Iron deficiency anaemia, *Helicobacter pylori* infection and delayed pubertal growth

We read with great interest the paper by Choe et al. concerning our paper on iron status and development. We wish to raise two matters not discussed in Dr Morley’s letter.

Firstly, when the paper was published, there was prominent media coverage, erroneously concluding that our paper rendered cow’s milk unnecessary in the later part of the first year, and that cow’s milk was just as satisfactory to children with Down’s syndrome as breast milk. We wish to correct this misinterpretation through the media.

Secondly, the haemoglobin data we included were not central to the focus of the paper, from those central factors and should not be taken as typical of those for the UK population. A more complete presentation of iron status in children in the trial is now published.

The current evidence on the importance of iron in infancy is much debated and frequently cited in policy documents. We wish to highlight that community based testing is also feasible for CD.

A study at our centre investigating the prevalence of coeliac disease in type I diabetes utilised patient self sampling for screening of blood samples. Blood was drawn into a lithium heparin capillary tube (Monovette, Sarsdedt Ltd, Germany) or onto filter paper. The in house assays used for AGA and EMA were performed on 10–20 ml of serum or plasma; thus capillary samples were more than adequate. This method could easily be incorporated into the “at school” testing described by the authors.

Annual screening for hypothyroidism is recommended. How often screening should be performed for CD is still a matter of debate. With their proposal to establish a Scottish-wide opportunity to perform a Scottish-wide population study for the prevalence of coeliac disease in Down’s syndrome and, more importantly, to identify those children who may benefit from early detection. Community based screening with capillary samples would make that a very realistic prospect.

R K RUSSELL
P M GILLET
Department of Paediatric Gastroenterology,
Royal Hospital for Sick Children,
9 Science Museum Road, Edinburgh EH8 9LP, UK
email: rickrussell771@hotmail.com


LETTERS TO THE EDITOR

**Rapid responses**

If you have a burning desire to respond to a paper published in *Arch Dis Child* or *FeNi* why not make use of our “rapid response” option?

Log on to our website (www.archdischild.com), find the paper that interests you, click on “full text” and send your response by email by clicking on “submit a response”.

Providing it isn’t libellous or obscene, it will be posted within seven days. You can retrieve it by clicking on “read rapid responses” on our homepage.

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

**Iron deficiency anaemia, *Helicobacter pylori* infection and delayed pubertal growth**

We read with great interest the paper by Choe and colleagues which investigated a possible relationship between *Helicobacter pylori* infection, iron deficiency anaemia (IDA), and subnormal growth at puberty.1 The authors concluded that *H pylori* infection and related IDA, rather than bacterial infection alone, might cause delayed pubertal growth. However, we believe that these results need some reconsideration.

*H pylori* infection may cause IDA in different ways: (a) the bacteria can cause a decrease in the gastric juice of the concentration of ascorbic acid, which is the best promoter of non-heme iron absorption;2 (b) *H pylori* may increase iron demand because iron is an essential bacterial growth factor; (c) *H pylori* contains a 19.6 kDa protein resembling ferritin with a binding activity for heme iron in erythrocytes;3 (d) acute or chronic blood loss and IDA are associated with typical symptoms of subclinical/silent coeliac disease: an analysis on 1026 consecutive cases. *Dig Dis Sci* 1999;44:691–6.

**Iron status and development**

**EDITOR,—**You have recently published a response by Dr Morley to Dr Stevens concerning our paper on iron status and development.1 We wish to raise two matters not discussed in Dr Morley’s letter.

Firstly, when the paper was published, there was prominent media coverage, erroneously concluding that our paper rendered cow’s milk unnecessary in the later part of the first year, and that cow’s milk was just as satisfactory as breast milk. We wish to correct this misinterpretation through the media.

Secondly, the haemoglobin data we included were not central to the focus of the paper, from those central factors and should not be taken as typical of those for the UK population. A more complete presentation of iron status in children in the trial is now published.

The current evidence on the importance of iron in infancy is much debated and frequently cited in policy documents. We wish to highlight that community based testing is also feasible for CD.

A study at our centre investigating the prevalence of coeliac disease in type I diabetes utilised patient self sampling for screening of blood samples. Blood was drawn into a lithium heparin capillary tube (Monovette, Sarsdedt Ltd, Germany) or onto filter paper. The in house assays used for AGA and EMA were performed on 10–20 ml of serum or plasma; thus capillary samples were more than adequate. This method could easily be incorporated into the “at school” testing described by the authors.

Annual screening for hypothyroidism is recommended. How often screening should be performed for CD is still a matter of debate. With their proposal to establish a Scottish-wide opportunity to perform a Scottish-wide population study for the prevalence of coeliac disease in Down’s syndrome and, more importantly, to identify those children who may benefit from early detection. Community based screening with capillary samples would make that a very realistic prospect.


Consider absolute risks in SIDS prevention

EDITOR,—The demonstration by Blair et al of an association between poor postnatal growth and an increased risk of sudden infant death syndrome (SIDS) is a useful addition to our understanding of the aetiology of this condition. It is unfortunate that the conclusion in the abstract that “Poor postnatal weight gain was independently associated with an increased risk of SIDS and could be identified at the routine six week assessment” goes beyond the data presented.

