LETTERS TO THE EDITOR

Prospective surveillance of neonatal meningitis

EDITOR,—We read the paper by Hristeva et al on neonatal meningitis with interest.1 Their study included only those infants admitted to a neonatal unit. A previous study has shown that 45% of neonatal meningitis is seen in infants admitted from home and this population would not be represented in the study of Hristeva et al. Their incidence of bacterial meningitis at 0.25/1000 live births could therefore be an underestimate.

A review of neonatal meningitis in Liverpool between 1984 and 1986 revealed 34 cases: 20 treated in neonatal units and 14 in the wards of the local children’s hospitals. The incidence of bacterial meningitis in infants born to mothers resident in Liverpool and South Sefton health districts was 0.33/1000 live births (that is 16 out of 48 369 deliveries; HM Thomas, unpublished data). This is similar to the figure of de Louvois and Harvey of 0.32/1000.2 Dr Hristeva et al state that none of the 11 infants who completed treatment for bacterial meningitis relapsed. In Liverpool over a 10 year period two cases of neonatal meningitis have relapsed after discharge, both due to Escherichia coli. Both were admitted to a children’s hospital for treatment of the second episode and would also have been missed by the study of Dr Hristeva et al.

Finally, Dr Hristeva et al conclude that lumbar puncture may be deferred in the first 48 hours after birth and need only be performed if blood cultures are positive. This is not supported by their own data as in two of the eight early cases blood cultures were negative and antibiotic treatment was directed by the results of lumbar puncture in two of these cases. In two other recent studies blood cultures were negative in 18 out of 35 infants with bacterial meningitis on the first two days of life.3 4 We agree that a lumbar puncture should be deferred in any infant in a poor clinical condition. But we believe that a lumbar puncture should be performed if meningitis is clinically suspected and should not be avoided solely on the basis of negative blood cultures.

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Varicella zoster virus infection in pregnancy

EDITOR,—I would like to correct some inaccuracies in the recent annotation by McIntosh and Isaacs on the subject of varicella zoster virus infection in pregnancy.1 In their table 2, the authors presented data on the proportion of neonates with detectable varicella zoster antibody at birth in relation to the timing of the mother’s chickenpox rash before delivery.2 The table attributed to a Lancet publication by myself and colleagues from the Manchester Public Health Laboratory3 is incorrect, as it is based on the entries in their table. The actual data from our study are shown in the accompanying table. The misrepresentation of our results has potentially important implications for the management of perinatal exposure to chickenpox in the United Kingdom.”

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Drs Chan and Greenough comment:

We agree that only large trial sizes will detect small differences between groups, but such differences may not be clinically relevant. In addition, there are very few treatments that have no associated side effects and large trials are likely to expose a greater number of infants to such problems. In our study use of nasal CPAP resulted in agitation of seven infants with acute respiratory distress and two with chronic respiratory distress and hyperoxia in a further two infants. Our trial thus, importantly highlights the need for larger studies. The authors should also have mentioned that CPAP is not appropriate for ‘all-comers’. Before embarking on another randomised trial it is therefore important to clearly identify a group of infants who would benefit from CPAP.

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