Intravenous atropine treatment in infantile hypertrophic pyloric stenosis

Hypertrophic pyloric stenosis of infancy is a disorder of early infancy with typical clinical features and well-established radiological appearances of the pyloric canal. Many studies with surgical and medical treatment have been reported over the past fifty years. Pylorotomy has tended to become the favoured method of treatment as with expert paediatric, surgical, anaesthetic, and nursing services and specialised accommodation for infants, the outcome is good with low mortality, short stay in hospital and few complications. However, a variety of studies of medical treatment with anticholinergic drugs and successful outcomes in some large series of cases have also been reported from Sweden, United States of America and the United Kingdom.

Since 1996 this group of workers from Osaka, Japan, has revived an interest in medical treatment with reports of a new regime using methyl atropine nitrate intravenously. To achieve satisfactory short term outcomes considerable variation in drug dosage and modified feeding regimes were necessary which involved much medical supervision and careful monitoring for toxic effects of the drug, which were minimal. The treatment was successful in the relatively small number of infants in the trial (19) with two infants being referred for pylorotomy, no mortality and no serious complications. An interesting part of this paper is the long term clinical follow up of the successfully treated infants over two years and ultrasonography of the pyloric canal which demonstrated the changes in muscle thickness and length of the canal. The disadvantages of the treatment mentioned by the authors are length of stay in hospital and the necessity to continue atropine medication orally after discharge home.

Comparing the use of this anticholinergic drug intravenously with oral treatment using methyl scopolamine nitrate and similar restricted feeding regime, oral methyl scopolamine nitrate suppressed vomiting more quickly and reliably, was also available for subcutaneous injection if vomiting recurred as size of feeds was increased, and no toxic effects were seen in any dosage used. It would be interesting if these workers would be prepared to try the use of methyl scopolamine nitrate intravenously as pharmacologically this compound was reported to have a spasmylic effect on gut two to three times greater than methyl atropine nitrate with lesser central nervous effects.

This paper serves to emphasise once more that these infants should always be treated in paediatric centres where there is a high level of experienced paediatric care and nurses trained for neonatal special care.

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Author’s reply

We appreciate the interest shown by Dr Beryl Corner with regard to our article. Unfortunately, intravenous atropine therapy is not widely accepted in European countries or the United States; it is however now becoming popular in Japan. We are truly honoured to receive the comments of Dr Corner, who is a pioneering neonatologist and reported medical treatment with methyl scopolamine nitrate for infantile hypertrophic pyloric stenosis (IHPS) in 1955. She pointed out that methyl scopolamine might be better than atropine sulfate in terms of effectiveness and side effects. One of the reasons why atropine was used in our study is that methyl scopolamine is not available in our country. Scopolamine butylbromide is an available quaternary ammonium derivative of scopolamine and lacks toxic side effects. However, this agent tastes bitter and is difficult to give orally to infants. Therefore, this agent is only given intravenously in infants with IHPS.

We do not know if it is worthwhile to attempt combination therapy with intravenous scopolamine butylbromide and oral atropine rather than the intravenous and oral atropine therapy. Secondly, we already knew that an intravenous atropine injection of 0.01 mg/kg was effective enough to abolish transiently the phasic and tonic pyloric contractions characteristics of IHPS. We used an intravenous atropine injection of 0.01 mg/kg in our study to confirm that those pyloric contractions were the cause of disturbed transpyloric flow in this condition by seeing that their inhibition with the dose of atropine ameliorated symptoms.

We agree with Dr Corner’s last comment, but believe that intravenous atropine therapy is possible not only in high level paediatric centres, but also in general hospitals where infusion therapy with intravenous atropine injections can be done safely in small infants. Clinical trials are now ongoing to establish more efficient treatment strategy for IHPS with medical and surgical therapy in our country.