It can be estimated from the data in this study that the overall risk of SIDS was 0.77/1000 live births. The risk in babies with birth weights greater than the 15th centile, the group in whom the relation with postnatal growth was detected, was 0.68/1000. Given the reported odds ratio of 1.75 associated with being in the slowest growing 16% who might be identified at six weeks, the data suggest that the absolute risk of SIDS among this group would be about 1.1/1000. Even a programme targeted at infants below the 2nd centile for growth at six weeks, would identify a group whose absolute risk of SIDS was about 4.2/1000—that is, for every infant who might benefit from the intervention, there would be 25 who would not. Even if it were accepted that this level of risk was sufficient to trigger an intervention, the nature of the intervention remains unclear. To the best of our knowledge, with the exception of the “Back to Sleep” campaign, there is no convincing evidence of the effectiveness of any intervention aimed at preventing SIDS. None of the intervention programmes described in the accompanying commentary1 have been evaluated in appropriately controlled studies.

The discussion in the paper is rather more circumstantial than the conclusion in the abstract, but it is the latter which, reinforced by the accompanying commentary, is likely to have disproportionate impact on those readers who do not read the whole paper. In the commentary Dr Carpenter acknowledges that the low rates of SIDS make such interventions difficult to justify but then suggests that “Targeting such infants at home . . . will also identify numerous other problems at a remedial stage.” This is a suggestion for which there is currently little evidence.

There are clear dangers in recommending population interventions on the basis of relative risks without taking absolute risks or of the need for interventions to be of proven effectiveness.

STUART LOGAN
HELEN BEDFORD
Department of Paediatric Epidemiology,
Institute of Child Health, 30 Guilford Street,
London WC1N 1EH, UK
email: slogan@ich.ucl.ac.uk

DAVID ELLIMAN
St George’s Hospital,
Blackshaw Road,
London SW17 0QT, UK


Drs Blair, Fleming, and Platt comment:

Poor postnatal weight gain was a significant factor in the multivariate analysis despite controlling for low birth weight, prematurity, neonatal problems, poor socioeconomic sta-

us, and many other potential confounding factors, and remained significant when fur-

ther highly predictive covariates of SIDS such as infants put down prone, infants found with head covered, and tobacco exposure were added to the model. In this sense postnatal weight gain was independently associated with an increased risk of SIDS. We also found the difference in weight gain between the SIDS and control infants measured from birthweight to the last known weight was equally apparent if measured from birth to the six week assessment. We therefore stand by our conclusion in the abstract that poor postnatal weight gain “was independently associated with an increased risk of SIDS” and also that poor postnatal weight gain “could be identified at the routine six week assessment”.

We find it difficult to understand how this conclusion advocates an intervention campaign on the basis of these two statements and disagree with Logan and colleagues that our discussion on this matter was circum-

spect. We point out clearly that poor weight gain itself is not a sensitive marker and that it “should be seen as a thread in a web of factors that render an infant vulnerable to SIDS and is both a consequence of adverse health and social conditions”.

We agree that absolute risks must be used for targeted prevention campaigns but do not advocate such a campaign based on our soli-

tary finding. Preliminary analysis of risk scor-

ing on the first two years of our dataset, tested after the campaign, is likely to have disproportionate impact on those read-

ers. None of the intervention programmes described in the accompanying commentary1 have been evalu-

ated in appropriately controlled studies.

Are we requesting too many DMSA scans?

EDITOR,—The recent article by Christian et al highlights the value of clinical features in assessing the risk of renal scarring and there-

fore the need for dimercaptosuccinic acid (DMSA) scan after urinary tract infection (UTI).2 We recently performed a study to assess the recording of fever, malaise, recurrent UTI, and urine culture results in children investigated with DMSA scan after UTI. Between April 1996 and October 1997 there were 171 DMSA scans in our hospital that fitted these criteria; 30 case notes could not be traced. There were 105 girls (74%) and 36 boys. Age when UTI was diagnosed ranged from 9 days to 15.3 years (mean 4.2 years, SD 3.2).

Urine culture results were: UTI (>10^5 cfu/ml) in 82 cases (58%), contaminant (>10^4 cfu/ml) in 27 cases (19%), no growth in 21 cases (15%), and no urine culture in 11 cases (8%). There were 17 (12%) cases of definite or probable renal scar, none of which followed a sterile or contaminated urine cul-

ture. Of the 141 case notes, there was no mention of fever in 48 (34%), and no mention of malaise in 76 (54%). In 69 case notes reviewed there was no mention of previous history of UTI in 14 (20%) cases. Of those with a history of fever, 19% (10/53) had an abnormal DMSA scan compared to 10% (4/40) in those without fever. Eighteen per cent (9/50) of those unwell at the time of UTI had an abnormal scan compared to 13% (2/13) of those not ill.

These data suggest that in a substantial proportion of cases, the decision to request a DMSA scan is apparently not influenced by salient clinical features and urine culture results. In this series, it appears that those children with sterile or contami-

nated urine cultures should not have had a DMSA scan. This would have saved the cost and burden of 48 scans, 34% of this series, over an 18 month period. It is likely that these findings are peculiar to our district.

A H SACKEY
Leighton Hospital,
Middlewich Road, Crewe CW1 4QF, UK
email: ahsackey@lson.net


www.archdischild.com