

PostScript

LETTERS

Euroaspirations

We read with interest the recent article of Papadopoulos *et al.*¹ The Euro was accepted with remarkable alacrity by the peoples of the eurozone. The franc, mark, guilder, lira, and other currencies were effectively replaced within one week in February 2002. The euro changeover was effective, efficient, and essentially painless. The currency has prospered with 20% growth since its introduction and is now worth approximately 70 pence sterling and one US dollar 15 cents.

In the first few months of the euro, we too noticed several children presenting to our emergency department having decided to see how well the euro went down. These were children in the oral exploratory ages of 1-3 years. As a consequence and for guidance to our emergency department colleagues, a "eurometer" was made (fig 1). Some of the euro coins are small and some ended up in the upper airway, the oesophagus, and the stomachs of toddlers and preschool children (fig 2). We have seen 22 such patients in first eight months of this year. A few euroretrievals were required.

The UK's euro debate sees the euro as a threat to the national pound. Has anyone else considered the euroaspiration? "Should the UK stay in or out of the euro?" ask the headlines? Sweden plunged into controversy when it consulted its people in a euro

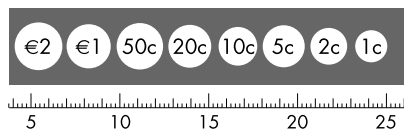


Figure 1 Eurometer.

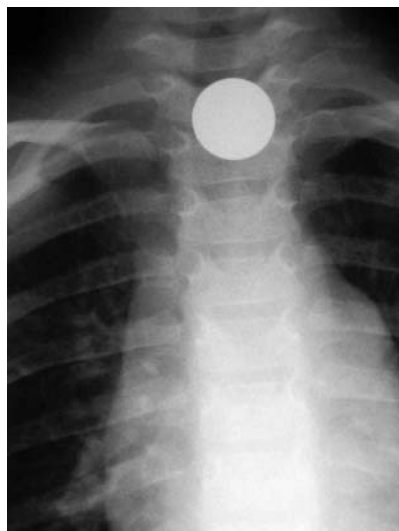


Figure 2 1 Euro coin in upper oesophagus.

referendum. Denmark is sitting on the fence. The UK is discussing and dallying on the issue.

All accident and emergency departments in the eurozone ought to be cognisant of the potential of the 1 cent coin to lodge in the oesophagus or sit in the upper airway. The differential diagnosis of any toddler with acute upper airway obstruction should, in the eurozone, include eurostridor.

We, like Dr Papadopoulos and colleagues, warn that if one aspires to euroconvert, one must accept euroaspirations.

D G Gill, S Ryan

Children's University Hospital, Temple Street, Dublin 1, Ireland; gilld@iol.ie

doi: 10.1136/adc.2003.045419

Reference

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Thrombocytopenia is predictive of lethality in severe childhood falciparum malaria

Moulin and others¹ reported that thrombocytopenia was not a marker of severity in children suffering falciparum malaria. In a previously published study,² we have shown a highly significant association between thrombocytopenia and either severity or prognosis in childhood falciparum malaria. To our knowledge, this association had not been noted³ or looked for⁴ before.

Our study took place from October 1997 to March 1999, in the paediatric department of the Hôpital Principal in Dakar, Senegal, where clinical presentation, WHO criteria of severe malaria, and platelet count were prospectively recorded. Of 288 falciparum cases, 215 matched the 2000 WHO definition of severe malaria. Median platelet counts were lower (98 000/mm³ versus 139 000/mm³; $p < 0.02$) among severe cases than in mild cases, and in children who died ($n = 26$) than among those who recovered (68 500/mm³ versus 109 000/mm³; $p < 0.002$). In severe cases, children presenting with a platelet count $< 100\,000/\text{mm}^3$ were more likely to die (20%, 22/110) than those with higher platelet count (3.8%, 4/105; odds ratio (OR) 6.31, 95% confidence interval (CI) 2.0 to 26.0; $p < 0.0003$). Moreover, multivariate analysis identified thrombocytopenia as an independent predictor of death (OR 13.3, 95% CI 3.2 to 55.1; $p < 0.0001$)—that is, when the effect of cerebral malaria, respiratory distress syndrome, severe anaemia, and other severe malaria criteria was taken into account.

The absence of an association between thrombocytopenia and clinical malaria severity in the samples studied by Moulin and others does not prove its non-existence. Moreover, their study suffers from several limitations that may explain the discrepancy between their and our conclusions. First, they have observed only four deaths among 234 malaria cases. Their survey was consequently powerless to establish any association between thrombocytopenia and lethality.