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References


Hypothermia in a child secondary to ibuprofen

A 7 year old girl was admitted with right lower lobe pneumonia. On admission her temperature was 39.7°C. After five hours she received ibuprofen (6 mg/kg). Subsequent to this single dose her temperature decreased to 33.5°C (core temperature 34.9°C) over four hours. On examination her pulse was 90/min, blood pressure 90/50 mm Hg, SaO2 96% in air, and respiratory rate 20/min. Respiratory examination was consistent with signs of right lower lobe consolidation. The rest of the examination, including the central nervous system, was unremarkable.

Results of investigations included: Hb 125 g/l; white blood cell count 10.7 × 109/l platelet count 81 × 109/l; C reactive protein 180 mg/l; blood glucose 4.6 mmol/l. Electrolytes and all other biochemical investigations were normal. Thyroid and cortisol assays were normal. Results of all tests to determine possible bacterial or viral aetiology were all negative (blood and urine culture, viral serology, and tests for mycoplasma). Magnetic resonance imaging (MRI) of the brain was normal.

The hypothermia was so marked that we had to use a hot air spacer blanket to raise her temperature. Despite all the efforts she remained persistently hypothermic for four days (see fig 1).
A single dose of hydrocortisone and an albumin infusion were given initially. She was subsequently treated with warmed intravenous fluids for three days and antibiotics for 10 days. She recovered completely and continues to enjoy good health.

1 Year old hypothermia is extremely rare in children over 5 years of age. Results of investigations excluded infective and endocrine causes. A normal MRI brain scan showed there was no lesion of the hypothalamus or corpos callosum. Ibuprofen is commonly prescribed for a raised temperature and is well tolerated in children. Side effects are not common, even in oedematous patients, whereas we postulate that ibuprofen was responsible for hypothermia in this case. We are not aware of any published evidence documenting hypothermia after a single therapeutic dose of ibuprofen, but it has been recorded in a few cases of accidental and deliberate overdose. Although patients may sometimes receive ibuprofen in toxic quantities, hypothermia is not a consistent feature.1 Hypothermia in overdose is attributed to central nervous system depression.4

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References

Vagal overactivity: a risk factor of sudden infant death syndrome?

Since early 1990, the incidence of sudden infant death syndrome (SIDS) has dropped sharply because of public health campaigns decrying the dangers of the prone sleep position. The other known risk factors, such as preterm birth and young maternal age, are less susceptible to prevention campaigns.

Disordered autonomic function, including cardiorespiratory control, has been suggested to be involved in SIDS.1 Vagal overactivity (VO), characterised by breath holding spells and recurrent epiglottic hypoglycaemia, has been described as a manifestation of autonomic dysfunction.2 To investigate a possible relation between VO and SIDS, we investigated 65 children presenting documented VO; for example, clinical characteristics and a positive test for eyeball compression and/or electrocardiographic monitoring. Participants were interviewed about their family history, especially with respect to the occurrence of SIDS among their other children.

Among their siblings, five of 126 had died of SIDS. All five children were full term infants. The average maternal age, birth weight, and age at death were respectively 27.4 (3.5) years, 3.3 (0.3) kg, and 3.5 (1.1) months. The rates of SIDS in siblings of children with VO were compared to the age and sex matched general population using the standardised incidence ratio (SIR), which is the ratio of the observed number to the expected number of cases of SIDS calculated from the French national rates. The expected number of SIDS was 0.17 and hence the SIR was 29.4 (95% CI 9.5 to 68.6; p < 0.00001). Our result showed an overall significant excess of SIDS among siblings of children with VO, but that recruited children had not come to the centre because of a family history of SIDS. Since children with a positive family history of SIDS could be followed up more regularly than others, we estimated the SIR separately among siblings of children recruited during their follow up and those of children recruited during their first visit, and verified that there was no significant difference in SIR between these cases. Despite the marked decline in SIDS, it is still the leading cause of postneonatal mortality. Better knowledge of other risk factors may allow identification of populations at high risk and the decrease in infant mortality from SIDS through the implementation of appropriate prevention measures. Our findings suggest that VO may be involved in SIDS and that children with VO or a family history of VO may be a population at potential high risk of SIDS.

