Letters to the Editor

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Fludarabine in the treatment of an active phase of a familial haemophagocytic lymphohistiocytosis

Editor,—Familial haemophagocytic lymphohistiocytosis (FHL) is a lethal disease with an uncontrolled activation of T lymphocytes and macrophages due to a perforin gene defect.1 The only current curative treatment is bone marrow transplantation. However, favourable outcome is associated with clinical remission status at the time of the procedure. 2 Unfortunately, the use of steroids, etoposide (VP16), cyclosporin A, and antithymocyte globulins alone or in association frequently fails to control recurrent active phases.

BL, a 2 month old boy, was admitted in June 1999 for an active phase of FHL. His elder brother had died of FHL. The diagnosis was established on clinical (vomiting, fever, pallor, hepatosplenomegaly) and biochemical presentation features (pancytopenia, hypertriglyceridaemia (3.82 mmol/l), haemodilution, hypofibrinaemia (0.65 g/l), a moderate eleva- tion of aspartate transaminases (2N) and hypofibrinogenemia (3.82 mmol/l), haemodilution, hypofibrinaemia (0.65 g/l), a moderate eleva-

Table 1 Patient characteristics

<table>
<thead>
<tr>
<th>Patient</th>
<th>Patient 1</th>
<th>Patient 2</th>
<th>Patient 3</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age at diagnosis (years)</td>
<td>1</td>
<td>1.5</td>
<td>2</td>
</tr>
<tr>
<td>Holiday destination where infected</td>
<td>South of France</td>
<td>Elba</td>
<td>South of Spain</td>
</tr>
<tr>
<td>Interval from exposure to appearance of symptoms (months)</td>
<td>7</td>
<td>12</td>
<td>6–18</td>
</tr>
<tr>
<td>Interval between appearance of symptoms and diagnosis (weeks)</td>
<td>6</td>
<td>3</td>
<td>12</td>
</tr>
<tr>
<td>Hepatomegaly (cm)</td>
<td>6</td>
<td>3</td>
<td>5</td>
</tr>
<tr>
<td>Splenomegaly (cm)</td>
<td>10</td>
<td>3</td>
<td>15</td>
</tr>
<tr>
<td>Hb (mmol/d)</td>
<td>2.9</td>
<td>3.0</td>
<td>4.1</td>
</tr>
<tr>
<td>WBC (× 10⁹/l)</td>
<td>4.5</td>
<td>3.5</td>
<td>1.5</td>
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<tr>
<td>Platelets (× 10⁹/l)</td>
<td>47</td>
<td>107</td>
<td>10</td>
</tr>
<tr>
<td>Ferritin (µg/l)</td>
<td>10813</td>
<td>260</td>
<td>4612</td>
</tr>
<tr>
<td>LDH (U/l)</td>
<td>4779</td>
<td>911</td>
<td>1761</td>
</tr>
<tr>
<td>Triglycerides (mmol/l)</td>
<td>5.64</td>
<td>6.9</td>
<td>3.9</td>
</tr>
<tr>
<td>Serum IgG (g/l)</td>
<td>13.9</td>
<td>15.6</td>
<td>36</td>
</tr>
</tbody>
</table>

LDH = lactate dehydrogenase

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Visceral leishmaniasis: also beware of these deceptive microbes in non-endemic countries!

Editor,—We read with interest the report by Grech et al.1 From their population based study, it seems that the annual incidence of visceral leishmaniasis (VL) declined considerably in Malta as a result of the eradication of stray dogs. VL is still endemic around the Mediterranean Sea and sporadic cases are reported in children living in Northern Europe. It seems likely that with increasing tourism the incidence of VL will also increase in areas where until recently this condition would not even be thought of. During the last 18 months, we have diagnosed three children with VL. As the presentation features can be fairly dramatic and physicians in Northern Europe are not always alert to the possibility of this condition, we would like to call attention again to the possibility of VL in non-endemic countries.

The main clinical features of the patients are shown in table 1. All three children presented with spiking high fevers, anorexia, hepatosplenomegaly, and pancytopenia. The onset of the symptoms was insidious and it took 3–12 weeks to establish the diagnosis. In all three patients this was achieved through bone marrow aspiration and the demonstration of the typical amastigotes in macrophages. The diagnosis was further confirmed through the demonstration of antibodies to the leishmania parasite. All three patients needed erythrocyte transfusions and patient three also needed platelet transfusions. A 5–10 day course of liposomal amphotericin-B was given to all three children. The treatment was well tolerated, and they all became afebrile within a week. Pancytopenia subsided over the ensuing 2–3 weeks and the children gradually returned to normal activity.