The low level of lethality among severe cases (3.5%, 4/112) also suggests that clinical presentations observed in the Moulin *et al* study were less life threatening than in ours, even if 69 of their severe cases have been admitted in the same paediatric department where our study was conducted previously. It is well established that the case fatality rate varies according to clinical presentation⁵ and definition of severe falciparum malaria criteria.⁶ Thus any analysis of prognostic factors of malaria lethality must take into account the composition of the sampled cases in terms of clinical presentations, for example, analysing the different syndromes separately. In our study, the association between thrombocytopenia and lethality was significant among children with cerebral malaria or respiratory distress but not among children only presenting with severe anaemia, convulsions, or hypoglycaemia. It is possible that the composition of the severe cases sampled by Moulin and others may have confused the association. Age is another confounding factor that was not controlled in the Moulin *et al* study. We showed that age was associated with both lethality and thrombocytopenia.

Furthermore, the exposure of children to malaria may be not so different between the samples studied by Moulin and others. In large areas of Dakar, the level of transmission is very low, less than one infective bite every 10 years. It is thus likely that a significant proportion of the children from Dakar, and to a less extent from Libreville, had never been infected before by *Plasmodium falciparum*. Without reliable information about the exact location of their habitat, it is inappropriate to estimate their level of previous exposure to malaria in urban areas where the level of transmission is heterogeneous.

Finally, it must be stressed that in the studies of prognostic factors of lethality, the occurrence of death rather than substitute variables, for example, severe malaria criteria, is the variable of interest. Among children suffering severe falciparum malaria, thrombocytopenia ($< 100\,000/\text{mm}^3$) should be considered as predictive of a fatal outcome, especially in those with cerebral malaria or respiratory distress.²

C Rogier

Parasitology Unit, Institut de Médecine Tropicale du Service de Santé des Armées-IFR 48, Le Pharo, Marseille, France; christophe.rogier@wanadoo.fr

P Gerardin

Neonatal and Pediatric Intensive Care Unit, Hôpital Alfred Isautier, Saint-Pierre, Reunion Island, France

P Imbert

Department of Infectious Diseases and Tropical Medicine, Hôpital d'Instruction des Armées Bégin, Saint-Mandé, France

doi: 10.1136/adc.2003.045179

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Judged by our legacy

Current global health strategies focus on important issues in child health, such as the eradication of polio by 2005 and the drastic reduction of child mortality by the year 2010. These short term goals are essential to provide the necessary political focus and public health impetus. However, the ultimate success of our current health initiatives will be measured by their ability to provide sustained health to present and future generations.

At the beginning of the third millennium we celebrated the tremendous strides that health care has taken in the past century, while rightfully reflecting on current global inequities in access to health care. Reflection also emphasises the unequalled human impact exerted on our planet in the 20th century and the environmental responsibility that faces health care providers in the 21st century.

As paediatricians we need to provide an articulate voice for all the children of our planet, both for current and future generations. Current initiatives stir more emotion and elicit more political commitment, but protecting the health of future generations is as much our ethical responsibility, as the reduction of present mortality. Short term goals are important, but we have to redefine what is meant by the attainment of child health for all, within the framework of sustainability. The real challenge facing our generation is to improve child health for all, now and in the future.

B J Marais

Centre for TB Research and Education, Faculty of Health Sciences, University of Stellenbosch, PO Box 19063, Tygerberg, Cape Town 7505, South Africa; bmarais@sun.ac.za

doi: 10.1136/adc.2003.045898

Response to salbutamol by wheezy infants

We previously reported a randomised controlled trial of β_2 agonist in wheezing infants.¹ Within this paper we reported no measurable physiological response to 400 μ g salbutamol. Pulmonary function tests were performed after the completion of a symptom diary study and prior to enrolment in a study of inhaled corticosteroid.² Raw values of maximum flow at functional residual capacity ($V'_{\max\text{FRC}}$) and bronchodilator response were reported as there were no suitable standard values for $V'_{\max\text{FRC}}$ available, either from our laboratory or internationally.

