LETTERS TO THE EDITOR

Rapid responses
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The editors will decide, as before, whether to also publish it in a future paper issue.

A national review of neonatal resuscitation programmes for midwives

Editor,—A considerable number of babies resuscitation programmes for midwives (72%) have not standards of achievement set for resuscitation training. Standards were characterised by competence in basic life support, clinical scenarios, and theoretical knowledge of neonatal resuscitation. Resuscitation training was compulsory for midwives in 132 (72%) units. Midwives in the United Kingdom are inadequately provision of neonatal life support and mask ventilation to endotracheal intubation. There is a national lack of neonatal resuscitation. Midwives in the United Kingdom are currently not following standard guidelines, 61% have variably incomplete in their evaluation of the knowledge and practices of health professionals. In Bristol, one neonate a month is admitted with hypernatraemic dehydration in an exclusively breast-fed infants due to maternal lactation failure. *Am J Forensic Med Pathol* 1998;19:19–22.


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<th>Plasma urea (mmol/l)</th>
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Table 1 Clinical details of five neonates presenting with hypernatraemic dehydration


Hypernatraemia: why bother weighing breast fed babies?

Editor,—Hypernatraemia is associated with cerebral oedema, intracranial haemorrhage, hydrocephalus, gangrene, and death, but is notoriously difficult to detect clinically. It is accepted in paediatric practice that weighing is an essential part of the assessment of an infant’s hydration.


Placing of neonatal central venous catheter tips in the right atrium: a practice to be avoided?

EDITOR,—Following the recent media interest in pericardial tamponade complicating the use of percutaneous central venous catheters in neonatal patients, we wish to alert readers to our experience. Our previous policy was to accept right atrial placement of percutaneous central venous catheter tips. This was in line with published recommendations and is still considered acceptable practice in some units in the United Kingdom, in contrast with practice in the United States. Between 1996 and 1997, we had five cases of neonatal pericardial tamponade, three of which resulted in death. All were associated with right atrial tip placement of lines implanted by angulation, curvature, or looping of the line. We have now changed our unit policy to avoid placement of catheter tips in the right atrium, and instead place them in the superior or inferior vena cava. In addition, to allow for the possibility of catheter migration, we recommend that catheter tips should lie at least 0.5 cm outside the cardiac outline on chest radiograph in small infants, or 1.0 cm outside in larger infants. Although an atrial position carries a small risk of thrombosis or hydrothorax, these complications are more benign than pericardial tamponade, which has a mortality of 65%. We recommend that placement of a percutaneous central venous catheter tip in the right atrium should no longer be accepted. In addition, we suggest that catheters that display angulation, curvature, or looping within the right atrium carry a particularly high risk of pericardial tamponade and demand urgent action. Although this issue has been the subject of correspondence in the RCPCH email discussion list, where the consensus was to avoid right atrial tip position, we believe there is a strong need for a wider debate about current practice in the United Kingdom.

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Newborns have unique confounding factors regarding the TIR-F ratio

EDITOR,—Sweet et al investigated the serum transferrin receptor (STIR) and, for the first time in neonates, transferrin receptor-log ferritin (TIR-F) ratio in a prospective series of cord blood taken from term infants and their mothers. They are to be congratulated on completing another piece of the complex jigsaw that is fetal and neonatal iron metabolism.

STIR and TIR were increased in iron deficient mothers, but not in their infants. The authors discuss at some length the translational (not transcriptional as stated in the discussion) control of intracellular ferritin synthesis. They measured serum ferritin, which is a glycosylated form of L-ferritin, and has been shown to correlate with intracellular iron in the absence of confounding factors. However, serum ferritin is secreted in response to a wide variety of other stimuli, including, for example, inflammation and shows gender differences in newborns. Of these limitations, serum ferritin may not accurately represent tissue iron stores.

It has already been reported that STIR does not correlate with other measures of iron metabolism in the newborn. Therefore, it is highly expressed by reticuloocytes and other immature erythroid cells, with or without iron deficiency.

The high sensitivity and specificity of the TIR-F ratio in adults is based upon their relationship in iron deficiency in the absence of factors that might otherwise elevate STIR levels. With both variables subject to these confounding factors in the neonate, I do not agree with the author's assertion that the TIR-F ratio "gives a measure of iron requirements in relation to iron availability" in this unique population.

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Changes in plasma creatinine in first 72 hours of life

EDITOR,—Recently, Miall et al have reported a rapid rise in serum creatinine in the first 48 hours of life in neonates. But we have noticed in our clinical day to day practice that this rise is transient and may not be clinically significant. To confirm this, we looked at the initial serum creatinine levels in a stable group of term neonates admitted to the neonatal intensive care unit of King Fahd Hospital of the University, Al-Khobar, Saudi Arabia.

Neonates with congenital anomalies, perinatal asphyxia, and those requiring ventilatory support were excluded. The serum creatinine levels were measured together with electrolytes by using an automatic analyser (Dimension, Delaware, USA), which were relayed by the reporting computer system (Ulti-view, Los Angeles, USA). Serum creatinine was available for the first 72 hours on a limited number of neonates. Of the selected babies were stable within the first 48 hours of life and there was no need for extra serum electrolyte and creatinine measurements. Out of all the newborns admitted during the three month study period who fulfilled the inclusion criteria, 13 neonates had serum creatinine measurements available for the first 72 hours. These readings were down and were analysed using the SPSS statistical package. The mean, standard deviation and statistical significance.

Out of thirteen neonates, seven (53.8%) had an increase in their plasma creatinine on the second day while four (30.7 %) had a
We hope this report may help in the investigation of other families.