 Naturally, we cannot draw epidemiological conclusions from such a small number of patients, but it is intriguing to find three unrelated cases within a relatively short period. While the eradication of stray dogs may go a long way to reduce the incidence of VL, vaccination would be more desirable.2 Although resistance and immunity against the leishmania parasites is not well understood, the seemingly increasing incidence of VL in children travelling from Northern Europe might be because they have no transplacental immunity against the parasite and are therefore more prone to develop this condition than local children. There is much in common between the presentation features of the haemophagocytic syndromes and VL. It is noteworthy that all three of our patients showed signs of macrophage activation and haemophagocytosis was observed in their bone marrow smears. With increased awareness of this condition by physicians in non-endemic countries, the time required to reach the correct diagnosis and institute treatment should be reduced.
I have explored the validity of this assay for use in supernatants of faecal homogenates in children with cystic fibrosis and found it wanting. Recovery of a 500 pg/ml spike of IL-8 progressively increased from 41% in samples which were a 12-fold dilution of faeces to 189% in samples which were a 120 000-fold dilution of faeces, when used according to manufacturer’s instructions. Prediluting the samples 50/50 in newborn calf serum, and using calf serum for further dilutions gave this assay (R&D catalogue no DB8000) mean (SD) spike recovery of 92.1 (12.5%) and coefficients of variation of 3.4% (intra-assay) and 6.85% (interassay). Without knowledge of an IL-8 ELISA, validation data of Smyth et al, I assume that this assay returns similarly spuriously high IL-8 concentrations in polyethylene glycol based whole gut lavage fluid to my 120 000-fold dilution faecal supernatant. The absence of a significant difference between CF patients and controls in their α-antitrypsin output suggests that intestinal inflammation was not present in the CF patients. Overestimation of the WGLF IL-8 concentration would explain the apparently implausibly large volumes of swallowed sputum that the authors estimate would be required to account for their results. In this study which could not turn off the worst cytokine escalator, but did dramatically increase the rate of intestinal transit and exclude exogenous pancreatic enzymes, swallowed sputum is the most likely explanation for the results.

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Intestinal inflammation in cystic fibrosis

EDITOR,—Following their studies of whole gut lavage fluid, Smyth et al have suggested that a non-idiopathic intestinal inflammation occurs constitutively in patients with cystic fibrosis (CF), as a consequence of a proinflammatory effect of the patient’s CFTR mutations.1 They reported marginally elevated excretion of IgG, IgM, interleukin 1 (IL-1), neutrophil elastase, and eosinophil cationic protein, and much more significant increase in excretion of IL-8 and albumin, but no increase in excretion of α-antitrypsin or IgA. In this study where lavage fluid was administered continuously, and intestinal effluent was collected in discrete samples, pooling of the effluent before analysis would have allowed small differences in calculated inflammatory marker outputs to be interpreted as representative of gastrointestinal output. Of all the inflammatory markers presented, only IL-8 shows a range of cytokite outputs in CF patients with or without fibrocystic disease. As with the IL-8 cytokite assay (R&D Minneapolis) protocol.


Intestinal inflammation in cystic fibrosis: an alternative hypothesis

EDITOR,—I was interested by the report of Smyth and colleagues on the finding of markers of intestinal inflammation in whole gut lavage in patients with cystic fibrosis.1 As the α-antitrypsin levels were not elevated when compared to controls, perhaps another hypothesis needs to be considered. Conceivably the inflammatory markers are not increased within the bowel, but rather, they are not degraded due to the lack of intestinal enzymes, α-antitrypsin, which is resistant to proteolytic enzyme activity, would not be affected by such a phenomenon and, therefore, would be the same in patients with cystic fibrosis and controls. Perhaps the authors would need to resort to the somewhat dated technique of radio labelled albumin to definitively answer this question.

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Intestinal inflammation in cystic fibrosis

Dr Grech comments:

The development of visceral leishmaniasis after travel to endemic countries is not a new facet of this problem. At the time of writing, a Medline search using the key words visceral leishmaniasis did not return any articles. At the time of writing, Dr Grech comments:

1 Meinecke CK, Schottelius J, Oskam L, et al. Visceral leishmaniasis (kala-azar) after a visit to the University Medical Centre, POB 7057, 1007 MB, Amsterdam, The Netherlands.


7 SMH POB 7057, 1007 MB, Amsterdam, The Netherlands.

8 1 antitrypsin levels were not elevated concerning the increase in intestinal inflammatory markers in the whole gut lavage of cystic fibrosis patients, which is not found in the controls), this still shows significantly increased IL-8 output in the cystic fibrosis patients (p<0.0001) and unfeasible volumes of sputum would still be required. For these, and reasons detailed in our paper and previous correspondence,1 we do not believe that sputum is the primary cause of the intestinal inflammatory abnormalities found. Our observations concerning the increase in intestinal inflammatory markers in the whole gut lavage of cystic fibrosis patients have now been supported by a study which investigates intestinal inflammation in patients suffering from mucosal biopsy samples.5 This provides additional support to the hypothesis that the basic defect of cystic fibrosis transmembrane regulator could be proinflammatory.6

Dr Eisenberg correctly points out the potential influence of pancreatic enzymes and degradation. The results we found for α1 antitrypsin were unexpected, given differences for albumin and IgG. Some discordance in data has been found previously in whole gut lavage from subjects with active inflammatory bowel disease who are pancreatic sufficient and who also have can raised intestinal permeability.7–9 However, our data that showed raised albumin and IgG is consistent with well established data showing raised intestinal permeability in children with cystic fibrosis.8 As we discussed, it has been found that protein outputs from balloon perfusion experiments (which exclude upper intestinal secretions) are similar to those found in whole gut lavage, which suggests that any potential effect of degradation from pancreatic enzymes is minimal.1 3 We also showed eosinophilic cationic protein to be raised in children with cystic fibrosis. As with α1 antitrypsin, this is relatively stable in faeces at room temperature (approximately 21 % loss over 24 hours).10 This loss would be considerably lower during whole gut lavage. Thus, degradation would be unlikely to explain this difference.

