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 1 mg doses of vitamin K orally, and presented at 9 weeks of age with large haematomas at the sites of her previous injections. Her weight had dropped from 2530 g to 2015 g at birth to 1990 g at 8 weeks.

Her haemoglobin was 720 g/l, white blood cell count 13×10^9/l, platelet count 325×10^9/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 faeces. 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.

Torgerson 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 PIYKA-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.

T Verghese, D Beverley
York District Hospital, Wiggins ton 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 questionnaire returns from the survey should have been 61.8% not 70%, as reported.

Secondly, mental health problems are prevalent in a range of socioeconomic class. Unfortunately, working class parents were seriously under-represented in the study. The results from educated and pre-nanry class parents 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 Zambia—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 more evidence of trials, 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.

S S Gada, S Kanumakala
Department of Paediatrics, The Royal Free Hospital, Pond Street, London NW3 2QG, UK
Correspondence to Dr Gada; sni1@doctors.org.uk

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 was 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 actual 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 parents. Although all social groups were well represented in the trial, the point made 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 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 randomise patients to receive an intervention with fluticasone or placebo, fewer than 1% of all drug prescriptions for children in hospital and up to 10% of all drug prescriptions for children in primary care cases, the risks of prescribing fluticasone are either unlicensed or off label. ‘Off label’ use of all drug prescriptions for children in hospital are either unlicensed or off label. ‘Off label’ use of 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 that off label dosages are necessary in children. Bearing in mind that there have now been two reported deaths and many intensive care cases, the risks of prescribing fluticasone for children appears to be great, but is significantly less than 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."

Pearce and Mabin correctly state that "individuals having different sensitivities to inhaled corticosteroids, Idiosyncratic re- sponses to inhaled corticosteroids may occur even at licensed doses”. I agree, however, such patients “disappear” in large multicentre studies, particularly when the pharmaceutical company will not make available data on individual patients. This is of concern as there is evidence that fluticasone is associated with significantly more intensive care than low cortisol values than other inhaled steroids. Also, over 75% of patients developed adrenal crisis greater than one year after starting fluticasone, and it is known that length of time taken inhaled adrenocortical hormones is a major factor in determining the frequency of side effects. How many studies of fluticasone have lasted greater than one year?

Pearce and Mabin correctly state that in recent years, when paediatricians are confident that 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 (both adults) 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 ever died as a result of adrenal suppression induced by inhaled corticosteroid therapy”. Further, some cases reported in our survey had previously being taken very high doses of either beclomethasone 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”. Almost half of 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 that off label dosages are necessary in children. Bearing in mind that there have now been two reported deaths and many intensive care cases, the risks of prescribing fluticasone for children appears to be great, but is significantly less than 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."

R G Todd
Antim Hospital, 45 Bush Road, Antim SR1 2IL, UK, drgeoffreytodd@hotmail.com

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Under 3 years but the studies that have been
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Dose selection which has not been mentioned
corticosteroid, I believe there is an aspect of
question of adrenal suppression when using higher doses of inhaled


Eid N, Morton R, Olds B, et al Decreased morning serum cortisol levels in children

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PostScript

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. Nicoll, A; Williams A. Breastfeeding an


Cattaneo A, Buzzetti R, on behalf of the
of breast feeding of training for 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, not only 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 inter-
ventions 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; seconlta@glauco.it

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Moderately high doses still need

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 treat-
ing 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/benomethasone 800 mcg/
day or fluticasone 500 mcg/day.

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

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S Conti Nibali
Family Pediatrician, Azienda Sanitaria Locale, n.5
Messina, Italy; seconlta@glauco.it
Adrenal crisis due to inhaled steroids is underestimated

G R G Todd

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