Subsequently, collaboration between centres in the UK and USA has produced enough

Table 1 Changes in pulmonary function following salbutamol in infants grouped by baseline $V'_{\max\text{FRC}}$ Z score

	Baseline Z score ≤ -2	Baseline Z score > -2
Number	7	22
Mean change (SD) in $V'_{\max\text{FRC}}$ (ml/s)	18.7 (19.3)	-2.38 (31.8)
Mean change (SD) in $V'_{\max\text{FRC}}$ Z score	0.37 (0.34)	-0.05 (0.39)*
Mean % change (SD) from baseline $V'_{\max\text{FRC}}$	30.6 (24.8)	-1.8 (18.7)**

* $p < 0.05$, ** $p < 0.005$.

data to allow calculation of standard deviation scores (Z scores) for $V'_{\max\text{FRC}}$.³ We have now reanalysed our data in the light of this new information.

Of 29 subjects, seven had abnormal baseline $V'_{\max\text{FRC}}$ Z scores (≤ -2). In comparison with subjects with normal baseline $V'_{\max\text{FRC}}$ (Z score > -2) these patients showed a significantly greater response to salbutamol (see table 1). Six of seven subjects showed a significant change in $V'_{\max\text{FRC}}$ ($>12.5\%$ or $>2 \times$ group coefficient of variation) compared to 3/22 in the normal group (see fig 1). This suggests that patients with identifiable obstruction at the time of testing are more likely to respond to inhaled salbutamol.

The evidence for efficacy of bronchodilators in early life is lacking. Although many intervention studies have been performed, the majority have suboptimal methodology or insensitive outcome measures.⁴ Infant pulmonary function studies are usually performed at a time the child is asymptomatic due to concerns of sedating ill patients, reducing the opportunity to measure a response. Due to the protocol design of our study, subjects could only have pulmonary function measured during a two week period^{1,2} and thus, although not acutely unwell on the day of testing, may have had a recent exacerbation or upper respiratory tract infection with persisting changes of airway obstruction. This may have allowed us to measure this response, although this was not apparent without the use of an appropriate reference standard.

Post hoc subgroup analysis of this kind should always be interpreted with caution. An alternative explanation for these findings could be regression towards the mean, with those with worst baseline function becoming more "normal". While this data involves only

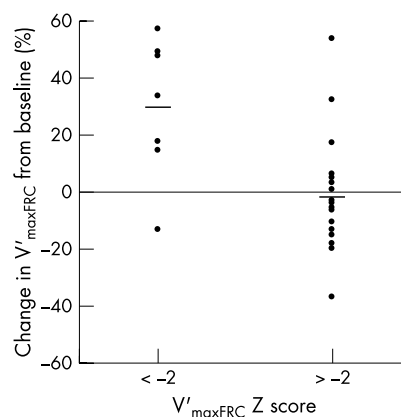


Figure 1 Percentage change in $V'_{\max\text{FRC}}$ after 400 μ g salbutamol in recurrently wheezy infants grouped by baseline $V'_{\max\text{FRC}}$ Z score.

small numbers and no control group, it does indicate a possible benefit of salbutamol in infants with demonstrable airway obstruction at the time of testing, and invites further studies using appropriate techniques and reference standards.

R J P G Chavasse, P Seddon

The Royal Alexandra Hospital for Sick Children, Dyke Road, Brighton, UK

Correspondence to: Dr R J P G Chavasse, Queen Mary's Hospital for Children, Wrythe Lane, Carshalton, London SM5 1AA, UK; richard.chavasse@epsom-sthlier.nhs.uk

doi: 10.1136/adc.2003.040949

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The use of insulin pumps improves the metabolic control in children and adolescents with type 1 diabetes

We greatly appreciated the article by Torrance and colleagues¹ about the use of insulin pumps and we agree with their conclusion that the benefits of continuous subcutaneous insulin infusion (CSII) outweigh the disadvantages. Our three year experience with CSII at the Juvenile Diabetes Regional Centre of Tuscany has shown that not only does this form of insulin administration enhance the compliance in children and teenagers with type 1 diabetes (T1DM), but it also represents an effective way to improve the metabolic control of our patients.

We studied the entire group of 34 (16 males, 18 females) T1DM patients aged up to 18 years followed at our centre, who in the period from January 2000 to November 2002 started CSII therapy and continued it for at least one year without interruption. At the time of attaching the pump the mean age was 14.4 years (range 9–17.8) and the mean duration of diabetes was 6.2 years (range 0.6–15.8). We found that the mean HbA1c values of the group decreased from 8.35% (SD 1.08) at the beginning of the treatment

with CSII, to 7.81% (SD 0.95) 12 months later (paired *t* test: *p* = 0.002). In addition, the mean daily insulin requirement of the patients dropped by 23.7%, from 58.2 IU (SD 15.3) to 44.4 IU (SD 11) (paired *t* test: *p* < 0.001); the mean body mass index did not vary significantly in the period (from 20.7 (SD 2.5) to 21.2 (SD 2.4)). During the period studied no episodes of hypoglycaemia occurred; one episode of ketoacidosis was caused by displacement of the cannula. No episode of local infection occurred. Three patients discontinued the CSII after the first year and one after the second year of treatment.