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Letters to the editor

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Lumbar puncture should not be performed in meningococcal disease

Editor,—I was dismayed to see your publication of the letter by Dr Sam regarding the role of lumbar puncture in meningococcal disease. While fully understanding the need to get as much information as possible, the benefits of isolating the causative organism need to be weighed against the risk of causing clinical deterioration in a patient who may have case-massive compromise and increased intracranial pressure, both of which are recognised in meningococcal infection. It is generally accepted that throat swabs should not be taken from children with epiglottitis until the child’s airway has been protected, because of the risk of clinical deterioration. It is time that textbooks of emergency paediatrics stated clearly that lumbar punctures on children with a haemorrhagic rash, and clinical signs of meningeal inflammation, should not be carried out until the clinical condition has been stabilised, and only if the procedure will add further valuable information that cannot be obtained elsewhere.

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Table 1 Potential cost of prophylaxis in the community

<table>
<thead>
<tr>
<th>Year</th>
<th>£1000s</th>
<th>£1000s</th>
</tr>
</thead>
<tbody>
<tr>
<td>1999–2000</td>
<td>19.2</td>
<td>1.3</td>
</tr>
<tr>
<td>2000</td>
<td>9.6</td>
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Prophylaxis for respiratory syncytial virus infection: missing the target

Editor,—Two recent reports about hospitalisation for respiratory syncytial virus (RSV) infection in high risk infants1,2 have suggested that the introduction of prophylaxis may, potentially, be beneficial in certain subgroups. We would like to emphasise that the “bigger picture” also warrants further consideration.

During the winters of 1998–1999 and 1999–2000, we recorded our admissions who were RSV positive and had a Cambridge “CB” post code. “At risk” infants—that is, pre-termers under 6 months of age, or those with bronchopulmonary dysplasia (BPD) under two years, were identified from the records of the maternity and neonatal units serving our postal region. The total cost for admission was calculated using length of stay on the ward (£255 (approx £380)) and in the intensive care unit (bed day cost of £1136 (£approx £1700)). The potential cost of prophylaxis in the community was also estimated (see table).

Table 1 shows the potential cost of prophylaxis for our study population. The RSV related admission rate (95% CI) from our under 6 month old population was in the range of 19–41 per 1000 (denominator estimated from the numbers of births with a CB post code; personal communication with A Sneeden, Office for National Statistics, London). In the pre-term infants who were under 6 months the proportion admitted during the two winters (1998–1999 and 1999–2000) was 5/51 (9.8%, 95% CI 3.3 to 21.4%) and 4/62 (6.5%, 1.8 to 15.7%) respectively. Supposedly “low risk” infants accounted for 92% (66/72) and 90% (54/60) of our RSV related admissions for each winter. There were no deaths in any of the admissions including the two with BPD.

In the first winter, 10 intensive care bed days were needed, none in the “high risk” population. In the second winter, such infants used 12 out of 54 intensive care bed days. Finally, inpatient costs for RSV in “high risk” infants was about 10% and 15% of total RSV related hospital costs for the two winters respectively (see table).

Taken together, even if there were potential savings following the introduction of prophylaxis to specific subgroups, a target population—arguably equally in need of protection—is being overlooked. In fact, in our area, the potential effect of introducing prophylaxis would more likely double health authority costs for RSV, with little impact on our so called “low risk” more major caseload.

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Hajj and risk of blood borne infections

Editor,—Annually, some two and a half million pilgrims congregate in the city of Mecca in Saudi Arabia to perform the Hajj (pilgrimage), a religious duty for all adult Muslims who are physically and financially able. Because of the very large numbers of peoples from disparate regions, and the hostile climate of the Arabian Desert, the chances of disease are high. Heat exhaustion, sunstroke, and infectious diseases such as pneumonitis and meningitis have traditionally caused the greatest disease burden.

One of the rites of the Hajj is for males to shave their heads, although trimming the hair is also acceptable. Most will choose the former, often in makeshift centres run by opportunistic barbers. A razor blade is commonly used, and may be used on several scalps before ultimately being discarded. The risks of blood borne infections such as hepatitis and hepatitis B and C are obvious, especially considering that many pilgrims come from regions of the world where such infections are endemic. Pilgrims should be aware of the potential dangers and be educated to insist on the use of a new blade. We would also strongly recommend that they be vaccinated against hepatitis B.

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