**LETTERS**

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**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 mg doses of vitamin K orally, and presented at 9 weeks of age with large haematomas at the sites of her primary immunisations. Her weight had dropped from 25th to 90th centile at birth to the 2nd centile.

Her haemoglobin was 720 g/l, white blood cell count 13×10⁹/l, platelet count 523×10⁹/l, prothrombin time >10 seconds (normal range 0.8–1.2), activated partial thromboplastin time 109.4 seconds (normal range 24.0–34.0), and fibrinogen 4.5 g/l (normal range 1.7–4.5). She received 1 mg of vitamin K intravenously and repeat coagulation screen was then normal. Sweat osmolality was 110 mmol/l (normal range 17–80) and 105 mmol/l on repeat testing. No chymotrypsin activity was found in the facies. DNA analysis confirmed homozygosity for delta F 508.

Vitamin K deficiency can occur in undiagnosed cystic fibrosis (CF) infants due to malabsorption of fat soluble vitamins. It is uncommon, since vitamin K is given to all newborns in the UK. As universal screening for CF is not undertaken in the UK, asymptomatic CF patients can be missed and a bleeding diathesis may be the presenting symptom.

Torstenson and colleagues reported three cases of severe life threatening bleeding subsequently diagnosed as CF in infants less than 6 months of age, and Rashid and colleagues found 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. 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 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.

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

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

Secondly, mental health problems are prevalent in families of both lower socioeconomic class. Unfortunately, working class parents were seriously under-represented in the study. The results from educated and pre-eminent families from Oxford are not applicable to areas like ours. In the Camden and Islington boroughs of London, we work with parents of mostly lower socioeconomic class and of varied ethnicity—from Albania to Zaire—to whom these results are not relevant. We need more studies conducted in these people to know the best evidence.

Thirdly, the intervention effect is seen at 6 months (short term) follow up. We wonder whether the maturational effect seen in the control group will actually decrease the effect of parenting in the intervention group in the long term? Moreover the intervention effect is said to be statistically significant. But is it clinically significant as well? And there is no cost-benefit analysis given. Does this justify the considerable use of resources, especially in today’s cash strapped, staff depleted (fewer health visitors) NHS? Furthermore, parents in the intervention group might have believed that the parenting programme is efficacious, and consequently feel and perform better than those who were in the control group, as they were aware of group allocation. Also, unblinded study personnel who are measuring and recording outcomes (such as quality of life) may provide different interpretations of the findings, which can distort the results. We now know that negative, inconsistent parental behaviour in families with high levels of adversity are associated with emergence of problems in early childhood and later life.

Hence, we believe that parenting interventions should be applied in high risk populations. That is parents of children with ECBI scores of 127 or more and not children with 100 and above as included in the study. It would have been helpful if authors gave ECBI and SDQ scales as a web supplement to the above article.

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**Authors’ reply**

Dr Srinivas, Gada, Shanker, and Kanumakala make a number of useful points about our trial. Firstly, they query our response rate. This rate can 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 is 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 parents. 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, 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. 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. 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 they are with those from majority ethnic groups.
The authors suggest that the changes we have observed in our trial could be a speedup 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 somewhat 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 make population level 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 many studies arguing against this intervention have failed to follow up their initial results beyond 6 months. The impact on mental and physical health and on parent-child relationships has a long term course. Effectiveness in this context is more likely to be assessed in terms of delayed effects, as measured by children’s outcomes we measured showed changes 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.

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Adrenal crisis due to inhaled steroids is underestimated

In response to comments by Pearce and Mabin on Professor Russell’s editorial 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. Therefore, it is not obvious that it is due to an idiosyncratic reaction to the drug. 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). Case reporting clearly plays a much greater role than clinical studies in post license surveillance. Fluticasone is one of the better studied of all the available inhaled corticosteroids. Almost all drug prescriptions for children in hospital are either unlicensed or off label. Prescribers have every right to expect a reasonable margin of safety with a drug they should decide if drug labels are not necessary in children. Bearing in mind that there have now been two reported deaths and many intensive care admissions, the risks of prescribing fluticasone must outweigh the possible benefits for patients, and will have serious medico-legal implications for doctors, particularly when there is not a single study showing better efficacy for fluticasone compared with other available inhaled corticosteroids.

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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, 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 500 mcg/day.

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Reference


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. 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 4th 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, only not to consider breastfeeding the same as artificial feeding, but “artificial” as “natural”.

In an historic and ever pertinent editorial, 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.

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Cultural representation of newborn feeding

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