Vitamin K deficient bleeding in cystic fibrosis

We would like to report a female infant (initially breast fed and subsequently formula fed) who had received two 1 mg doses of vitamin K orally, and presented at 9 weeks of age (initially breast fed and subsequently formula feed). The vitamin K deficiency can be attributed to malabsorption secondary to CF, and emphasises the need to consider CF as a diagnostic differential in bleeding diathesis presenting in the first year of life. If a universal neonatal screening programme for diagnosing CF had been in place, a potentially life threatening complication may have been prevented.

T Vergheze, D Beverley
York District Hospital, Wigginton Road, York YO31 3HE, UK; David.W.Beverley@york.nhs.uk

Improving mental health through parenting programmes: are the results valid?

We read the article by Patterson et al with interest. Firstly, the percentage of questionnaires returned from the survey should have been 61.8% not 70%, as reported.

Secondly, mental health problems are prevalent in a wide array of both socioeconomic classes. Unfortunately, working class parents were seriously under-represented in the trial. We wonder whether the maturational effect seen in the middle class was carried out in Oxford and that the socio-economic mix was somewhat biased towards the middle class families than there are in families living in social deprivation.2 Secondly, they point out that this trial was not conducted in other ethnic minority groups.3 We now know that 78% of pancreatic insufficient patients had PIVKA-II concentrations >3 µg/L. Deficiency of vitamin K in children with CF may be due to inadequate dietary intake, malabsorption, and malabsorption.1 Decreased intestinal synthesis of vitamin K, following diarrhoeal disease or antibiotic administration can also be a contributing factor.

Our patient developed vitamin K deficient coagulopathy despite receiving oral supplementation and vitamin K from formula feed. The vitamin K deficiency can be attributed to malabsorption secondary to CF and emphasises the need to consider CF as a differential diagnosis in bleeding diathesis associated with malabsorption secondary to CF or insufficient patients had PIVKA-II concentrations >3 µg/L.

References

Authors’ reply

Drs Srinivas, Gada, Shanker, and Kanumaka make a number of useful points about our trial. Firstly, they query our response rate. This rate may not be calculated using either the number of families or the number of children as the denominator. The rate we quoted 800/1155 is the proportion of families responding. The rate of 61.8% (1105/1788) relates to the proportion of children. Given that this was a trial about parents and parenting we decided that the family based response rate was the most appropriate to report. Secondly, they point out that this trial was carried out in Oxford and that the socioeconomic mix was somewhat biased towards middle class families. Although all social groups were well represented in the trial, the point Dr Srinivas and colleagues make is valid. However, behaviour problems are common in all social groups,1 and because of the distribution of children in each social class, there are considerably more children with behaviour problems in middle class families than there are in families living in social deprivation.2 An important finding in this trial was that those who consented to take part were more likely than those who did not to have a child with problem behaviour. We feel that this validates our population approach. At the same time, it is true that our results may not be totally transferable to Islington. That does not stop them, however, being both valid and important.

Dr Srinivas says that more studies of programmes with parents from lower socioeconomic groups are needed. In fact, the great majority of trials of parenting programmes have been conducted with high risk groups and we know from these trials that they are valuable with families living in social deprivation.3 We are currently completing a systematic review of parenting programmes for minority ethnic families and have found no evidence that parenting programmes are less effective with parents from such groups than are those from majority ethnic groups.

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PostScript

LETTERS

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The editors will decide, as before, whether to also publish it in a future paper issue.
The authors suggest that the changes we have observed in our trial could be a speeding up of a normal maturational effect. Half of the child outcomes we measured showed changes compatible with this interpretation, but the other half do not. The latter show either continuing improvement in both groups or more change in the intervention than control group at six month follow up. We will be publishing the results of our 12 month follow up.

The authors also ask whether our results are clinically significant. The differences between intervention and control group scores at 6 months represent effect sizes of around 0.3 (of a standard deviation). In clinical terms such changes are regarded as small. However in public health terms a small change in a large group is often more important than a big change in a small group, so these differences are of public health significance.

Dr Srinivas and colleagues also ask about cost effectiveness. We did not undertake a formal economic analysis in this study, but the costs of the intervention were mainly in the staff time. Taking account of time spent in supervision, but not training, the costs fall somewhere between six and ten hours of group leader time per parent attending the course. Effectiveness in this context is more difficult to estimate and cannot be measured only in terms of immediate behavioural outcomes. The evidence that the quality of parent-child relationships has a long term impact on mental and physical health and on social well being is mounting. Estimating all the societal benefits of this intervention was beyond the scope of our study but could be very considerable.

Dr Srinivas and colleagues also suggest that our results may be invalid because they were not collected by researchers blind to intervention group. All our outcomes were based on self-report by parents, so blinding of study personnel is irrelevant. It is unfortunately not possible, in trials of health promoting interventions, to blind participants to the intervention. Although it is theoretically possible to mask any observations of some of these outcomes, such approaches greatly increase the cost of studies and were not possible with the funding we had available.

Finally, and perhaps most importantly, Dr Srinivas and colleagues suggest that unequivocal NHS resources should be concentrated where they are needed most, and not on relatively well middle classes. There will be many readers who agree with them. The pros and cons of self-report versus high risk approaches are much debated. The point, however, is that these approaches are not mutually exclusive and authoritative sources1 of advice on child health now recognise the need for both. The argument about population approaches to the promotion of mental health were cogently put many years ago by Geoffrey Rose,2 to whose paper we direct interested readers.