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Perforated duodenal ulcer disclosing medium chain acyl-CoA dehydrogenase deficiency

Medium chain acyl-CoA dehydrogenase deficiency (MCADD; McKusiek 201430) typically presents in the first two years of life with recurrent episodes of hypoketotic hypoglycaemia, lethargy, coma, or sudden infant death. The trigger may be fasting, intermittent infections, anaesthesia, or surgery. Incidence in the UK is estimated at 0.45–1/10 000 live births.1 We describe the case of a child who presented with marked encephalopathy unexplained by perforated duodenal ulcer, which led to the diagnosis of MCADD.

A 1 year old girl presented with a three week history of corzyl symptoms and three day history of frequent coffee ground vomiting. She was shocked, and had hepatomegaly and decreased conscious level. Blood glucose was 3.9 mmol/l (reference interval 3.3–5.3), plasma sodium 129 mmol/l (135–147), potassium 5.2 mmol/l (3.5–5.0), urea 17.8 mmol/l (3.3–6.6), creatinine 36 mmol/l (30–74), bicarbonate 15 mmol/l (21–28), albumin 36 mmol/l (42–30), lactic acid 3.5 mmol/l (2.2–3.8), and decreased conscious level. Blood glucose was 12 mmol/l on admission. Her heart rate was 100 bpm, respiratory rate 20/min, and blood pressure 80/50 mmHg. She was somnolent and presented with right upper quadrant pain.

PostScript
Thus, any child who has unexplained en- 
ophthalmopathy, regardless of its cause and 
clinical setting, should be screened for 
MCADD.

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Glucose metabolism in sleep disordered breathing

An association between sleep disordered breathing (SDB) and impaired glucose toler-
ance has been reported in adults.1 Although SDB has been reported in diabetic children, no data are available on glucose metabolism in children with SDB. We used glycated haemoglobin (HbA1c) for the preliminary assessment of glucose metabolism in paediat-
ric SDB patients.

HbA1c was measured in 12 children aged 26–116 months (mean 63) with suspected SDB owing to adenotonsillar hypertrophy. Informed consent was obtained from the guardians of each patient, and consent was obtained from the child if older than 5 years of age. Overnight polysomnographic studies obtained from the child if older than 5 years of age. The severity of respiratory disturbances during sleep in diabetic children has been known to correlate with the degree of desaturation in non-obese SDB patients; HbA1c levels should, however, be monitored after treatment. SDB and glucose metabolism are hypothesised to be closely associated in children as well as adults.

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Short versus standard duration antibiotic treatment for UTIs: a comparison of two meta-analyses

Having recently published a meta-analysis on the same clinical question,5 it was with great interest that we read Michael et al’s systematic review of short versus standard duration anti-
biotics for urinary tract infections (UTIs) in children.6 Given the publication (in close succ-
tion) of two meta-analyses on the same question with (on the surface) strikingly dif-
ferent results, we thought a comment was in order.

First, we applaud the authors on their methodologically sound review. The litera-
ture search was explicitly described and exhaustive. In fact, the authors identified a few studies that we had missed.7 The study outcomes for meta-analysis (frequency of positive urine cultures at 0–7 days after treat-
ment and at 10 days to 15 months after treatment, and development of resistant organ-
isms and recurrent UTI) were relevant and clearly defined.

The authors provided appropriate and im-
portant meta-analysis measures including summary relative risks (RRs) and a quasi-
NNT calculation with varying risk of treat-
ment failure in the standard treatment group and confidence intervals corresponding to “best” and “worst” case scenarios.

For their primary outcome, frequency of positive urine cultures 0–7 days after treat-
ment, the authors found no significant difference between short (3 days) and stan-
dard (7–14 days) treatment (RR 1.06; 95% CI 0.64 to 1.76). This is in contrast to our find-
ing of a 94% increased pooled risk of treatment failure with short course treat-
ment (<3 days) compared to standard treat-
ment (7–14 days) (RR 1.94, 95% CI 1.19 to 3.15; NNT=15, 95% CI 100 to 7). Why the discrepancy? We postulate a few possible explanations and conclude that the two meta-analyses, on closer inspection, actually have very similar results.

