(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 individual 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 potential of Doppler techniques 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 authors' 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 cm.2 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.3 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 some 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 acceptable 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 relapse.2 This improvement would have provided an important prospective diagnostic description to define the population more accurately.

Secondly, the many clinical factors that influence the patency of the ductus were excluded.4 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 question, as those studied by Rennie et al did not have serum indomethacin concentration data.

Finally, the manuscript states that a safer treatment for the persistent ductus, is amplified. 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.

**Anthony J Marino**, **Mujaed Anwar**, **Anne Kenny**, **Mark Hiatt**, **Thomas Hegyi**

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Drs Rennie and Cooke comment:

We thank Dr Marino and his colleagues for their interest in our paper. We accept that echocardiographic diagnosis of patent ductus arteriosus would have provided more objective evidence on which to enrol and subsequently assess subjects, but at the time this study was started the technique was not available to us in either centre. We would obviously use this method in any future studies.

The problem of differences in management should have been taken into account by the investigators. Certain specific descriptions were missing in the methods section that would be helpful in justifying prolonged low dose indomethacin as an acceptable treatment.

During much of the time that this study was in progress we were also recruiting infants to a randomised surfactant trial. The problem of the severity of disease was partly addressed by the demonstration that by chance the long course group tended to be nursed in higher ambient oxygen at enrollment.

We would not agree that serum indomethacin concentrations are important for the management of patent ductus arteriosus. Our experience with measuring concentrations of this drug confirmed the large and unpredictable variation noted by Brash et al.4 We were unable to establish a threshold at which clinical response was certain and felt that this was due to the fact that even low levels of indomethacin were associated with cessation of prostaglandin synthesis. Our observations led to the present study as we felt, like Seybert et al that resurgance of prostaglandin synthesis could be important in relapse.


2 VoS LD. Birthday: measurement of the child, and measurement of the child's 'elasticity' as much as measurement. The main point at issue is whether measurements made by a trained auxologist using the Harpenden stadiometer and applying traction are affected by the child's 'elasticity' as much as measurements made by a health visitor, using a Mirotoise or wallchart. Mr Bailey and Ms VoS suggest that they are, by their statement that '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.

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