S Stewart-Brown, J Patterson, J Barlow
Institute of Health Sciences, Oxford, UK

Correspondence to: Dr Stewart-Brown, Health Services Research Unit, Institute of Health Sciences, Old Road, Headington, Oxford OX3 7LF, UK; sarah.stewart-brown@public-health.oxford.ac.uk

References

Adrenal crisis due to inhaled steroids is underestimated

In response to comments by Pearce and Mabin on Professor Russell's editorial3 on our paper.

They doubt that our survey underestimated the true scale of the problem. I can inform them that this is not the case. Since our survey was completed, we have notified of a further seven cases (five children, two adults). All but one of the children had been taking fluticasone in similar dosages to those reported in our survey. Three had been critically ill in intensive care and an 8-year-old girl died due to adrenal crisis. The remaining child was only 20 months old and had been given budesonide in extremely high doses of 2000–8000 mcg/day. Both adults had been taking fluticasone (1000 mcg/day, 2250 mcg/day). The case reporting clearly plays a much greater role than clinical studies in post license surveillance. Table 2 shows a compelling 20 year study of drug safety discontinuations, nearly all occurred as a result of case reporting. Despite the studies the authors concluded that “it is impossible to know fully all the facts about a drug’s effects both beneficial and harmful at the time of approval”.4 Further, it is incorrect for Pearce and Mabin to claim “mean plasma cortisol concentrations have lasted greater than one year?”5

Cushing’s disease due to inhaled steroids is underestimated. The authors also ask whether our results add to whose paper we direct interested readers. We will be publishing the results of our 12 month follow up.

Pearce and Mabin correctly state that in recent years, when paediatricians are prescribing high doses of inhaled corticosteroids are necessary, more are choosing to prescribe fluticasone propionate. However, they need to explain why only 2 cases of adrenal crisis (one adult) in over 30 years of prescribing inhaled corticosteroids had ever been reported in literature before the introduction of fluticasone propionate allowing Russell to make a claim in 1994 that “there is no firm evidence that any child has died as a result of adrenal suppression induced by inhaled corticosteroid therapy”.6 Further, some cases reported in our survey had previously been taking very high doses of either beclometasone or budesonide but only developed adrenal crisis some time after changing to fluticasone.

Finally, it is unfair to blame doctors for prescribing fluticasone “off label” because companies will not make available data on all drug prescriptions for children in hospital are either unlicensed or off label.7 Prescribers have every right to expect a reasonable margin of safety with a drug should they prescribe it for children. Bearing in mind that there have now been two reported deaths and many intensive care cases, the risks of prescribing fluticasone are considerable and harmful at the time of approval.8

Moderately high doses still need to be considered for very young children

In relation to the question of adrenal suppression when using higher doses of inhaled corticosteroid, I believe there is an aspect of dose selection which has not been mentioned by previous authors.

There are limited data on the question of intra-pulmonary drug deposition in children under 3 years but the studies that have been published seem to indicate that around 1–2% of the drug released into the spacer reaches the airways,1 compared to 15–17% in an adult using the same device. Based on this figure, it seems reasonable to prescribe similar doses to very young children and adults alike.

I note that none of the cases of adrenal impairment have been reported in children under 3 years of age; most of them are significantly older. This could be partly because higher doses are not being used in this age group, but might also be confirmation that a smaller fraction of the drug reaches the airways.

I would argue that there are good reasons to use higher doses, at least initially, when treating very young children. The diagnosis of asthma is exceptionally difficult here, and if a “trial of treatment” is ineffective, one wishes to be reasonably confident that the reason for the negative response was not related to an inadequate dose. A negative response allows the clinician to withdraw ineffective steroid treatment in those infants who may well not have asthma at all. If there is an excellent response, the dose of steroid should be stepped down to the minimum required to control symptoms.

Finally, for clarity, the doses I am referring to are budesonide/beclomethasone 800 mcg/day or fluticasone 300 mcg/day.

D P Cochran
Respiratory Unit, Royal Hospital for Sick Children, Yorkhill, Glasgow G3 8SJ, UK; dominic.cochrann@rgh.sct.nhs.uk

Cultural representation of newborn feeding

Nicoll and Williams' suggested that attitudes to breast feeding need to change: “everyone (not just women) needs to see breast feeding as normal and education needs to start early”. In Italy breastfeeding rates are low.3 Numerous training initiatives have been set up to heighten awareness with the aim of promoting breastfeeding. These initiatives have been based on implementation of the Baby Friendly Hospital Initiative; three hospitals in the country being nominated “Baby Friendly”.

I was recently invited to discuss the importance of breastfeeding for newborns with two 4 year junior school classes (41 children in total (17 girls and 24 boys), aged between 9 and 10). Before talking to the children, I asked them to draw on a sheet of paper everything they thought was necessary for a baby to grow up healthy. All except four drew a feeding bottle next to a baby: 15 children drew a baby alone with a bottle; only three children drew a baby in his/her mother’s arms, but all these the babies were still holding a bottle. Only two drawings showed the baby with both parents and in without a bottle; the other two drawings without a bottle depicted a scene in the hospital. When I asked how many of them thought that formula milk was the same as mother’s milk, 28 out of 41 raised their hands. I believe this reflects the widespread tendency, also reported in other countries, not only to consider breastfeeding the same as artificial feeding, but “artificial” as “natural”.

In an historic and ever pertinent editorial,1 the Lancet hoped a warm chain for breastfeeding could be created, and warned about the ambivalent messages often encouraged by the marketing campaigns of formula manufacturers. I feel that the implementation of interventions aimed at supporting breastfeeding should not be limited to the healthcare system, but should cover a wider range of activities, aimed at changing the cultural representation of newborn feeding and at defending breastfeeding.

S Conti Nibali
Family Pediatrician, Azienda Sanitaria Locale, n.5 Messina, Italy; seconti@glauco.it

References