Our omission of certain studies identified by Michael and colleagues may have biased our results. However, of the three studies7 that we missed and that they included in their analysis of treatment failure at 0–7 days after completion of treatment, two favoured standard duration treatment, which would have supported our pooled RR result. Another possible explanation for the difference was the use of different definitions of treatment failure. For our definition of treat-
ment failure we pooled persistent infection failure to eradicate the organism (0–7 days after initiation of treatment) and relapse (recurrence of symptoms and reinfection within 2 weeks of cessation of treatment affected initial bacteriologic cure), whereas Michael et al used frequency of positive cultures 0–7 days after cessation of treatment as their primary outcome measure of treatment failure. If infections reappeared after 7 days after cessation of treatment occurred more often in recipients of short course treatment, then Michael et al’s definition of treatment failure could have failed to capture the thera-
peutic advantage of standard duration treat-
ment.

However, the most likely explanation for the divergent results was the different ways in which the study question was framed and the resulting differences in studies included in the meta-analyses. We compared <3 days of treatment to 7–14 days of treatment, whereas Michael et al compared 2–4 days of treatment to 7–14 days of treatment and excluded 13 studies comparing single-dose or single-day treatment to standard duration treatment.7

The reasons for this exclusion are unclear, although we presume that they felt single-dose or single-day treatment was not a fair comparison with 7–14 day treat-
ment. However, a number of randomised controlled trials (RCTs) made this compari-
sion, suggesting that clinicians are, in fact, interested in the potential efficacy (and signs) of increased cure rates with short-course single-dose or single-day treatment. Inclu-
sion of these studies in our analysis strongly in-
fluenced the pooled risk of treatment failure with short-course treatment. When we excluded these studies in a sub-group analysis of 3-day versus long course (7–14 day) treatment, the risk of treatment failure fell to 1.36 (95% CI 0.68 to 2.72) (NNT=50; 95% CI 33 – 13).

Thus, our meta-analysis demonstrates clearly that single dose or single day antibiotic treatment is not as effective as long-course treatment for UTIs in children. The two meta-
analyses together suggest that (1) “longer” short-course therapies may be as effective as 7–14 days of antibiotics and
(2) there is probably a duration of treatment threshold for “short-course” antibiotic treatment, above which longer duration of treatment confers no therapeutic advantage.

Michael and colleagues suggest that as little as 2 days of treatment may be sufficient. However, only one of the trials in their meta-analysis studied 2-day treatment and that one did not use a short-course treatment with a RR of UTI 0.7–7 days after completing short course treatment of 2.17 (95% CI 0.48 to 9.76). The duration of treatment threshold may be 3 days, but the point estimate of relative risk of treatment failure with 3 day treatment in our meta-analysis suggests otherwise. If the duration of short-course treatment for which there is no difference in efficacy compared with standard treatment is actually greater than 3 days, then the added convenience and cost-savings of “short-course” treatment become marginal. In the absence of appropriately powered RCTs (or meta-analyses) examining outcomes (treatment failure, reinfection, emergence of resistant organisms and cost) with “longer” short course treatment regimens (3, 4, and 5 days), we think that clinicians should continue to treat UTIs in children with at least 7 days of antibiotics.

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Table 1 Results of three systematic reviews of randomised controlled trials comparing short duration with standard duration of antibiotic treatment for lower tract urinary tract infection.

