LETTERS TO THE EDITOR

Rapid responses
If you have a burning desire to respond to a paper published in ADC or F&N, 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 eLetters” on our homepage.

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

Infantile colic and chiropractic spinal manipulation

EDITOR,—We congratulate Olafsdottir et al on their article. The sum of the evidence on spinal manipulative therapy (SMT) in the treatment of infantile colic now is that there are 3 randomised controlled trials (RCTs) on the subject.

Two RCTs demonstrated a significant positive effect of SMT;1,2 and 1 RCT was unable to demonstrate any treatment effect.1 The reasons for this discrepancy are not known, but Olafsdottir et al suggest that their finding of no effect of SMT may be due to the blinding of the infants’ mothers. Another equally likely explanation could be that we are witnessing a dose response phenomenon.

In their trial, Olafsdottir et al used a treatment protocol of a maximum of 3 sessions of SMT, whereas the other 2 RCTs, which found a positive treatment effect, used a treatment protocol relying more on the treating chiropractor’s clinical judgement. This more pragmatic approach resulted in 64% of the infants in one RCT receiving 4 or more sessions of SMT (with a maximum of 7),3 and the majority of infants in the other RCT receiving up to 6 sessions.4 We believe that this dose response problem should be addressed in future trials of SMT for infantile colic.

G N GRUNNET-NILSSON
University of Southern Denmark
n.nilsson@umuc.m Odense

JESPER WIEBERG
Private practice, Copenhagen,
Denmark


Diluted treatment effects?

EDITOR,—If my reading of this colic study is correct, it appears that both groups received standard counselling and recommendations for the care of a colicky child. My question to the authors is, if their posttreatment recommendations are effective in the reduction of colic, does this not raise the possibility that any treatment effect in the SMT group could have been diluted by the introduction of a second active treatment (standard recommendations) in the control group? Put another way, was the placebo intervention an inert intervention or was it a second active intervention?

G W KUKURIN
Pittsburgh, PA, USA
doc@ad-compmed.com

Commentaries—read with caution!

EDITOR,—The commentary by Lenney correctly points out that clinicians are often slow to apply good research evidence to clinical practice. However, the choice of once daily intravenous gentamicin to illustrate this point is unfortunate. Extended interval aminoglycoside dosing is widely used in paediatric and neonatal practice for the treatment of serious gram negative infections, the treatment of newborn infants with sepsis, and the treatment of chronic Pseudomonas aeruginosa infection in patients with cystic fibrosis. However, the implementation of extended interval dosing has not been based on the results of appropriately designed trials in children and neonates.

The largest meta-analysis of single versus multiple daily dosing of aminoglycosides for the treatment of gram negative sepsis in children and neonates was published in 2000.4 Many of the included studies in this meta-analysis were placebo controlled, and the dose was varied over the course of the trial. The authors of this meta-analysis concluded that once daily dosing is not inferior to multiple daily dosing for the treatment of gram negative infections, the treatment of newborn infants with sepsis, and the treatment of chronic Pseudomonas aeruginosa infection in patients with cystic fibrosis. However, the authors of this meta-analysis did not report the number of these problems that we see in hospital paediatric practice.

We believe that this dose response problem should not be extrapolated to children. Instead, quality of evidence should be the gold standard criterion. We would not recommend the routine use of extended interval dosing in CF due to the high frequency of nephrotoxicity and ototoxicity that has been associated with this treatment approach. We would not recommend the routine use of extended interval dosing for the treatment of gram negative sepsis in children and neonates due to the high frequency of nephrotoxicity that has been associated with this treatment approach.

N CHERRY
Nottingham City Hospital NHS Trust
Nottingham, Nottinghamshire

K TAN
Academic Division of Child Health, University of Nottingham, Nottingham, UK
Kelvin.Tan@nottingham.ac.uk

A SMYTH
Nottingham City Hospital NHS Trust, Nottingham, UK

K TAN
Academic Division of Child Health, University of Nottingham, Nottingham, UK
Kelvin.Tan@nottingham.ac.uk

D CROSSLAND
K POTTER DE LA MORANDIERE
Department of Paediatrics, Sunderland Royal Hospital,
Kayll Road, Sunderland SR4 7TD UK
davidcld@hotmail.com


Alcopops are not responsible for acute paediatric attendances with alcohol intoxication

EDITOR,—We were interested to read Dr Robson’s leading article regarding alcohol misuse and the reference to acute alcohol admissions to Alder Hey in Liverpool, UK.1,2 We too are concerned by the increasing number of these problems that we see in hospital paediatric practice.

We carried out a retrospective case note review of all the children seen in the Paediatric Emergency department in Sunderland between November 1999 and October 2000. One hundred and twenty children (57 girls and 43 boys) attended for 106 admissions with acute alcohol intoxication (2 children attended twice and 2 three times). The notes of 97 attendances were available for review. Most children were aged 13 to 15 (77%), range 10–16 years. As might be expected, the majority presented during the weekend (66%) and in the evening or at night (84% between 19:00 and 01:00). Half had been drinking with friends in a public place, although precise details were not recorded in many cases. Sixty one children (63%) were brought in by emergency ambulance and 48 (49%) were admitted. Thirty six (31%) were documented to have been drinking vodka, 21 cider (22%), 12 (12%) beer or lager, 11 (11%) other alcohol drinks, 5 kids wine and 8 (8%) a combination of these. The type of alcohol was not recorded in 7 (7%) cases.

