archer (one at 28, one at 30, two at 32, and three at 33 weeks). Furthermore our results were substantiated by analysis of ductal flow. Unfortunately Drs Evans and Archer did not utilise the potential of their technique for serial measurement to the full; results were presented in a cross sectional manner, with different numbers studied at each age. Group means were compared when it would have been better to analyse the rate of fall in each individually separately.

In truth, neither of these papers can probably come to a definite conclusion about the relative rate of fall of pulmonary arterial pressure in term and preterm babies. However, the use of non-invasive Doppler techniques that have been introduced to neonatology and this discussion helps to clarify some of the potential merits and shortcomings of each.


Accuracy of height measurements

Sir,—In their study of the accuracy of measurements made by health visitors, Ahmed et al assume that a reading by a trained auxologist on a Harpenden stadiometer was obtained without error.1 Not only is this assumption dangerous and unjustified, it is also unnecessary, as the author's analysis happens to contain an estimate of the error's variability. The column headed 'children' in table 2 of their paper does not, as may be thought, provide the variance of the heights of the children who took part in the experiment. For children selected at random from the population, or even from a day nursery, as in the study, this should be of the order 15–20 cm2. Moreover, the effect due to a child is removed, in the analysis, by the differencing that occurs when the auxologist's measurement is subtracted from that of a health visitor. Our standpoint, on the other hand, is that the auxologist's measurement is the best available, and the purpose of our study was to examine how health visitor measurements compared with this best.

Prolonged low dose indomethacin for persistent ductus arteriosus

Sir,—We reviewed with great interest the article by Rennie and Cooke.1 The treatment of patent ductus arteriosus remains an important issue in the care of the premature infant. However, we would like to address several areas in order to clarify the results achieved by the investigators. Certain specific descriptions were missing in the methods section that would be helpful in justifying prolonged low dose indomethacin as an effective treatment.

Our first concern is the basis for the diagnosis of the patent ductus arteriosus and its relapse. While clinical symptoms are important diagnostic parameters, they are subject to observer bias especially in a study spanning different institutions. Echocardiography, the preferred diagnostic method, would strengthen the initial diagnosis and the presence or absence of relief.2 This improvement could have provided an important prospective diagnostic description to define the patient population more accurately.

Secondly, the many clinical factors that influence the patency of the ductus were excluded.3 There was no mention of important confounding variables such as fluid management, methods of ventilation, use of exogenous surfactant, or severity of the respiratory disease. In addition, the lack of serum indomethacin concentrations leaves an important void in the clinical results.4

The premise for the study, to find a safer treatment for the persistent ductus, is applauded. However, the lack of more detailed description of the patients and methods prevents this investigation from the universal acceptance desired by its authors. We would like to obtain the missing information or, if unavailable, suggest that repetition of the investigation controlling for the confounding variables. The results of such a study would provide an alternative way for the management of an all too common neonatal concern.

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