<table>
<thead>
<tr>
<th>Systematic review</th>
<th>Comparison of duration of therapy</th>
<th>Number of data sets</th>
<th>Risk for persistent bacteriuria</th>
</tr>
</thead>
<tbody>
<tr>
<td>Tran et al, 2001†</td>
<td>1–4 days v &gt;5 days</td>
<td>13</td>
<td>RR: 4.26 (95% CI 0.95, 9.48)</td>
</tr>
<tr>
<td>Keren &amp; Chan, 2002‡</td>
<td>3 days v 7–14 days</td>
<td>5</td>
<td>RR: 1.36 (95% CI 0.68, 2.72)</td>
</tr>
<tr>
<td>Michael et al, 2002</td>
<td>2–4 days v &gt;7–14 days</td>
<td>8</td>
<td>RR: 1.06 (95% CI 0.64, 1.76)</td>
</tr>
</tbody>
</table>

†RR, risk difference; ‡CI, confidence intervals; †RR, relative risk

Because there is no significant difference between short duration and standard duration antibiotic treatment for the number of children with persistent UTI after treatment, it is not possible to calculate a number needed to treat to prevent one episode of persistent bacteriuria.

From our systematic review, we are not able to determine whether there is an “optimum duration of treatment threshold” as postulated by Keren and Chan. Only one study included in the meta-analysis, examining the effects of short duration and standard duration treatment in clearing bacteriuria, compared 2 days of treatment with 10 days’ treatment. In their letter above, Keren and Chan argue that this study favours standard duration treatment. However, there was no significant difference between treatments in the number of children with persistent bacteriuria at the end of treatment (RR 2.17; 95% CI 0.48 to 9.76) although the wide confidence intervals do not exclude the possibility that short duration treatment could be more or less effective than standard duration treatment.

No significant differences in the number of children with persistent UTI after treatment between short duration and standard duration antibiotic treatment have been found in three systematic reviews of randomised controlled trials despite different study inclusion criteria and definitions of persistent infection. As described in our review, the wide confidence intervals around the summary estimates indicate residual imprecision in the results. However, this statistical imprecision is of doubtful significance for most children, who are at a low risk (1–3%) of persistent UTI at the end of treatment following their first lower tract UTI. Therefore, we do not support Keren and Chan’s conclusion that clinicians should continue to use longer treatment with standard duration treatment. Instead, we believe that short duration treatment may be used to treat children with lower tract UTI.

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References
2 Keren R, Chan E. A meta-analysis of antibiotics at the end of treatment following their first lower tract UTI.

Authors’ reply
In response to Keren and Chan’s thoughtful letter regarding our recent systematic review, we need to emphasise that the study question we addressed was different from that addressed by Keren and Chan in their own systematic review of randomised controlled trials comparing short with standard duration treatment in the treatment of children with urinary tract infection (UTI). The aim of our study was to determine the relative efficacies of short (2–4 days) and standard duration (7–14 days) treatment. However, there was no significant difference between treatments in the number of children with persistent bacteriuria at the end of treatment (RR 2.17; 95% CI 0.48 to 9.76) although the wide confidence intervals do not exclude the possibility that short duration treatment could be more or less effective than standard duration treatment.

No significant differences in the number of children with persistent UTI after treatment between short duration and standard duration antibiotic treatment have been found in three systematic reviews of randomised controlled trials despite different study inclusion criteria and definitions of persistent infection. As described in our review, the wide confidence intervals around the summary estimates indicate residual imprecision in the results. However, this statistical imprecision is of doubtful significance for most children, who are at a low risk (1–3%) of persistent UTI at the end of treatment following their first lower tract UTI. Therefore, we do not support Keren and Chan’s conclusion that clinicians should continue to use longer treatment with standard duration treatment. Instead, we believe that short duration treatment may be used to treat children with lower tract UTI.
Is life long follow up for patients with Kawasaki disease indicated?

Brogan et al. recommended life long follow up for patients with Kawasaki disease, including those who do not have coronary artery involvement. The reason they quoted was to document the blood pressure and provide general advice regarding other risk factors. The American Heart Association recommends echocardiographic (ECG) evaluation of the coronary arteries at presentation and follow up ECG at 6–8 weeks and 6–12 months after coronary arteries at presentation and follow up echocardiographic (ECG) evaluation of the coronary arteries at presentation and follow up ECG at 6–8 weeks and 6–12 months after the onset of symptoms for those who did not have or just have transient coronary artery involvement. They do not recommend follow up after first year unless cardiac disease is suspected.