In no cases were alcopops thought to be the beverage responsible for the acute attendance, and the beverages consumed are comparable with Alder Hey figures from 1996.3 Alcopops and designer drinks appeal to young people, particularly 14–16 year olds, and there has been criticism that marketing may be aimed at this age group. We would argue that the consumption of alcopops has been associated with drinking in less controlled environments, heavier drinking, and greater self reported drunkenness.4 However, our data do not suggest that they are a problem in relation to acute intoxication presenting to Accident and Emergency. We support the statement that children will mimic adults in their use and misuse of alcohol, and consider that it is society’s changing attitude to alcohol and not the type of alcohol available that is of concern.

A SMYTH
Nottingham City Hospital NHS Trust, Nottingham, UK
Kelvin.Tan@nottingham.ac.uk

Other implications of disposable nappies

EDITOR,—Partsch, Aukamp, and Sippell propose that increased testicular temperature in early childhood might affect later spermatozoa. They suggest that disposable nappies could contribute to this and demonstrate a significant difference between the scrotal temperature recorded in infants using disposable nappies and washable cotton nappies. They mention in their introductory paragraph that other environmental factors may be important in the deterioration seen in male reproductive health over recent years, but do not relate any of these factors to disposable nappies.1

There are many concerns about the use of disposable nappies in addition to increasing scrotal temperature that may impact on fertility and general health. The disposable nappy consists of a plastic outer layer, a layer of superabsorbent chemicals and inner liner. Nappies are not subject to government controls or independent testing and disposable nappy manufacturers do not need to disclose the contents.2

Recently, concern has been raised about the presence of Tributyl Tin (TBT) in disposable nappies. Greenpeace and Women's Environmental Network have commissioned research which showed that there were significant levels of TBT in many brands of disposable nappy, including those on sale in the UK. TBT may be in contact with up to 3.6 times the WHO's estimated tolerable daily intake. TBT is an environmental pollutant which is used in anti-fouling ship paint. It is known to disrupt the endocrine and immune system. TBT is also used in anti-fouling ship paint. It is known to disrupt the endocrine and immune system. It is also known to increase the risk of developing cancer.3

As paediatricians committed to the health of children, we should be aware of the issues raised by the use of disposable nappies, the alternatives that exist, and sources of information and support for parents who are concerned about ensuring a safe and sustainable future for their children.

C HEAL
Consultant Paediatrician,
Royal Albert Edward Infirmary, Wigan Lane,
Wigan WIG 2NN, UK

REFERENCES
4 Greenpeace. Greenpeace calls on parents to return contaminated nappies to producers: new tests show that TBT-free nappies are a rarity. Press Release 19th May 2000 www.greenpeace.org.
8 Greenpeace. Greenpeace calls on parents to return contaminated nappies to producers: new tests show that TBT-free nappies are a rarity. Press Release 19th May 2000 www.greenpeace.org.

Dexamethasone, survival, and neurological impairment

EDITOR,—Professor Pharoah questions whether the increased rate of cerebral palsy among newborn infants who were randomly allocated early postnatal dexamethasone therapy in the trial by Shinwell et al might be because dexamethasone increased survival of infants who were impaired before birth, and not because dexamethasone caused cerebral impairment.1

However, two recent systematic reviews of randomised trials of postnatal dexamethasone therapy in infants at risk of chronic lung disease do not support this hypothesis. Half-day and Ehrenkraut found no difference in survival in trials of dexamethasone given within 96 hours of birth.2 Doyle and Davis found no difference in survival, overall or in any subgroups, in trials of dexamethasone therapy at any time after birth.3 Both reviews concluded that postnatal dexamethasone may cause neurological dysfunction and called for further trials with appropriate follow up.

Professor Doyle is currently co-ordinating such a trial in infants under 1000 g or less than 29 weeks who are ventilated after 7 days from birth (the DART study, Dexamethasone in tiny infants—a Randomised Trial). Those interested in participating in this important study are very welcome to contact him at l.doyle@obgyn-rwh.unimelb.edu.au.

W TARNOW-MORDI
Westmead Hospital and The Children's Hospital at Westmead,
University of Sydney, Australia
ltd@unimelb.edu.au

NOTICE

Notice of duplicate publication


The same data, resulting from a single pilot study were reported in the two above papers. The authors have apologised, explaining that they had not intended to flout accepted academic standards, rather that they wished to bring their findings to the attention of two separate readerships—namely paediatricians and nurses. However, we would not wish to compilers of systematic reviews to include these data twice and therefore we give notice of duplicate publication and withdraw the article published in Archives of Disease in Childhood.