Our experience shows that use of an insulin pump improves the metabolic control of T1DM in children and adolescents, and reduces the daily insulin requirement.

S Toni, M F Reali, A Fasulo, P Festini, A Medici, M E Martinucci

Tuscan Regional Centre for Juvenile Diabetes, Meyer Pediatric Hospital, via L. Giordano 13, Florence 50132, Italy; mf.reali@meyer.it

doi: 10.1136/adc.2003.046805

Reference

- 1 **Torrance T**, Franklin V, Greene S. Insulin pumps. *Arch Dis Child* 2003;**88**:949–53.

Read the label carefully

The figure shows the packaging of a “rice slice”, which a mother gave to her 23 month child, believing it to be free of any milk. The patient had an anaphylactic reaction shortly after ingesting a very small amount. On close inspection of the ingredients, casein is listed but not qualified as a milk protein.

The child initially presented at 8 weeks of age with a cutaneous reaction to cows’ milk formula on her second exposure, having previously been breast fed. She had raised specific IgE level to milk and a positive skin prick test (3 mm wheal with 6 mm erythema). Thereafter she was managed with an extensively hydrolysed formula and the family were given advice to avoid all milk and its derivatives. They were prescribed antihistamine but not an adrenaline auto-injector.

This case illustrates the difficulty of managing allergy in real life. It is easy to see how a product described as a “delicious alternative to cheese” could be wrongly thought to be milk free unless the ingredients are closely scrutinised. Thirty per cent of children diagnosed as allergic have been shown to have a further exposure in the year after diagnosis.¹ A further difficult issue in clinical practice is when to prescribe adrenaline, especially for the youngest patients in whom



there is no proprietary device in the correct dose for size.²

B Laguda, M E Coren, G Lack

Department of Paediatrics, St Mary's Hospital, London W2 1NY, UK; bisolal@hotmail.com

doi: 10.1136/adc.2003.040501

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Board senseless

The ward patient name board is a familiar sight, placed prominently on most hospital wards. Concerns regarding guidance on patient confidentiality,¹ stemming from the Caldicott report,² led our trust to remove the boards from the general areas of the paediatric wards. They were placed in a less public area—generally the treatment room. It led to delays in staff being able to identify a child's location and their nurse's identity, and general dissatisfaction among the clinical teams.

The parents of 20 patients (age range 11 months to 13 years) on our regional paediatric oncology ward completed a questionnaire. Parents who had only recently received the diagnosis were excluded. Parents responded to five statements, with “strongly agree, agree, disagree, strongly disagree, or neither”.

(1) *I object to having my child's name and location on the board where everyone else can see it*—17 disagreed (11 strongly), with 1 parent agreeing.

(2) *I think that having the centrally placed name board helps the people looking after my child to quickly find out where my child is and who is looking after them*—19 agreed (14 strongly), with 1 disagreeing.

(3) *I think having my child's name on the board represents a risk to their safety*—18 disagreed (11 strongly), with no parents agreeing.

(4) *I like to be able to look at the board to see which other patients whom we know are on the ward*—18 agreed (13 strongly), with no disagreement.

(5) *I would be happy for the name board to be re-introduced*—19 agreed (15 strongly) with no disagreement.

Armed with these results, and mindful of various comments made by parents, the boards are back to their original place. On admission, the parents are asked whether they object to their child's full name being placed on it. This appears to work well, with satisfaction among clinicians, parents, and managers—an unusual state of affairs!

Dr I Rodd

Paediatric SpR, Wessex region, UK; ian.rodd@weht.swest.nhs.uk

Dr J Kohler

Southampton General Hospital, UK

doi: 10.1136/adc.2003.044933

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CORRECTION

The authors of the paper entitled Epidemiology of paediatric renal stone disease in the UK (Coward *et al*, *Arch Dis Child* 2003;**88**:962–965) would like to acknowledge the source of their data in Table 1. This table was adapted from data published in the paper by So *et al* (*Pediatr Nephrol* 2001;**16**:133–139).