Tuohy et al. demonstrated, in their multi-institutional review of 536 patients, that no patient with a normal follow up ECG, performed within 2 months following disease onset, subsequently developed echocardiographic coronary artery abnormalities. Even those patients with initial echocardiographic abnormalities that became normal at 1–2 months remained normal thereafter. Scott and colleagues showed that no patient with a normal ECG at 2 weeks to 2 months after the onset of symptoms had subsequent ECGs that revealed coronary artery abnormalities, and questioned the value of 6–12 month ECG in the same group.

Brogan et al. did not make any comments about the adverse effects of life long follow up, such as anxiety and inappropriate restriction of activities. Finally, there were no comments about the cost and resources for providing life long follow up. The authors did not specify whether paediatric cardiologists, general paediatricians, or general practitioners would follow up; all of them already have increasing demands of workload.

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References

Management of childhood osteoporosis

I read with interest this recent review article that summarises current knowledge about this subject. I have a number of comments that are pertinent to the discussion. As the authors allude to, there is currently a lack of good evidence on which we can base preventive management. Although alendronate and vitamin D supplements are routinely used by some paediatric rheumatologists, there appears to be only one short term study suggesting this may be beneficial for bone density. The hypothesis that growth hormone therapy in relation to growth hormone therapy are methodologically flawed because neither have accounted for the change in apparent bone density, which will occur in any child who grows better for any reason when assessed by modalties such as dual energy x ray absorptiometry.

As illustrated by another article in the August 2002 edition of Archives, there is a lack of good evidence on which to base much paediatric management and it is imperative that further research, especially randomised controlled trials, is undertaken in the area of prophylaxis against osteoporosis in children with chronic disease on steroids. Paediatric endocrinologists will be familiar with the flurry of small uncontrolled studies undertaken in numerous groups of children with chronic disease; however, the management and prevention of osteoporosis requires specialist expertise just as the management of growth retardation currently does. It is important that in each tertiary centre such a specialist service is provided by one department that has expertise in the interpretation of bone density scans in children and the management of children with osteoporosis. Such individuals may not only be paediatric endocrinologists but may also be a paediatric rheumatologist, a general paediatrician with a special interest in bone disease, or a metabolical bone disease subspecialist. It is only in this way that we can learn more about the management of this condition and avoid children being treated inappropriately.

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Newborn screening for Duchenne muscular dystrophy

Elliman, Dezaute, and Bedford, in their recent leading article on newborn and childhood screening, include reference to newborn screening for Duchenne muscular dystrophy (DMD). They argue that the main value of such a screening programme is to warn parents that future sons may be affected, and support this statement with reference to Jarvinen et al. This paper does not report a newborn screening study but the results of a retrospective study of 23 females in Finland carrier tested for DMD during childhood. However, a newborn screening programme for DMD has been running in Wales since 1990 (1990–8 as a research evaluation and from 1998 health authority funded). During the research period interim evidence was published. More recently the full results of our prospective study have been published. Our evaluation has demonstrated that a newborn screening programme for DMD can be acceptable to both parents and health professionals, providing that a rigorous service delivery protocol is in place and the programme is supported by an effective infrastructure, in particular by paediatric and genetic services.