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Neonatal paroxetine withdrawal syndrome or actually serotonin syndrome?

Three cases are described in which a neonate born to a mother on fluoxetine had jitters, irritability, tachypnoea, temperature instability, tremors, increased muscle tone, and a hyperactive Moro reflex. All except the last of these are clinical features seen in serotonin toxicity in adults using selective serotonin uptake inhibitors (SSRIs) therapeutically or in overdose. The neonate in this case had fluoxetine levels that were measurable initially and which fell as symptoms resolved.

In the two cases reported with paroxetine, the syndrome is referred to as a withdrawal phenomenon. However, the time course and symptoms were similarly typical of serotonin excess.

In the cases reported by Stiskal et al the neonates developed the features soon after birth and they resolved over a period of days. In case 2 an increased serum paroxetine level was reported in the infant. The level was too low to detect by day 15, supporting a toxicity syndrome. However, in case 4 there was a raised serum paroxetine level at the time of the adverse effects. Serum paroxetine levels have been positively related to serotonin toxicity in adults.

The features of case 4 may also have been exacerbated by the use of opiates in the delivery room. Pethidine is a well recognised cause of serotonin toxicity in conjunction with a serotonergic agent.

By March 2001, there were 13 reports to the Committee classified as ‘withdrawal syndrome’ in the March 2001 issue of the journal. The authors describe what they have called ‘neonatal paroxetine withdrawal syndrome’. However the syndrome described in the 4 neonates appears to be more consistent with serotonin toxicity, rather than withdrawal of paroxetine.

We would like to comment on the article ‘Neonatal paroxetine withdrawal syndrome’ in the March 2001 issue of the journal. The authors describe what they have called ‘neonatal paroxetine withdrawal syndrome’. However the syndrome described in the 4 neonates appears to be more consistent with serotonin toxicity, rather than withdrawal of paroxetine.

The literature to date contains one large series, two similar case reports with fluoxetine and two case reports with paroxetine.

The fluoxetine cases the syndrome was not described as a withdrawal phenomenon. In the first, a neonate born to a mother on fluoxetine had jitters, irritability, tachypnoea, temperature instability, tremors, increased muscle tone, and a hyperactive Moro reflex. All except the last of these are clinical features seen in serotonin toxicity in adults using selective serotonin uptake inhibitors (SSRIs) therapeutically or in overdose. The neonate in this case had fluoxetine levels that were measurable initially and which fell as symptoms resolved.

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Table 1

<table>
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<th>Child</th>
<th>Date of birth</th>
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</table>
neonatal" in conjunction with maternal use of an SSRI. However, on perusal, many appear to describe serotonin toxicity. We have also been involved with the management of a neonate, born to a mother following a sertraline overdose, who exhibited features of serotonin toxicity. In this case there was a single maternal ingestion 1 hour before delivery and therefore no earlier foetal exposure to cause withdrawal. We are concerned about the increasing use of the term “neonatal withdrawal syndrome” in symptomatic neonates being born to mothers on SSRIs. This may prompt the use in symptomatic neonates being born to mothers following a sertraline overdose, who exhibited features of serotonin toxicity. In this case there was a single maternal ingestion 1 hour before delivery and therefore no earlier foetal exposure to cause withdrawal. We are concerned about the increasing use of the term “neonatal withdrawal syndrome” in symptomatic neonates being born to mothers on SSRIs. This may prompt the use in symptomatic neonates being born to mothers following a sertraline overdose, who exhibited features of serotonin toxicity. In this case there was a single maternal ingestion 1 hour before delivery and therefore no earlier foetal exposure to cause withdrawal. We are concerned about the increasing use of the term “neonatal withdrawal syndrome” in symptomatic neonates being born to mothers on SSRIs. This may prompt the use in symptomatic neonates being born to mothers following a sertraline overdose, who exhibited features of serotonin toxicity. In this case there was a single maternal ingestion 1 hour before delivery and therefore no earlier foetal exposure to cause withdrawal.

Authors’ response

Editor,—Isbister and colleagues point out and others described. Their argument is that the described syndrome is due to a hyper serotonergic state, rather than a lack of serotonin effect, as the term “withdrawal” suggests. We agree that this issue must be clearly solved because of the significant implications in the clinical management of some of the patients, especially concerning the role of continued breast feeding. At the same time, we are unsure whether we have sufficient data to declare that this is a hyper serotonergic condition. When we started summarising our experience as a report, we debated what terminology should be used to describe our patients. The term “SSRI discontinuation syndrome” was considered as it simply describes the temporal relationship between the dose and the syndrome. However, we opted for “withdrawal” because of its common use in similar cases in the literature. For example, a report by Kent and Laidlaw describes a full term healthy boy born to a mother on sertraline who was breast fed for three weeks. A day after weaning developed agitation, poor feeding, constant crying, insomnia, and an enhanced startle reaction. These effects intensified over 48 hours then subsided. The time course in this case strongly suggests a withdrawal reaction. Our 2 patients had therapeutic serum concentrations of the drug. However, we do not know the concentrations prior to the presentation, hence the interpretation of the data is not as simple as Isbister and the colleagues indicate.

We think that the conditions we described resulted from a hypo-serotonergic state due to withdrawal. However, the possibility of functional excess of serotonin cannot be ruled out from the clinical assessment alone as there is considerable overlap between the two entities. The cause of the discontinuation syndrome in adults also remains incompletely understood.1

References

Neonatal paroxetine withdrawal syndrome or actually serotonin syndrome?

GEOFFREY K ISBISTER, ANDREW DAWSON, IAN M WHYTE, FELICITY H PRIOR, CHRISTINE CLANCY and ANTHONY J SMITH

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