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References

Sanctions were imposed on the people of Iraq in 1990. Iraqi people are still suffering, especially children. Infant mortality (IM) has increased more than five times. Previously it had decreased from 139 in 1960 to 20 in 1989, which was comparable to developed countries. In 1992 it went up to 111.¹ In 1999, a decade later, IM was still high at 104.² The Gulf War and trade sanctions caused a three-fold increase in mortality among Iraqi children under 3 years of age. It has been estimated that more than 46,900 children died between January and August 1991.³

The study of the UN Food and Agricultural Organisation, published in a letter to the BMJ in 1995, concluded that deaths of more than 50,000 could be attributed to UN sanctions. It also stated that the death rate among children under 5 years in Baghdad had increased fivefold since the war ended in 1991.⁴ Data for 1994–99 showed that mortality for children under 5 years was 131 per 1000 live births, compared with 56 for 1984–9, and a threefold increase in mortality among children 0–4 years of age. In 1992 it went up to 111.²

A report in 1996 showed that one third of hospital beds were closed. More than half of all diagnostic and therapeutic equipment was not working due to lack of spare parts and maintenance. All public health centres experienced serious problems with lighting, cleaning, water supply, and sewage. The population had been burdened by a rapid rise in serious infections, nutritional deficiencies among children and pregnant women, and other treatable conditions for which neither drugs or medical supplies were available.⁵ Paediatricians have been isolated by the intellectual embargo of the international medical community. Physicians who wish to attend international conferences face travel restrictions, like denial of visas to European countries or the USA. In 1990, the delivery of medical supplies was also stopped. Physicians who wish to attend international conferences face travel restrictions, like denial of visas to European countries or the USA. In 1990, the delivery of medical supplies was also stopped. In 1999, the intellectual embargo served to undermine the care of patients, and denies Iraqi doctors the right to share scientific advancement and its benefits.⁶

In school children aged 6–8 years the prevalence of wasting ranged from 1% in the upper class to 6.7% in rural areas. Similar differences were found for stunting and underweight.⁷ In a 1994 survey 1.6% of children under 5 years were reported to have night blindness, indicating vitamin A deficiency. A survey of school children in the north in 1994 showed a 30–50% prevalence of goitre, and evidence of iodine deficiency disease elsewhere throughout the country. Rickerts and co-workers reported a 2% prevalence of goitre in hospitals at a rate of 3–5 cases per week.⁸

Diarrhoeal diseases and mortality due to dehydration were well under control prior to the Gulf War; there was a threefold increase from May 1990 to May 1991.⁹ Other water born infections increased from 1990 to 1999, for example typhoid by 60% and cholera almost fivefold.¹⁰ A measles epidemic occurred in 1998.¹¹ There have been alarming rises in cases of malaria and leptospirosis. Other infections like tetanus, poliomyelitis, diphtheria, and pertussis all showed an increase after the Gulf War.¹²

The National Immunization Programme which had begun in 1985 came to a complete halt between January and April 1991.¹³ The percentage of fully immunised one year old children fell from 94 for tuberculosis, 83 for diphtheria, tetanus, and pertussis, 83 for polio, and 82 for measles to 79, 63, 64, and 68 respectively.¹⁴

A child psychology study (1991) revealed a level of psychological stress and pathological behaviour that was the highest the authors had seen in 10 years of conflict research. It revealed a highly disturbed population of children. Fear and anxiety were associated with memories of crisis. Seventy five per cent felt sad and unhappy, and four out of five expressed fear of losing their family by death or separation.¹⁵

There was a threefold increase in leukaemia in the southern provinces, sites of the Gulf War battlefield. A WHO investigation in 1995 suggested a possible link to products—now banned under UN sanctions—that had been manufactured in a factory which were derived from depleted uranium used in piercing artillery shells. There were staggering deficiencies in cancer treatment facilities because of UN sanctions which were intended to exclude food, drugs and medicines.²⁰

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Paediatricians have been isolated by the intellectual embargo of the international medical community. Physicians who wish to attend international conferences face travel restrictions, like denial of visas to European countries or the USA. In 1990, the delivery of medical supplies was also stopped. In 1999, the intellectual embargo served to undermine the care of patients, and denies Iraqi doctors the right to share scientific advancement and its benefits.⁶

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References

Differential diagnosis of periodic fevers

We just read the short report of Galanakis et al.¹ We have been involving in periodic fevers management for many years. At present, PAFPA is an unclear entity, classified among non-hereditary fevers. It is an unclear nosological entity. Pharyngitis, cervical lymphadenopathies and oral aphthae are exclusive findings in PAFPA. Among periodic fevers, cerebral hyperleukocytosis and episodic fever can occur in patients with HyperIg D and periodic syndrome (HIDS), and less in Familial Mediterranean Fever (FMF). Oral aphthae (as minor sign), cervical adenopathies, and isolated fever can be in children affected by FMF. Pharyngitis, oral aphthae, cervical adenopathies, and recurrent fever also characterise Crohni’s disease (CD). Lastly, oral aphthae and recurrent febrile attacks characterise the onset of Behcet’s disease (BD) in children. The efficacy of steroids does not confirm the diagnosis of PAFPA; BD and CD are responsive to steroids, too. The lack of familial involvement is not a criteria to exclude an inherited disorder, as FMF and HIDS are recessive and BD and CD are multifactorial diseases. Furthermore, the initial clinical picture of these disorders can be atypical and incomplete and can change during the clinical course.

So, considering the provenance of Galanakis’ series (Greece), we not be surprised if some cases had BD or FMF, that will be recognised in the future. Nowadays, with increased diagnostic sensitivity and multi-ethnic societies, periodic fevers are being recognised outside their traditional area of incidence. Close follow up is essential in further years, in these patients. A possible genetic screening for gene causing FMF, HIDS, or immunological assay for HLA B51 could also be useful.

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Reference

Mechanisms of pulmonary hypertension in Bordetella pertussis

Casano et al describe a case of refractory pulmonary hypertension with severe Bordetella pertussis infection.¹ Their description of the literature is incomplete. We described four cases of fatal pulmonary hypertension (PHT) in a series of 13 critically ill infants with B. pertussis.² The cases that developed PHT all presented with severe hyperleukocytosis (WCC>100 x 10⁹/L) which was unresponsive to all currently available modalities including extra-corporeal membrane oxygenation. Hyperleukocytosis was an independent predictor of death when corrected for presentation severity. Close follow up is essential in further years, since histological evidence² was such that extreme leukocytosis prediposes to the formation of lymphocyte aggregates in the pulmonary vasculature and increased pulmonary vascular resistance by obstruction rather than
hypoxic vasoconstriction. Therefore Dr Casano’s recommendation for the early use of pulmonary vasodilators is unlikely to be sufficient in this context. We are assessing the impact of strategies aimed at reducing lymphocyte numbers and adhesion in addition to standard treatments for pulmonary hypertension.

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**References**

**Authors’ reply**
As Peters comments in his letter, we know that hyperleukocytosis has been postulated as a factor for pulmonary hypertension in Pertussis infection, but necessary brevity did not make it possible to report. Nevertheless, our patient never reached these values of leucocytosis; it’s possible, as in many other diseases, that several pathogenic mechanisms contribute to pulmonary hypertension, making a concomitant treatment approach necessary.

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**CORRECTIONS**

In the paper by Clarkson and Choonara in the December issue of *ADC* (*Arch Dis Child* 2002;87:462–7) the following corrections have been noted:

Results; first sentence: there were 331 deaths with 390 suspected drugs (not 390 and 389 respectively as stated in the paper).

Results; section “Corticosteroids”: the third sentence starting “No details were available...” should be deleted.

Results; section “Non-steroidal anti-inflammatory drugs (NSAIDs)”: the second sentence “All reports for NSAIDs have occurred since 1990” should be deleted.

Discussion; fifth paragraph: the penultimate sentence should be “as recently as 1999 our study found a single fatality” (not 2 reported fatalities).

The following figure should have appeared with the letter by Desai and Babu in the October issue of *ADC* (*Arch Dis Child* 2002;87:357).

**Figure 1** Scimitar syndrome. Chest x ray showing a curvilinear density which extends from the right hilum towards the right hemi-diaphragm which represents the anomalous pulmonary vein.
Hypothermia in a child secondary to ibuprofen

P R Desai and S Sriskandan

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